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ANCIENT BRITAIN

AND

THE INVASIONS OF JULIUS CAESAR

BY

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'There seems no human thought so primitive as to have lost its bearing on our own thought, nor so ancient as to have broken its connection with our own life'.—E. B. Tylor.

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PREFACE

THIS book is in one sense a companion of my *Caesar's Conquest of Gaul*; and much that was written in the preface of that volume is equally applicable here. The last three chapters of Part I, and the later articles in Part II, are intended to do for Britain what I formerly tried to do for Gaul; but whereas the main object was then to illustrate the conquest, and the opening chapter was merely introductory, my aim in these pages has been to tell the story of man's life in our island from the earliest times in detail. What has been called 'prehistory' cannot be written without knowledge of archaeology; but from the historical standpoint archaeological details must be handled, not for their own sake, but only in so far as they illustrate the development of culture. The two books are constructed on the same principle: in this, as in the other, the second part is devoted to questions which could not properly be discussed in narrative or quasi-narrative chapters, though I am encouraged by the judgement of expert critics, British, American, and Continental, of *Caesar's Conquest of Gaul*, to hope that general readers who are interested in these matters may not find the articles which deal with them tedious. Those on Stonehenge, Ictis, and the ethnology of Britain, although they controvert certain opinions which are commonly accepted, will, I hope, tend to place facts in their true light. Two articles deal with well-worn themes,—the identity of the Portus Itius, and the place of Caesar's landing in Britain. These problems have been pronounced by eminent scholars,

including Mommsen, to be insoluble ; nevertheless, I venture to affirm that in both cases the inquiry has now been worked out to demonstration. Critics who may be disposed to regard this claim as arrogant or frivolous will, I trust, read the articles through before passing judgement upon them. The questions would have been settled long ago if any competent writer had bestowed upon them as much care as has been expended in investigating Hannibal's passage over the Alps.

Books and articles on various branches of the study of ancient Britain are practically innumerable ; no other book, intended to treat it comprehensively from the beginning to the Roman invasion of A.D. 43, has, so far as I know, yet appeared.

I wish to express my gratitude to all who have in any way helped me. I am indebted to Sir John Evans for figures 1–6, 8–11, 14, 15, and 18–29, as well as for an opinion, most kindly given, in regard to certain coins which are not mentioned in his *Coins of the Ancient Britons* ; to the Director of the British Museum for figures 30, 36–9, 41, 43, and 44 ; to the Society of Antiquaries for figures 7, 13, 16, 31, 35, and 40 ; to the Delegates of the Oxford University Press for figures 12 and 32–4 ; to Dr. Joseph Anderson for figure 17 ; and to Canon Greenwell for a proof of a valuable and interesting article—' Early Iron Age Burials in Yorkshire '—which, I believe, is to appear in *Archaeologia*. Captain Tizard, R.N., F.R.S., kindly answered various questions which I asked him about tidal currents. Mr. E. J. Webb, Sir George Darwin, Professor Postgate, Professor Haverfield, Mr. Clement Reid, F.R.S., Mr. George Barrow, F.G.S., Captain J. Iron, Commander Richmond, R.N., and Commander Boxer, R.N., gave me information, which, in every instance, will be found acknowledged either in footnotes of Part I, or in Part II, on various points of detail.

It is vain to plead that work would have been better if circumstances had been more favourable. But if any indulgence may be accorded to an author who, except on holidays, can only find leisure for writing or research after he has fulfilled the duties of an exacting profession, and who, in order to gain time, has worked steadily throughout his vacations for nearly thirty years, I am entitled to it.

11 Douro Place, Kensington, W.
October 19, 1907.

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[The maps of South-Eastern Britain and East Kent, like all maps of Ancient Britain, are inevitably inexact; but the errors are unimportant. The Dover cliffs, for instance, have lost by erosion, but one cannot say how much (see pages 528–30); nor is it possible to indicate the exact nature of the slight change which the coast has undergone between Sandown Castle and Walmer Castle (pages 521–5). Again, I have not attempted to delineate the coast west of Pevensey or west or north of Reculver precisely as it was in 55 B.C., because, even if such an attempt had been successful, nothing would have been gained for the purpose of this book. As far as possible, however, the maps represent the conclusions reached in the article on the configuration of the coast of Kent in the time of Caesar. The outline of Richborough harbour and of the estuary between Thanet and the mainland is intended to show approximately the high-water mark of spring tides. At low tide the channel was very narrow (page 519).]

ANCIENT BRITAIN

AND

THE INVASIONS OF JULIUS CAESAR

CHAPTER I

INTRODUCTION

WHEN Caesar was about to sail on his first expedition to Britain, he summoned the Gallic traders whose vessels used to ply between Gaul and the Kentish coast, and tried to elicit from them information; but, to quote his own words, 'he could not find out either the extent of the island, or what tribes dwelt therein, or their size, or their method of fighting, or their manners and customs, or what harbours were capable of accommodating a large flotilla.' Even after he had seen the country and its inhabitants with his observant eyes he was not much better informed: all that he could learn about the aborigines he summed up in a single sentence; and later writers, Greek, Italian, and mediaeval—Strabo, Diodorus Siculus, Augustus Caesar, Pomponius Mela, Tacitus, Suetonius, Dion Cassius, Herodian, and the rest—added very little to the knowledge which he had gathered. Yet the materials which are now available for a description of prehistoric and pre-Roman Britain, however limited their range, are so abundant that the difficulty is to use them with discrimination and to fashion the essential into a work of art. How have these materials been obtained? When the general reader takes up a history, he accepts the narrative in a spirit more or less sceptical. He knows that it has been composed, either directly or at

second hand, from written, perhaps also from oral testimony; and he rarely troubles himself to inquire what the evidence is, or with what diligence and acuteness it has been sifted. But when he is invited to read an account of the evolution of culture among people who recorded nothing and of whom nothing was recorded, it is natural that he should insist upon peering into the writer's workshop that he may judge for himself what the materials are worth.

During many centuries, while the materials were most abundant, they remained unused. Many of them were rifled by treasure-seekers, carted away by builders, or destroyed by the plough. Even when the Renaissance turned men's minds to the study of the past, they had no thought of any sources of information except the written documents which they were only beginning to learn how to use. The Italian scholar, Raymond de Marliano, the Dutch geographer, Abraham Ortels, made futile guesses about topographical questions suggested by Caesar's *Commentaries*, but never dreamed that there was anything to be learned of a people who had lived in Britain when the South Foreland and Cape Grisnez were still undivided. Camden travelled over the length and breadth of England, amassing stores of information, much of which he did not know how to interpret, and built up geographical theories upon place-names, which, in default of linguistic science, were of necessity worthless. Even the great French scholars of the seventeenth and eighteenth centuries—Chifflet, Du Fresne, Scaliger, Sanson, and d'Anville—although their geographical essays are still worth reading, failed to determine the port from which Caesar had sailed to Britain. Stukeley, who was one of the first to excavate barrows and describe their contents and who made valuable observations of some of our megalithic monuments, encumbered his folios with fanciful speculations which only served to entertain his contemporaries and to mislead posterity.[1] But

[1] Dr. Joseph Anderson (*Scotland in Pagan Times,—the Bronze and Stone Ages*, 1886, p. 135) has pointed out that the subject of stone circles was first treated in a scientific spirit in 1692—long before Stukeley wrote—by Prof. Garden of Aberdeen (*Archaeologia*, i, 1770, pp. 312-9).

these men had no access to the sources which are now open to many who are intellectually their inferiors; and, notwithstanding the smallness of their achievement, they did their work as pioneers.

About the middle of the eighteenth century a spirit of antiquarian curiosity was aroused in England. The Society of Antiquaries, which had been founded in 1717, received in 1752 a charter from George the Second; and in 1770 appeared the first number of their principal organ, *Archaeologia*, which is still in course of publication. Many of the earlier papers were crude and superficial, showing keen interest in the things of the past, but naturally betraying ignorance of the methods by which alone the significance of antiquarian discoveries could be ascertained. Early in the nineteenth century, however, Sir Richard Colt Hoare and his friend, William Cunnington, began to excavate the barrows of Wiltshire; and with their labours the era of scientific investigation may be said to have begun. Hoare had in earlier life been an ardent fox-hunter; but, as he grew older, he found that barrow-digging was a pastime more exciting still. Craniology was at that time unborn; and Hoare omitted to measure the numerous skeletons which he discovered or to utilize them for the advancement of ethnology. Even the work that he professed to do was often marred by a lack of thoroughness which, although it was inevitable in a pioneer, irritated the critical spirit of later explorers.[1] But with all its limitations the *Ancient History of North and South Wiltshire*, the first volume of which appeared in 1812, was an important work. A few years earlier, John Frere had recorded in *Archaeologia*[2] the discoveries of stone implements which he had made at Hoxne in Suffolk. Such discoveries had of course in innumerable instances passed unrecorded. In the British Isles, as in many other lands, flint arrowheads were regarded by the peasants who found them as fairy-darts; while stone axes, which in Scotland, Ireland,

[1] See A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 1898, p. 18 (preface), and cf. *Archaeol. Cambr.*, 6th ser., vi, 1906, p. 72.

[2] xiii, 1800, pp. 204–5.

and Cornwall, are still deemed to possess medical virtues, were said to have fallen from the sky.[1] In the time of Charles the Second, however, Sir Robert Sibbald, greatly daring, affirmed that the fairy-darts had been made by man;[2] and nearly a century before the time of Frere an implement, which has since been assigned to the Palaeolithic Age, had been found near Gray's Inn Lane, and had been vaguely described as 'a British weapon'. But Frere saw that the tools which he had collected were not to be ascribed even to the 'painted savages' who had resisted the invasion of Caesar; and although even he did not suspect their immeasurable antiquity, he declared that they must have belonged to 'a very remote period indeed' and to 'a people who had not the use of metals'. In 1824 Dr. Buckland, who had spent some years in exploring ossiferous caves, published an account of his work in *Reliquiae Diluvianae*, a book which, by attributing the phenomena that it recorded to an universal deluge, impelled geological research in a wrong direction, and delayed for many years the recognition of the truth that the earlier human occupants of the caves had been contemporary with the mammoth and other extinct animals. Soon afterwards MacEnery, whose example was followed by Godwin Austen, examined Kent's Cavern near Torquay, a task which was systematically completed some five-and-twenty years ago by a committee of the British Association. It was not, however, before the middle of the nineteenth century that the knowledge of the Stone Ages began to be built up on a sound foundation. From 1841 to 1860 Boucher de Perthes was patiently exploring in the neighbourhood of Abbeville and Amiens the gravels which the river Somme had deposited in the Pleistocene Period, and collecting flints which were proved to have been shaped by the hands of man. Lyell, Prestwich, John Evans, Lubbock, and Flower visited the scene of his labours, and testified to the authen-

[1] Sir A. Mitchell, *The Past in the Present*, 1880, pp. 155–7; Sir J. Evans, *Ancient Stone Implements . . . of Great Britain*, 2nd ed., 1897, pp. 56–61, 65, 362–8; *Rev. arch.*, 4e sér. 1, vii, 1906, pp. 239–59.

[2] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 363.

ticity of his discoveries ; and after long controversy the most reluctant were forced to admit that the human race had existed at a period infinitely more remote than had hitherto been imagined. Similar discoveries were soon made in England, in various European countries, in Africa, Asia, and America. In our islands, as well as on the Continent, as antiquarian zeal became more widely diffused, the need of organized effort was felt ; and, side by side with the leading academies—the Society of Antiquaries, the Society of Antiquaries of Scotland, the Royal Archaeological Institute of Great Britain and Ireland, the Royal Irish Academy, the Royal Society of Antiquaries of Ireland, and the Cambrian Archaeological Association—local societies were gradually formed in every important provincial town. Accident from time to time revealed objects for which no search had been made. Ploughmen guiding their teams, navvies working upon roads or in railway-cuttings, miners and quarrymen, labourers draining land, sportsmen groping after game which they had shot, came upon antiquities of the nature of which they were ignorant. Evans, in the intervals of leisure which he could win from a busy life, indefatigably collected implements of stone, bone, and bronze, systematized the discoveries of a host of minor workers, and marshalled facts and deductions in volumes which have become classical ; and, not content with this, he supplemented the labours of Akerman, Hawkins, Roach Smith, and others, and revealed to his countrymen the origin, the varieties, and the geographical distribution of the coins which their British ancestors had minted, and the historical value of which he was the first to emphasize. His son, who has lately become famous as the explorer of Crete, carried his researches further afield, but often found time to grapple with British problems ; contributed to our knowledge of Stonehenge and other megalithic circles ; and by his discoveries at Aylesford in Kent threw a beam of light upon the history of the Celtic Iron Age. Boyd Dawkins explored the caves of Somersetshire, Derbyshire, and Wales. Bateman, Thurnam, Davis, Warne, Greenwell, Mortimer, and Atkinson of Danby continued in a more

scientific spirit the labours of Hoare,[1] and recorded the discoveries which they had made in numerous barrows. General Pitt-Rivers brought the experience of a soldier, the sagacity of a man of the world, and the genius which was his own to the investigation of archaeological and anthropological problems; demonstrated the value of thorough excavation [2] and of accurate pictorial illustration; impressed upon the rising school of students the need of precision in recording the circumstances of every find; and by expending a considerable fortune in adding to knowledge set an example of enlightened generosity. Sir Arthur Mitchell, in a series of lectures [3] which have been described as a masterpiece of sceptical irony, warned antiquaries, but in no didactic spirit, to think, and to think again, before they drew conclusions from the records which the spade had revealed. The Devonshire Association appointed committees to examine the antiquities of their richly dowered county, and printed a series of reports upon the megalithic monuments, the graves, and the 'hut-circles' of Dartmoor. John Abercromby traced from Great Britain to the original seat of manufacture the sites where the so-called drinking-cups, which accompanied so many British interments of the earlier round barrows, have been found; while Romilly Allen, following in the steps of Wollaston Franks, helped to elucidate the development of the art of the Bronze Age and the Late Celtic Period. Professor Gowland disclosed by excavation the origins of Stonehenge,

[1] None of these excavators, however, was so thorough as Pitt-Rivers; and Bateman was often careless (see H. St. G. Gray's *Index to 'Excavations in Cranborne Chase'*, 1905, p. xvi). But it must be remembered that to do such work properly, not only skill and perseverance are needed, but also money.

[2] Pitt-Rivers remarks (*Excavations in Cranborne Chase*, iii, 1892, pp. x, 254) that a rampart almost always yields something, for example, pottery, which throws light on the period of its construction; while his experience shows the importance of 'digging the whole of a camp over, down to the undisturbed soil' (*ib.*, iv, 14 [preface]). He tells us (*ib.*, p. 4) that when he was excavating the earthwork called South Lodge Camp, 'in the first three sections little or nothing was found, which shows what very false conceptions are liable to be formed by merely digging one or two sections in a camp.' See also vol. iii, pp. xi, 13; vol. iv, pp. 46-8, 138, 144, 187; *Trans. Epping Forest... Naturalists' Field Club*, ii, 1882, pp. 59-60; and *Report of... the Brit. Association*, 1904, pp. 691-700.

[3] *The Past in the Present*, 1880.

and by his metallurgical knowledge enabled us to understand the methods of prehistoric miners. Charles Read made intelligible, even to casual visitors, the collection of antiquities in the British Museum which illustrates the culture of the Ages of Stone, Bronze, and Iron. Francis Haverfield, scholar, archaeologist, and practical excavator, while making himself the foremost authority on the history of Roman Britain, incidentally enlarged the records of pre-Roman times. Joseph Anderson carried on the work which Daniel Wilson had begun, and described the successive stages of culture through which the inhabitants of Scotland had passed from the earliest to the beginning of the historic period. Coles, Christison, and Bryce added significant details to the information which his lectures had given. But it would be tedious to prolong the list of workers. Everywhere the success with which the last resting-places of the dead had been made to tell their tale stimulated antiquaries to search for fresh relics that might help them to realize more fully how those dead had lived. Flint quarries and workshops, where primitive tools were fabricated, hut-circles, Scottish brochs, lake-dwellings, pits, and 'earth-houses' were explored; and, in response to the exhortations of Pitt-Rivers, camps and other earthworks were patiently excavated, although, for lack of funds, research of this kind has not progressed very far. The exploration of the far-famed marsh-village at Glastonbury is nearly complete; and the results which have been obtained, collated with those that were yielded by the examination of the camps of Cissbury, Lewes, Hod Hill, and Hunsbury, have done much to dispel the old fancy that the ancient Briton was a savage.

But perhaps no intelligent man ever progressed far in archaeological study without discovering for himself this caution:—though the relics of man's handiwork, unlike his written history, cannot lie, their meaning may in divers ways be misinterpreted. They will not yield it up except to the trained and discerning eye.[1]

[1] The following passage from Sir John Evans's *Ancient Bronze Implements . . . of Great Britain and Ireland*, 1880, pp. 25–6, is instructive:—'In company

Meanwhile toilers in other fields were co-operating with the archaeologists. Physical anthropology began to make strides. Since Davis, Thurnam, and Rolleston described the skeletons which had reposed in the long barrows and the round barrows of Wiltshire, Gloucestershire, Yorkshire, and Northumberland, since Huxley wrote his memoirs on the river-bed skulls of England and Ireland, greater accuracy of method has been evolved, and Beddoe, Turner, Garson, and Haddon have supplemented and corrected their predecessors' work. Geologists endeavoured to determine the configuration of the land at the time when man first lived in Britain; and a definite result was attained when borings made in implement-bearing beds showed the relative chronology of the period during which palaeolithic hunters had inhabited the eastern counties. Burial customs revealed by the opening of barrows and cists, holes drilled in the stones of dolmens, strange devices sculptured on graves and on rocks, suggested problems as to the religious ideas of the Neolithic and Bronze Ages, which the archaeologist, the ethnographer, and the folklorist attempted to solve. Philologists studied the Celtic languages, and succeeded in some measure in deducing from place-names and other relics of the ancient dialects information bearing upon the history of the invasions and the distribution of the two great branches of the Celtic stock.

A great advance was made when the Comparative Method was brought to bear upon the study of primitive culture. It was recognized that the antiquities of our own island

with Sir John Lubbock I was engaged in opening a grave [at Hallstatt] in which we had come to an interment of the Early Iron Age, accompanied by a socketed celt and spear-heads of iron, when amidst the bones I caught sight of a thin metallic disc of a yellowish colour which looked like a coin. Up to that time no coin had ever been found in any one of the many hundred graves which had been examined, and I eagerly picked up this disc. It proved to be a "sechser", or six-kreutzer piece, with the date 1826, which by some means had worked its way down among the crevices in the stony ground.... Had this coin been of Roman date it might have afforded an argument for bringing down the date of the Hallstatt cemetery some centuries in the chronological scale. As it is, it affords a wholesome caution against drawing important inferences from the mere collocation of objects when there is any possibility of the apparent association being only due to accident.'

could not be adequately comprehended without reference to those of other lands. For at every turn the inquirer found himself arrested by obstinate questionings. Whence had the immigrants of the Old and the New Stone Age, the Bronze Age, and the Early Iron Age set out? Whence was the knowledge of bronze derived? What was the starting-point of the culture of the Iron Age? What were the first beginnings of Late Celtic Art? How was one to account for the existence in remote countries of this or that British custom? The British archaeologist who would intelligently ponder these questions must take account of the work which has been done by Cartailhac, the brothers Siret, Bertrand, Edouard Piette, Salomon Reinach, Montelius, Sophus Müller, Arthur Evans, Ridgeway, Myres, and Flinders Petrie in elucidating the antiquities of France, Spain, Italy, Central Europe, Scandinavia, the Aegean Sea, North Africa, and Egypt; and the British ethnologist cannot afford to be ignorant of what Broca, Hamy, de Quatrefages, Salmon, Hervé, Manouvrier, Virchow, Ranke, and Sergi have done for the ethnology of Europe. Pitt-Rivers saw that ethnography, which informs us about the arts and crafts, the manners and customs of surviving savage tribes, can give archaeology indispensable aid;[1] and all who have compared the contents of the American Room and the Ethnographical Gallery in the British Museum with what they have seen in the Prehistoric Room will believe the Keeper when he assures them that 'in all probability the resemblance between the perishable productions of the modern savage and those of prehistoric man, which are now lost, was as great as that which undoubtedly exists in the case of implements of stone and bone which have remained':[2] but in endeavouring to apply their knowledge to the elucidation of the antiquities of a particular country they will not forget to be on their guard. Nor may we neglect the facts which folk-lore

[1] See Mr. H. Balfour's interesting introduction to Pitt-Rivers's *Evolution of Culture*, 1906, p. xiv.

[2] *Guide to the Antiquities of the Stone Age* (Brit. Museum), 1902, p. 76. See also *Journ. Anthr. Inst.*, xxxiii, 1903, p. 18.

societies have in late years so diligently collected; but those who have learned from the great works of Tylor how much of primitive custom still lingers in the depths of modern civilization will become sceptical when they are invited by less sober reasoners to trace the origin of this or that surviving superstition to any one race or tribe or period of the remote past; and readers who have accepted with enthusiastic admiration the seductive theories of *The Golden Bough* should weigh well the criticism which Sir Alfred Lyall, qualified by intimacy with primitive peoples as well as by a sceptical and cultivated intellect, has published of that brilliant and truly epoch-making book.

When we have finished our survey of prehistoric times we shall find that while we can still rely upon the aid of the archaeologist and the anthropologist, other materials have been accumulating which will enable us to read our classical texts with an insight that was impossible for the old-fashioned historian. The texts themselves have been purified and restored. Inscriptions have yielded new information on matters of history, ethnology, and religion; and the vast labour which has been expended by those who have striven to elucidate the most interesting of all subjects cannot wholly fail to help us when we inquire what the British Celts thought of man's relation to the universe. As one scholar after another has noted the significance of dates recorded in Cicero's correspondence, and compared them with the relevant passages in the *Commentaries* and other ancient writings, chronological difficulties have gradually disappeared. Physical geography and geology, supported partly by written documents, partly by archaeological discoveries, have combined to reconstruct the map of the coast on which Caesar landed. Astronomers and hydrographers have perfected our knowledge of tidal streams, and thereby forged a key which, for those who possess the indispensable knowledge of seamanship and of ancient military history, can unlock the secrets of Caesar's voyages. Military experts and soldiers who have served in the field are willing to help us to understand the story of his campaigns.

But after the student has digested all the information which he can extract from books and manuscripts, from museums, from travel and observation, perhaps from practical experience in digging, and, above all, from those who combine learning with knowledge of the world, of affairs, and of men, he will find that his materials are still, and on certain points must always remain inadequate. Some branches of research, indeed, are virtually complete. All, or nearly all, that sepulchres and skulls and coins can teach us of Ancient Britain and its inhabitants we know. Many more implements, weapons, ornaments, and urns will be accumulated; but it may be doubted whether they will add sensibly to that knowledge which is really worth having. But much still remains to be learned. The geological record is still incomplete; and one of our most accomplished field-geologists is hopefully looking forward to a time when it may be possible to determine the uttermost antiquity of man and to illuminate the dark era that intervened between the Pleistocene Period and the apparent commencement of the Neolithic Age.[1] His experience has enabled him to tell archaeologists that in order to solve chronological problems, they cannot afford to neglect even the shells which abound in many burial-mounds.[2] There is room also for many labourers in excavating stone circles, camps, and earthworks, and determining their age, in exploring habitations, wherever they can be found, and learning what they can teach about those who constructed them.[3] What has been already done in this department has produced the most fruitful results: the speculations of Dr. Guest, for instance, in regard to the so-called 'Belgic ditches', have been stultified by pick and shovel.[4] But such work, which in other civilized countries is an object of national concern, languishes here for want of funds. No British Government can expect support from the intelligence and the public spirit of its constituents in spend-

[1] *Vict. Hist. of . . . Sussex*, i, 1905, p. 22.
[2] *Proc. Dorset Nat. Hist. and Antiq. Field Club*, xxi, 1900, p. 75.
[3] See A. Pitt-Rivers, *Excavations in Cranborne Chase*, iii, p. xii; iv, p. 28.
[4] *Ib.*, iii, p. xiii.

ing money upon archaeological research, or has the courage to give them a lead ;[1] and where are the wealthy Englishmen who will follow the example of their American cousins in endowing such work ?

Nevertheless, enough is already known to justify an attempt to create a synthetical work, the aim of which shall be to portray in each successive stage and to trace the evolution of the culture—nay, in some sort even to construct a history—of prehistoric Britain, and to rewrite the history of the period which is illustrated by contemporary records. Not only is the subject fascinating; it is an indispensable introduction to the history of England. I have tried to bear ever in mind the interdependence of all the sciences which can help to restore the past, and to remember the warning, 'Let him that thinketh he standeth take heed lest he fall.' It is easy to laugh at the guesses of Camden and the theories of Stukeley ; but they were only framing the hypotheses which are as necessary for the progress of archaeology as of other sciences ; and certain theories which in our own day have been acclaimed with enthusiasm, while serving their purpose like theirs, will, like theirs, be found open to criticism.

But we need not exercise ourselves overmuch in the region of theory. Though we must be content to remain ignorant of many things, the story of Ancient Britain, gaining as it progresses firmness of outline and fullness of detail, can be constructed upon a basis of fact.

[1] Mr. N. W. Thomas (*Man*, v, 1905, No. 25, pp. 47–8) points out that 'some five years ago the Berlin collections [of ethnographical objects] from British possessions were seven times as large as those in our national museum, and since then this disproportion has not been decreased', the reason being that 'the men whom the nation pays to perform certain duties [in national expeditions] are permitted to retain the objects collected in the performance of those duties'. But the nation is to blame as much as the Government.

CHAPTER II

THE PALAEOLITHIC AGE

A CHAPTER devoted to Palaeolithic Man may perhaps appear irrelevant to a work the aim of which is to serve as an introduction to English history; for it has been questioned whether in this country he left any descendants, and therefore whether he exercised even the smallest influence upon the later immigrants. But in France, if not here, the Palaeolithic merged, perhaps by a long period of transition, into the Neolithic Age:[1] the neolithic inhabitants of Britain were of course descended from palaeolithic ancestors; and in every part of the world in which it existed the palaeolithic culture was apparently much the same. There are therefore other reasons besides that of sentiment for attempting in this book to describe the life of primitive men and the surroundings in which they lived: yet sentiment has its weight; for no one who is not heedless of the past would forget the efforts of those who, in hard struggle with nature and with fierce beasts, were the unconscious founders of European civilization. Without the faith of the Shinto ancestor-worshipper one may share his daily repeated pious gratitude,—'Ye forefathers of the generations, and of our families, and of our kindred, unto you, the founders of our homes, we utter the gladness of our thanks.'[2]

Reasons for devoting a chapter to the Palaeolithic Age.

The palaeolithic people had acquired a degree of skill in the manufacture of stone tools which is only attainable by the most practised modern imitators. But the progress which they made during the incalculably long period of their existence was so small that they must have needed ages to ascend to the level at which we are able to observe them. Therefore, although no skeletons, no implements have yet been found which can be referred, in the opinion of all

Tertiary man.

[1] See pp. 61, 385–90, *infra*.

[2] Lafcadio Hearn, *Kokoro*, p. 290.

experts, to the Tertiary Period, the most sceptical are willing to believe that man, even if he did not deserve the appellation of *Homo sapiens*, did then wander upon the face of the earth.[2] But how, when he had assumed the erect position and had begun to make intelligent use of the hands which gave him such an advantage in contending with other carnivorous animals more powerful than himself, he learned slowly and by repeated efforts to chip the flints that he picked up into serviceable shapes; how in the struggle for a livelihood the stronger or the more cunning prevailed; how with developing intelligence came keener susceptibility to pain as well as to pleasure; how men's fancies were quickened by light and darkness, sun, moon, and stars, and their fears excited by storm and flood and fire; how they strove to communicate to each other their alarms, their desires, and their joys—these things may only be imagined; and the imagination of those who have read most wisely and have most observantly studied the ways of modern savages will lead them least astray.

The Ice Age.

The Tertiary was merging into the Quaternary or Pleistocene Period when the climate which had before fostered the palms and crocodiles whose fossils have been discovered in the London Clay,[2] but had been gradually changing, became intensely cold. Snow fell thickly upon the mountains of Scandinavia; glaciers began to creep down the valleys; and gradually the ice accumulated until it overspread the whole of Northern Europe, filled the basins of the Baltic and the North Sea, hid mountains and uplands in Scotland, and choked the dales of Northern England, of the Midlands, and of Wales; while isolated glaciers were formed even so far southward as the valleys of the Beaujolais and the Lyonnais. The ice has left its record upon the Highland and Cumbrian mountains, whose rugged crags it moulded into flowing curves; upon rocks which were scratched by stones embedded in slowly moving glaciers;

[1] 'Comme paléontologiste,' says M. Marcellin Boule, 'je crois fermement à l'existence de l'Homme tertiaire: je ne doute pas qu'on trouvera un jour ses traces' (*L'Anthropologie*, xvi, 1905, p. 267).

[2] Sir A. Geikie, *Text-book of Geology*, 4th ed., ii, 1903, pp. 1224–5, 1231.

in the mud, stiff and tenacious, which they deposited as they grided over many kinds of rocks, and which, being interspersed with stones, large and small, is called boulder-clay; in rocks which they transported and dropped far from their native sites, and by which the directions that they followed can still be traced; in moraines which mark the limits of their descent and their recession; in lakes that were formed, after the ice had disappeared, in glens which moraines had dammed;[1] in the Arctic plants which survive on mountains, and in those whose fossil remains have been found in Norfolk near the level of the sea. In many places the boulder-clay lies in two or more layers, separated by stratified sands and gravels, from which it has been generally inferred that the Ice Age was interrupted by a period—here and there by short intervals—during which the climate was mild. Told briefly and in general terms, the tale which a learner might piece together from geological textbooks[2] is something like this. The cold was most intense during the earlier stage, when the lower boulder-clay was being deposited, and, little by little, Britain rose until it became one with the Continent, with Ireland, and with Scandinavia, and extended far westward into the Atlantic Ocean. Then, we are told, the ice-sheet that covered Scandinavia was

[1] Sir A. Ramsay's theory (*Physical Geol. and Geogr. of Great Britain*, 6th ed., 1894, p. 269), that the basins of the Scottish and Cumbrian lakes were scooped out of the rocks by glaciers, was held by no British geologist a few years ago, except in a modified form. See A. J. Jukes-Browne, *Student's Handbook of Phys. Geol.*, 2nd ed., 1892, pp. 159, 624, 629–30; T. G. Bonney, *Ice-Work, Present and Past*, 1896, pp. 80–94; and Sir A. Geikie, *Text-book of Geol.*, 1903, i, 552; ii, 1323–4, 1385–6. Prof. W. M. Davis of Harvard has, however, recently produced fresh evidence 'in favour of the excavating power of glaciers' (*Trans. Roy. Soc., Edinburgh*, xl, part ii, 1902, p. 457); and Ramsay's theory is 'in no wise extinct' (*Quart. Journ. Geol. Soc.*, lxii, 1906, p. 166); but Prof. E. J. Garwood has recently investigated the Alpine lakes near Airolo, and holds (*ib.*, p. 190) that, with a few possible exceptions, they 'do not seem to be due to ice-erosion'.

[2] For instance, H. B. Woodward, *Geol. of England and Wales*, 2nd ed., 1887, pp. 475–512; J. Prestwich, *Geology*, ii, 1888, pp. 453–4, 469; A. J. Jukes-Browne, *The Building of the Brit. Isles*, 1888, pp. 281, 289, 294–6; Sir A. C. Ramsay, *Phys. Geol. and Geogr. of Great Britain*, 6th ed., 1894, pp. 229, 238, 242–3, 246–8, 252, 259, 263, 276; T. G. Bonney, *Ice-Work, Present and Past*, 1896, pp. 121, 277; and Sir A. Geikie, *Text-book of Geology*, 1903, i, 169; ii, 1302–32.

six thousand feet thick; and though it became thinner as it advanced southward, it shrouded the hill-tops in Scotland, where boulders were lifted right over the water-parting, and dropped on the western side, and scored its marks upon rocks in the Lake District at heights of two thousand five hundred feet; while, spreading over Ireland, it went out to sea beyond Cork and Kerry, where the wall of ice broke off and floated away in bergs. Then the land slowly sank until in the interglacial period only the hills stood out above the sea, and Great Britain became an archipelago. Again the movement was upward, though often interrupted and perhaps not general in extent: the climate was again becoming severe; and, although the rigours of the first period were not repeated, local glaciers crept down the higher valleys north of the Midlands, while icebergs floated over the parts that remained submerged and over the North Sea. Now too, as in the earlier period, the cold was not everywhere continuous: there were oscillations during which the glaciers alternately advanced and retreated. As the Ice Age was beginning to near its end, the land continued to rise until the North Sea, the English Channel, and the Irish Sea once more disappeared. In the latest stage of all, when Arctic conditions were about to vanish even in our northern latitudes, there was a gradual subsidence: Scotland was lowered about one hundred feet beneath the present level of the sea, as the highest 'raised beach' along the shores of the great estuaries testifies; and the waters rushed in over the sinking valley of the Dover Strait.

Such was the orthodox faith: but the rising geologists have discarded some of its articles; and even among the faithful there are pious doubters. Many authorities deny that the sea-shells which are found on hills in North Wales, Cheshire, and elsewhere, prove that they were once submerged: those shells, they insist, were ploughed up by glaciers out of the sea-floor; and they require us to believe that they were carried up the sides of the hills to heights of thirteen hundred and fifty feet above the sea-level.[1] But

[1] Mr. T. Mellard Reade, in an interesting paper (*Nat. Science*, iii, 1893,

although these shells are probably not in their original position, and the mere presence of marine organisms is no sufficient proof of former submergence, shells have been found near Inverness, five hundred feet above the sea, in the very place where they lived and died. Still, it does not follow that the submergence which they attest was interglacial.[1] Some inquirers believe that the glaciers advanced and retreated once and no more;[2] that there was only one slight elevation of the land and one slight subsidence: others that Britain was not only elevated twice, but also twice partially submerged; others that it was finally severed from the Continent in the earlier part of the Ice Age, when the drainage of Northern Europe, pouring into the North Sea and barred by the ice-sheet from escaping northwards, cut for itself a channel across the isthmus which now lies below the Dover Strait.[3] One expert still insists that when man first entered Britain the whole country stood at least six hundred feet above its present level:[4] another, in the same work, denies that its greatest elevation was more than seventy feet;[5] and their editor looks helplessly on. One writer suggests that there may never have been an Ice Age, in the strictest sense of the term, at all, but only local glaciers, such as now exist in Greenland.[6] Another has laboured to show that the accumulation of ice-sheets 'merely marked one or more culminating epochs in a period when the climate was at least as commonly temperate as Arctic'.[7] Others even now

pp. 423–35) has argued against the view that these shells were carried up the hill of Moel Tryfaen by a glacier. See also, in support of the theory of a period of extensive submergence, *Geol. Mag.*, 1893, pp. 35–7, 104–7; 1896, pp. 488–92; 1897, pp. 229–33.

[1] H. Carvill Lewis, *Papers and Notes*, &c., 1894, pp. 375–6; *Report of . . . the Brit. Association*, 1893 (1894), pp. 483–514; *Nature*, Aug. 16, 1906, p. 399; *Quart. Journ. Geol. Soc.*, lxii, 1906, pp. 33, 39. Mr. T. F. Jamieson, the author of the last-named paper, suggests that the submergence may have been confined to the northern part of Scotland.

[2] *Nat. Science*, iv, 1894, p. 472. Cf. *Trans. Roy. Soc. Edinburgh*, xl, part i, 1904, p. 82. [3] Clement Reid, *Origin of the Brit. Flora*, 1899, pp. 39–40.

[4] *Vict. Hist. of . . . Hampshire*, i, 253; *Vict. Hist. of . . . Somerset*, i, 176.

[5] *Vict. Hist. of . . . Sussex*, i, 25–6. See p. 19, *infra*.

[6] *Geol. Mag.*, 1895, pp. 63–4.

[7] C. Reid, *Origin of the Brit. Flora*, p. 38.

maintain that not one only, but five interglacial periods interrupted the intense cold ; [1] others again that there was no interglacial period at all, but only local ameliorations of climate.[2] Another fertile theme of controversy has been the origin of the boulder-clays. But the confession of a Fellow of the Royal Society, who, as a member of the Geological Survey, lived in Norfolk for eight years, studying its geology, suggests that, after all, a sense of humour may compensate for inability to fathom the mysteries of the Ice Age. 'After spending about a year in Norfolk,' he says, 'I began to believe I knew all about the drifts, but during the following seven years of my sojourn in that county, as I moved from place to place, I somehow seemed to know less and less, and I cannot say what would have been the result, but fortunately the geological survey of the county came to an end.' [3] Fortunately, too, it is not essential to our study of palaeolithic man to decide in every case between the theories of rival geologists. All admit that in Britain the Thames was the extreme southern limit of glacial movement, although even in the southern fringe Arctic conditions prevailed ; that glaciers covered a large part of the country north of the Thames, and on the higher regions coalesced into ice-sheets : the view that the lower boulder-clay was a *moraine profonde* has at last been generally adopted ; [4] while almost all agree that there was at least one interglacial period, and that there were climatic variations in certain tracts. Nevertheless one of the ablest and most experienced of our field geologists has recently given weighty reasons for his own conviction that even this solitary age of amelioration should not be regarded as an established fact.[5]

[1] See Sir A. Geikie's *Text-book of Geology*, ii, 1903, p. 1313. Cf. *Nature*, Aug. 16, 1906, pp. 388-9, 399.

[2] See Sir A. Geikie's *Text-book of Geology*, ii, 1903, p. 1313.

[3] *Proc. Geologists' Association*, ix, 1887, pp. 111-2.

[4] *Quart. Journ. Geol. Soc.*, liv, 1898, pp. 197-227, especially p. 209; *Trans. Roy. Soc. Edinburgh*, xl, part i, 1904, p. 83; *Vict. Hist. of . . . Durham*, i, 24. Professor Bonney, however (*Quart. Journ. Geol. Soc.*, lxii, 1906, pp. 491-2, 498), remains unconvinced.

[5] See Mr. G. W. Lamplugh's opening address, delivered in Section C of the

But, if we are to study the Palaeolithic Age intelligently, we must endeavour to test for ourselves the dogma that Britain was then continental. That dogma has recently been questioned by geologists who have minutely re-examined in the field the phenomena of the Glacial Epoch. Mr. Clement Reid, for instance, holds that in the Palaeolithic Age England never rose more than seventy feet above its present level,[1] and that men first entered it across a narrow strait which was formed in the earlier period of glaciation.[2] It is certain that the sea then washed the coast of Sussex and the western counties; for near Selsea there is a patch of boulder-clay—the only one south of the Thames—which must have been deposited by shore-ice, and there are rocks belonging to Bognor or the Isle of Wight, to the Channel Islands, and to Brittany, which were transported by icebergs and dropped when they melted under the summer sun.[3] Again, before the first English boulder-clay was formed Arctic plants flourished near Cromer; and, says Mr. Reid,[4] 'as these occur just above the present sea-level, and lie evenly on the strata below without deeply channelling them, the height of the land at the commencement of the Glacial Epoch, in Norfolk

Continental Britain.

British Association (*Nature*, Aug. 16, 1906, pp. 387–400). Mr. T. F. Jamieson, in a valuable and interesting paper (*Quart. Journ. Geol. Soc.*, lxii, 1906, p. 23), observes that 'although we have some evidence of more than one recurrence of an ice-sheet in [Aberdeenshire] . . . no evidence has hitherto been obtained of warm intervals, further than that which may be inferred from the melting away of the vast mass of ice which preceded and followed the deposition of the Red Clay and the shell-bed at Clava and elsewhere. It must have taken a great deal of heat to melt these enormous masses.'

[1] Is it certain that an elevation of seventy feet would not have been enough to unite Britain with the Continent? For thousands of years the scour of the tides must have been deepening the Channel. [On April 11, 1906, I submitted to Mr. Clement Reid, in a conversation which I had with him at the Geological Museum, the gist of the argument by which I endeavour to show (pp. 20–2, *infra*) that during some part of the Palaeolithic Age Britain must have been continental. He virtually admitted its force, remarking that an elevation of seventy feet would have enabled animals to cross from Gaul to Britain, as the scour of the tides had doubtless deepened the Channel.]

[2] *Origin of the Brit. Flora*, pp. 37, 38.

[3] *Ib.*, p. 41; *Quart. Journ. Geol. Soc.*, xlviii, 1892, pp. 344–61. Cf. *Vict. Hist. of . . . Hampshire*, i, 32.

[4] *Origin of the Brit. Flora*, p. 39. See also *Memoirs Geol. Survey—The Geology of the Country around Cromer*, 1882, p. 90.

at any rate, must have been almost the same as it is now'. The same observer assures us that in Southern Britain the first intense cold was succeeded, after an interval of which geology has nothing to tell, by an interglacial period in which the land sunk about one hundred and forty feet below its present level, so that shingle was deposited on what is now Portsdown Hill;[1] and that it then gradually rose until, long before the second glaciation began, its level, marked by fresh-water and estuarine deposits, once more virtually coincided with the present line.[2] But, he tells us, at some time after the disappearance of the ice which deposited the latest boulder-clay of Norfolk the land stood rather higher than now;[3] and he holds that even in the early part of the Neolithic Age Britain must have been almost connected with the Continent, for many of the river valleys were excavated to depths of from sixty to seventy feet below the present level of the sea.[4] The submerged forests of Devonshire, Cornwall, and the Bristol Channel, which contain traces of neolithic handiwork, flourished at a time when the land stood from fifty to seventy feet above its present elevation.[5]

But there are other facts which demonstrate that at some time after the first period of intense cold—perhaps in that interval of which geology has nothing to tell—the Continent must have included Britain. As we shall presently see, not only the mammoth, the woolly rhinoceros, the glutton, and other Arctic animals, but also many species which prefer a temperate climate, and others which are now tropical, lived in this country side by side with palaeolithic man. Nearly all of them had been represented here

[1] *Vict. Hist. of . . . Hampshire*, i, 33; *Vict. Hist. of . . . Sussex*, i, 22.

[2] *Vict. Hist. of . . . Hampshire*, i, 33–4; *Origin of the Brit. Flora*, pp. 42–3.

[3] *Report of the Brit. Association*, 1896, pp. 410–11. Cf. *Vict. Hist. of . . . Sussex*, i, 25–6. Mr. Reid has, however, concluded, from an examination of the palaeolithic deposits at Hitchin (*Proc. Roy. Soc.*, lxi, 1897, pp. 40–9, and especially p. 46), as well as at Hoxne, that before the time of the palaeolithic inhabitants of those districts the land had again sunk.

[4] *Origin of the Brit. Flora*, p. 46. Cf. A. J. Jukes-Browne, *The Building of the Brit. Isles*, pp. 291–2, 302.

[5] A. R. Wallace, *Island Life*, 1880, pp. 315–17; J. Prestwich, *Geology*, ii, 1888, pp. 523–5. See p. 62, *infra*.

before the earliest glaciers of Scotland were formed.[1] But even on the southern side of the Thames the cold was so intense during the earlier part of the Ice Age that none of the tropical, none even of the temperate species could there have lived: since the land was barren, treeless, and frozen,[2] even the mammoth, protected though it was by its woolly coat, could have found little food;[3] and large herds of Arctic animals travelled as far southward as Italy and Spain.[4] It is therefore evident that the beasts of tropical and of temperate climes whose remains have been found in the river-drift and in caves along with palaeolithic implements must have entered Britain after the coldest period had ceased.[5] Moreover, vast quantities of bones

[1] W. Boyd Dawkins, *Cave-Hunting*, 1874, p. 124; *Quart. Journ. Geol. Soc.*, xxxv, 1879, pp. 139–42; liv, 1898, pp. xcv–xcvi.

[2] Mr. Reid tells us that there was no vegetation except dwarf birches and willows and other Arctic plants. See *Nat. Science*, i, 1892, pp. 430, 432; C. Reid, *Origin of the Brit. Flora*, pp. 40, 42; *Vict. Hist. of . . . Hampshire*, i, 32–3.

[3] This fact seems to have escaped the notice of a writer who argues (*Nat. Science*, iii, 1893, pp. 261–6) that 'England was not restocked by a land connexion from the Continent after glaciation', and affirms (p. 266) that 'almost the only evidence of a post-glacial connexion with the Continent is the supposed necessity of such to account for our present fauna and flora'. The mammoth fed upon coniferous trees, fragments of the wood of which have been found in the crevices of its teeth (A. S. Woodward, *Outlines of Vertebrate Palaeontology*, 1898, p. 306).

[4] *Quart. Journ. Geol. Soc.*, xxxv, 1879, pp. 142–3.

[5] Sir Henry Howorth has written a series of articles (*Geol. Mag.*, 1892, pp. 250–8, 396–405; *Nat. Science*, xii, 1898, pp. 261–70), in which he claims to have proved that 'in no instance, so far as we know, does the drift actually underlie any land surface containing the remains of the mammoth and of its contemporaries'. Translated into the language of geologists to whom the glacial period is not a nightmare, this is tantamount to an assertion that the mammoth was neither postglacial, nor interglacial, nor glacial, but preglacial. Mr. A. J. Jukes-Browne (*Geol. Mag.*, 1892, p. 575) replies that 'gravels containing mammoth remains occur in many other valleys [besides that of the Great Ouse], which are generally considered to have been eroded out of a widespread mantle of Glacial Drift', and that 'this conclusion is not shaken by anything which Sir H. Howorth has written'; while Sir John Evans (*Anc. Stone Implements*, 1879, p. 701) remarks that 'in some cases, as at Fisherton, the worked flints have been found below the remains of mammoth'. Since the gravel at Hoxne, in which bones of the mammoth and of extinct animals contemporary with the mammoth were found (Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 573–5; C. Reid, *Origin of the Brit. Flora*, p. 77; *Guide to the Ant. of the Stone Age* [Brit. Museum], p. 20), was shown by the committee who

of Pleistocene mammals, some of which, such as the reindeer, have never been found in Britain in preglacial deposits, have been dredged up out of the bed of the North Sea, principally from the Dogger Bank ;[1] and it is therefore clear that at some time after the climax of the Glacial Period that sea or a large part of it did not exist. It cannot indeed be proved that the men of the river-drift and the caves entered Britain as soon as the other animals ;[2] and possibly the Dover Strait may have existed as a narrow channel at the time of their arrival : but since the bones that were raised from the Dogger Bank appear to belong to the time when the Thames was laying down the gravels in which men's tools have been found,[3] it seems probable that the land bridge was standing in some part of the Palaeolithic Age.

The relation of palaeolithic man to the Ice Age.

It has been demonstrated that palaeolithic men were living in East Anglia after glaciers had finally disappeared from that part of the country. The valleys of the Ouse and its tributaries, in the gravels of which their implements are to be found, were worn down through boulder-clay.[4] Excavations at Hoxne in Suffolk have shown that the people who left their tools there lived at a time which was separated by two climatic waves, attested by the flora of two sets of strata, from the age in which the latest boulder-

excavated it (*Report of . . . the Brit. Association*, 1896, pp. 400–11) to be later than the latest glaciation of the district, Sir Henry naturally discredited their report ; and he did so by declaring (*Nat. Science*, xii, 1898, p. 266) that the members of the committee were 'already committed . . . to the view that the implement-bearing deposit at Hoxne was newer than the Drift. This,' he continued, 'was not very promising. . . . It was, in fact, indecent.'

[1] Cf. *Quart. Journ. Geol. Soc.*, xxiii, 1867, p. 107, with *Geol. Mag.*, 1878, p. 98, and Boyd Dawkins, *Early Man in Britain*, 1880, p. 149 ; and see also the same writer's *Cave-Hunting*, 1874, p. 362. Mr. H. B. Woodward states (*Vict. Hist. of . . . Norfolk*, i, 23) that 'the Dogger Bank is a remnant of old Pleistocene deposits ; as Mr. Reid suggests [*Memoirs Geol. Survey,—The Geology of the Country around Cromer*, 1882, p. 122], a re-extension of the old Rhine estuary'. I confess that I do not understand how Mr. Reid would reconcile this suggestion with his belief that the Channel was formed in the earliest part of the Ice Age.

[2] *Vict. Hist. of . . . Hampshire*, i, 34.

[3] *Memoirs Geol. Survey,—The Geology of the Country around Cromer*, p. 122 ; *Vict. Hist. of . . . Norfolk*, i, 23.

[4] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 577, 683, 685, 697.

clay of that district had been deposited.[1] Moreover, in many cases in which evidence has been adduced to show that palaeolithic remains are of glacial or interglacial date, doubts have arisen either as to the artificial character of the flints or as to the age of the beds in which they were found.[2] When, for instance, a member of the Geological Survey announced that he had found palaeolithic implements at Brandon in Suffolk in three interglacial beds, separated by layers of boulder-clay,[3] Sir John Evans suggested that the clay was not in its original position, but had slipped down from a higher level.[4] Again, Dr. Henry Hicks and Sir Joseph Prestwich were convinced that the cave of Cae Gwyn in the Vale of Clwyd had been inhabited before the climax of the Ice Age.[5] Here a flint flake was taken out of earth separated by a superincumbent bed of clay from a layer of sand and gravel, above which again rested boulder-clay that, in Hicks's judgement, showed no sign of having ever been disturbed, and which, in the opinion of Mr. Clement Reid,[6] must have been deposited before the last glaciation of the district. Even this evidence, however, is not unanimously accepted. Flints have also been found in the Cromer Forest Bed at East Runton, which was certainly preglacial; but Sir John Evans cannot see on them the faintest marks of human workmanship.[7]

[1] See p. 21, n. 5, *supra*, and cf. *Journ. Anthr. Inst.*, xxxv, 1905, pp. 308, 310.

[2] *Journ. Anthr. Inst.*, xxix, 1899, pp. 333–4; *Proc. Liverpool Geol. Soc.*, ix, 1901, pp. 18–19.

[3] *Memoirs Geol. Survey,—On the Manufacture of Gun-Flints*, 1879, p. 68. Cf. S. H. Miller and S. B. J. Skertchly, *The Fenland*, 1878, pp. 548–51.

[4] *Anc. Stone Implements*, 1897, p. 568. See, however, *Proc. Geologists' Association*, ix, 1887, p. 126.

[5] *Quart. Journ. Geol. Soc.*, xliii, 1887, p. 117; liv, 1898, pp. lxxxvi–lxxxix.

[6] *Ib.*, lx, 1904, pp. 132–3.

[7] Professor Boyd Dawkins (*Quart. Journ. Geol. Soc.*, xxiii, 1867, pp. 91–109, and especially pp. 106–9) has argued that the Lower Brick-Earths in the Thames Valley, under which implements have been found, were preglacial,—locally, I presume. He observed that not one of 'the Post-glacial Arctic mammalia', namely, the glutton, lemming, marmot, musk-sheep, elk, and reindeer, is represented in this deposit, and maintains that, on the other hand, the presence of *Elephas priscus* and the big-nosed rhinoceros (*Rhinoceros megarhinus*) 'indicates the affinity of the [Brick-Earth] group to the Praeglacial deposits of Norfolk', &c. Prestwich, however (*ib.*, xxviii, 1872, p. 445),

Nevertheless, it is not improbable that when the hunters whose tools have been exhumed from the drift of South-Eastern Britain were living in a comparatively mild climate, Scotland, the Lake Country, and the highlands of Yorkshire and Wales may still have been partially buried beneath ice.[1] The high-level drift of the Thames valley, which has yielded so many implements, is believed by eminent geologists to have been laid down at a time when ice spread over Northern Britain;[2] and in support of this view it has been contended that in those regions no palaeolithic implements have been found.[3] The argument cannot be easily set aside; but it has been pointed out that in the northern districts, owing to the extreme scarcity of flint, stone tools could only have been made of harder rocks, on which it is not so easy to detect marks of human agency; that the alluvial deposits in those parts are not readily accessible to search; and that, if they are patiently explored, implements may yet be recovered from them.[4] Some years, however, have elapsed since this suggestion was made; and it has not yet been verified. Moreover, the absence from the country north of Yorkshire, save in a few preglacial deposits, of such bones as have been found with palaeolithic remains seems to indicate that the animals contemporary with palaeolithic man were unable to find food in Northern Britain owing to the continuance of an Arctic climate.[5] Man was undoubtedly living in Southern Britain in the cold period that succeeded the so-called inter-

differed from the professor; and Sir John Evans (*ib.*, p. 446) remarked that if the brick-earth were preglacial 'there would be a great difficulty in accounting for the presence of the high beds at Shackleton and Highbury, as these, though in a valley confessedly excavated by the river, and regarded as of more recent age than the lower beds, would yet be at a far higher level'.

[1] Prof. P. F. Kendall maintains (*ib.*, lx, 1904, p. 132) that even the Hoxne implements 'were of very late Glacial, perhaps the very latest Glacial Age,'—not, as I understand, of the Hoxne district, but of Britain as a whole. Cf. *Man*, iii, 1903, No. 31, p. 59.

[2] *Proc. Geologists' Association*, ix, 1887, p. 129; J. Prestwich, *Controverted Questions*, p. 45.

[3] See *Geol. Mag.*, 1894, p. 79.

[4] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 580.

[5] *Quart. Journ. Geol. Soc.*, xxviii, 1872, p. 435; xxxv, 1879, pp. 142-3.

glacial period of Sussex and Hampshire; for the plateau gravels that cap the Bournemouth cliffs, in which his tools have been found, are older than the valley gravels of the Hampshire Avon and the Stour, which were formed towards the end of the Ice Age by torrents that streamed over frozen chalk downs impervious to water and swept away the fragments of their crumbling surface.[1] Furthermore, stone implements have been found at Caddington below, and near London embedded in, a stratum known as 'contorted drift', which is believed to have been formed in a period of great cold;[2] and it is merely a question of words whether this period is to be included in the last phase of the Ice Age.[3]

'Eolithic' man?

But there is one district from which evidence has been obtained that has convinced many who sought conviction, that there were men in Britain before the first British palaeolithic tool was made. In the village of Ightham, near Sevenoaks, lives a tradesman, named Benjamin Harrison, whose discoveries have caused much searching of heart, if they have not revolutionized our knowledge of the life of early man. In 1885 he began to search for old stone implements on the chalk plateau between the valleys of the Medway and the Darent. There, embedded in patches of gravel that must have been drifted on to the plateau from hills higher still, which had been already worn down by denudation even when palaeolithic hunters were roaming among herds of mammoths in the valley of the Thames, he found flints of divers shapes which seemed to him to bear sure traces of man's handiwork, and which have been

[1] *Memoirs Geol. Survey,—The Geology of the Country around Bournemouth*, 1898, p. 10; C. Reid, *Origin of the Brit. Flora*, pp. 44–5; *Vict. Hist. of . . . Hants*, i, 35; *Vict. Hist. of . . . Sussex*, i, 22.

[2] Worthington G. Smith, *Man, the Primeval Savage*, 1894, pp. 170, 173, 191, 217–8. Cf. H. B. Woodward, *Geol. of England and Wales*, 1887, pp. 510–12, and Sir J. Evans, *Anc. Stone Implements*, 1897, p. 698.

[3] The descriptions, based upon the remains of fauna in caves, that have been given of the climate of Southern Gaul in successive periods of the Palaeolithic Age, however true they may be, do not apply in Britain. The little that is known of our climate suggests to Mr. Clement Reid 'extremes with sharp alternations of cold, drought, and sudden floods' (*Man*, iii, 1903, No. 29, p. 56, with which cf. *Proc. Roy. Soc.*, lxi, 1897, p. 46).

termed 'eoliths', or stone implements of a dawning age. Nearly all of them, indeed, were so rude that the chipping on their edges has been ascribed by sceptics to the action of nature. But if even a small fraction of them could be proved to be authentic, the contention of their finder would be established. They recur, again and again, in certain well-defined and peculiar shapes; the chips have in many cases been removed not from the exposed parts but from concave sides which, he would have us believe, natural agents could hardly have affected;[1] if Sir John Evans and other experts are unable to accept them as artificial, Canon Greenwell,[2] Pitt-Rivers,[3] and Prestwich[4] were convinced that they had been wrought by man; even the labourers who picked them out of the gravel hardly ever failed to distinguish them from the surrounding flints;[5] and, if we may believe the champions of their authenticity, those who assert that they were shaped by nature have failed to produce stones of similar forms from the valley-drift.[6] Now when the hunters of the Thames valley were making their tools, Britain had the same main features of hill and dale that it has to-day; but when the gravels were being drifted on to the Kentish plateau, Thames and Medway were yet unborn; and, filling the great valley that now lies between the North Downs and the Lower Greensand hills, some five miles further south, the plateau rose southward to Central Wealden uplands two thousand feet or more above the sea. With no special knowledge of geology the antiquary who spends a holiday in walking from Sevenoaks or Wrotham on to the plateau may satisfy himself that this is true. Mingled with the eoliths in the patches of

[1] *Journ. Anthr. Inst.*, xxiii, 1894, p. 275; B. Harrison, *Outline of the Hist. of the Eol. Flint Implements*, 1904, pp. 9–10.

[2] *Nineteenth Century*, April, 1895, p. 623.

[3] *Journ. Anthr. Inst.*, xxi, 1892, p. 272.

[4] *Controverted Questions*, p. 77.

[5] *Ib.*, p. 78. See also *Quart. Journ. Geol. Soc.*, liv, 1898, p. 298.

[6] The Rev. R. Ashington Bullen (*Nat. Science*, xii, 1898, p. 107) says that he has found 'worked flints of the plateau types' in 'valley gravels', but that they were 'derived specimens'. See also *ib.*, pp. 111–16. On the other hand, see p. 27, *infra*.

drift are fragments of chert that must have been washed down from the Lower Greensand at a time when it rose high above the plateau's level; for south of the eolithic area, inclining upward below the chalk and below the Upper Greensand, the outcrop of the Lower Greensand shows itself still. The plateau drift lies upon rock of pre-glacial age;[1] and although there is no evidence that it is itself older than the Pleistocene period, some geologists hold that it was deposited soon after, perhaps before, British glaciers began to form.[2]

But assuming that the eoliths are artificial, does it follow that they are older than the oldest palaeoliths, or that they were wrought by a race different from the men of the valleys? Mr. Clement Reid has pointed out that the gravel at Alderbury, some three miles below Salisbury, in which multitudes of eoliths have been found, is on exactly the same level as that of a gravel three miles lower down the valley, where Prestwich picked up a palaeolithic implement which had fallen from a yet higher elevation.[3] If the position of this implement was an index of its age, eoliths were being used in Wiltshire after palaeoliths had begun to be manufactured.[4] On the other hand, it is asserted that eoliths have lately been found in Tertiary deposits on the high plateau above the Avon;[5] and one

[1] *Nat. Science*, v, 1894, pp. 269, 271–2; *Nineteenth Century*, April, 1895, p. 626.

[2] J. Prestwich, *Controverted Questions*, 1895, p. 54. Cf. *Quart. Journ. Geol. Soc.*, xlv, 1889, p. 295, and *Journ. Vict. Inst.*, xxxiii, 1901, p. 223.

[3] *Quart. Journ. Geol. Soc.*, xxviii, 1872, pp. 39–40; Sir J. Evans, *Anc. Stone Implements*, 1897, p. 632; *Memoirs Geol. Survey,—The Geology of the Country around Ringwood*, 1902, pp. 36, 39.

[4] As Mr. Clement Reid points out (*ib.*, pp. 36–7), Prestwich's implement 'was not found in place, but picked up among fallen material. . . . The Alderbury gravel,' he remarks, 'judging from its less elevation above the river, is probably newer than the supposed Palaeolithic gravel north of Redlynch; yet it yields implements of more ancient type.' See also *Journ. Anthr. Inst.*, xxxv, 1905, p. 354, where Mr. H. Warren affirms that eoliths are associated with palaeolithic implements in the drift of High Down, Isle of Wight; and cf. *Man*, v, 1905, No. 80, p. 146.

[5] *Geol. Mag.*, 1903, pp. 105–6. Mr. Reid thinks that the beds in which these flints have been found are not necessarily of Pliocene date, as they may have been *remaniés*. Eoliths are said to have been unearthed from gravels at Dewlish in Dorsetshire side by side with the bones of the extinct elephant

geologist, who rejects all eoliths, would argue that Benjamin Harrison's labours have not been vain. Many palaeolithic implements have been found on the Kentish plateau, but never embedded in association with eoliths: most of them are unworn, and look as if they had remained on the very spot where they were lost; and it is easy to see that they are far less ancient than the eoliths. But certain implements have also been found there which, although they were not lying in the gravels, appeared to bear marks of having been derived from them and washed down in the same drift that contains the eoliths. Like the latter they were stained deep brown, covered with glacial scratches, and coated with the white deposit of silica.[1] If this argument had been generally accepted, one might conclude that the greater antiquity of British man does not depend for its proof upon the authenticity of the eoliths. What all admit is that in France flints of eolithic form have been found even in Tertiary beds.[2]

known as *Elephas meridionalis*, whose remains have never yet been met with in this island except in preglacial beds (*Quart. Journ. Geol. Soc.*, xliv, 1888, pp. 318–24; lxi, 1905, pp. 35–8; *Journ. Vict. Inst.*, xxxiii, 1901, pp. 212–3); but these flints were so battered that Mr. Reid, who accepts many eoliths as genuine tools and regards them as 'bad palaeoliths', was obliged to reject them (*Memoirs Geol. Survey,—The Geology of the Country around Ringwood*, p. 36).

[1] *Quart. Journ. Geol. Soc.*, liv, 1898, pp. 291, 293–4.

[2] *Association franç. pour l'avancement des sc.*, 1903, 1re partie, pp. 246–7; *Nature*, lxxii, 1905, pp. 438–9; *Journ. Anthr. Inst.*, xxxv, 1905, p. 261. Are any of these flints identical in form with the characteristic Kentish specimens?

Sir John Evans, who is unable to accept the authenticity of any eoliths, nevertheless believes, or did believe in 1897 (*Anc. Stone Implements*, pp. 608–9), that the palaeolithic implements which have been found on the plateau belonged to a time when the 'continuous slope now extending from the neighbourhood of the Thames to the summit of the chalk escarpment' was 'continued southward . . . over a part of what is now the Lower Greensand area, if not, indeed, into that of the Weald'. In other words he believed that the palaeoliths were as old as the eoliths, and therefore that the question of the authenticity of the latter was unimportant. It is, however, now generally recognized that this view was based upon a misconception. Mr. Harrison (*Outline of the Hist. of the Eol. Flint Implements*, p. 17) states that '*palaeoliths and eoliths have been found together only on the surface and never in the drifts in situ*'. Cf. J. Prestwich, *Controverted Questions*, p. 64.

Mr. Clement Reid (*Vict. Hist. of . . . Hampshire*, i, 34) sees no reason for believing that any of the Kentish eoliths are older than palaeolithic implements in general. In a conversation which I had with him on April 11, 1906, he

But while the extreme antiquity of many eoliths is certain, the question of their authenticity has recently been debated with renewed and redoubled vigour. About two years ago an eminent French palaeontologist, Monsieur Marcellin Boule, announced that in the process of manufacturing cement at Mantes many flints had been converted into eolithic forms;[1] and it has been contended that the conditions which were actually observed in the factory were analogous to those of the torrential streams by which flints may have been dashed hither and thither as they were swept on to the Kentish plateau in primaeval times.[2] An ardent advocate of the authenticity of eoliths insisted that some of the Kentish types would be looked for in vain among the machine-made specimens from Mantes;[3] but a sceptic affirmed that he had himself found an eolith, manifestly untouched by man, with its notch accurately fitting against another stone, the two having been ground together by a natural process which he described as the slipping, sliding, and foundering of the insoluble surface material from higher to lower levels.[4] Although it was objected that certain rectangular eoliths with blunt edges could not have been produced except by art,[5] it is permissible to doubt whether the human origin of eoliths will ever be established beyond dispute; and he who reflects that they have been met with not only in Tertiary beds but in those immeasurably later deposits which were contemporary with or but little older than palaeolithic man[6]

remarked that the patches of drift in which the eoliths had been found were generally dominated by higher ground, and that he could find no evidence that the flints had been washed down from the Weald. Eoliths have, however, been found in a pit at Terry's Lodge 'on the summit of the escarpment at a height of 770 feet' (*Journ. Anthr. Inst.*, xxxv, 1905, p. 360. Cf. *Essex Naturalist*, xiii, 1904, p. 332).

[1] *L'Anthropologie*, xvi, 1905, pp. 257–67.

[2] *Man*, v, 1905, No. 102, p. 179. Cf. No. 92, p. 165.

[3] *Ib.*, No. 91, p. 165.

[4] *Ib.*, No. 103, pp. 180–83. Cf. *Journ. Anthr. Inst.*, xxxv, 1905, p. 363, fig. 7.

[5] *Ib.*, p. 361.

[6] Mr. Hazzledine Warren (*Journ. Anthr. Inst.*, xxxv, 1905, p. 358) goes so far as to affirm that the mineral condition of some of Mr. Harrison's eoliths 'shows that they . . . are . . . clearly as late as the neolithic age'. There is

will leave them for the present without regret to the consideration of enthusiasts.

The environment of palaeolithic man in Britain.

Let us then try to conceive of the environment of those palaeolithic hunters of whose culture we have clearer indications in a late phase of the Ice Age, when the glaciers of Southern Britain had passed away. Then the configuration of the country was very different from that which we behold. The chalk ranges of Kent and of Picardy were unbroken. The Thames, fed sometimes by torrential rains, flowing rapidly and fitfully in the broad shallow valley which it was excavating, was depositing gravels on the slopes that bordered it, a hundred feet above the level of its existing waters,[1] and wandering far eastward across a plain from whose now sunken surface bones of mammoth and reindeer, of hyena and bear have been dredged, to swell that greater Rhine which found no outlet till it reached a far northern sea. Mammoths, woolly rhinoceroses, and giant elks with antlers ten feet across, roamed in the forests; hippopotamuses swam in the streams;[2] brown bears and grizzly bears and lions and hyenas made their dens in caves, and dragged into their dark and sinuous recesses the prey which they had torn down in the open.

Whence did he come?

The earlier palaeolithic immigrants, impelled perhaps by scarcity of game, had crossed the valley of the Dover Strait doubtless from the nearer parts of France or Belgium; but the original home of the race is unknown, for palaeolithic tools have been found not only in this island and almost every European country except Scandinavia, but also in North Africa, in the valley of the Nile,[3] in Palestine and

a bibliography of eoliths in *Geol. Mag.*, 1903, pp. 108–10, to which may be added, besides the works quoted in this chapter, *Rev. de l'École d'anthr.*, xiv, 1904, pp. 240–6.

[1] See p. 32, *infra*. It must of course be remembered that this description applies only to one part of the Palaeolithic Age: palaeolithic man was still here when the Thames had cut out its valley to its present depth.

[2] See p. 40, n. 2, *infra*.

[3] M. Boule (*L'Anthr.* xiv, 1903, p. 533) regards the question of the existence of a palaeolithic age in Egypt as unsettled; but, as Mr. H. R. Hall observes (*Man*, v, 1905, No. 19, p. 34), 'German investigators . . . have no doubt whatever that the Pitt-Rivers flints from Thebes and those of palaeolithic type from the Wâdi esh-Shêkh and elsewhere are in reality palaeolithic.' See also

Asia Minor, the Euphrates Valley, Somaliland, India, and North America: as a high authority has remarked, they are 'so identical in form and character with British specimens that they might have been manufactured by the same hands';[1] and the same may be said of those which were wrought by the Tasmanians, who, fifty years ago, had not yet been exterminated by the pioneers of Christian civilization.[2]

Chronological puzzles.

Many attempts have been made to calculate the number of millenniums that have elapsed since our Palaeolithic Age began and since it came to its end. Croll, the author of the astronomical theory of the Ice Age, finally concluded that that epoch ceased about eighty thousand years ago;[3] and Sir Archibald Geikie laboured in his youth to estimate the time which the rivers would have taken to excavate their valleys from the days when they were depositing the high-level gravels to the era when they reached their present depth.[4] But any one who uses his powers of reflection will see how many elements of uncertainty must stultify such a method as this;[5] and, since the cause of the Ice Age remains unknown, the calculations of Croll were futile.[6] Indeed, if it were possible to prove that eighty thousand years have passed since the beginning or since the end of the Palaeolithic Age, not much would be gained; for whose mind can conceive what such a period means?

A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv. 9–11 (preface), and *Association franç. pour l'avancement des sc.*, 1903, 2e partie, p. 860. Palaeolithic implements are also said to have been found in Patagonia (*L'Anthr.*, xvii, 1906, p. 255).

[1] Sir John Evans in *Report of . . . the Brit. Association*, 1897 (1898), p. 14. Cf. *Anc. Stone Implements*, 1897, pp. 528–30, 650–54. In regard to palaeolithic remains in America see also *Congrès internat. d'anthr. et d'archéol. préhist.*, 1900 (1902), p. 191.

[2] *Journ. Anthr. Inst.*, xxiii, 1894, p. 147. Cf. A. Pitt-Rivers, *Evolution of Culture*, 1906, p. xvi.

[3] *Climate and Time*, 1885, pp. 327–8.

[4] *Geol. Mag.*, 1868, pp. 249–54.

[5] J. Prestwich, *Controverted Questions*, pp. 22, 42; Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 705–7.

[6] *Geol. Mag.*, 1895, pp. 3–13, 55–65; A. Geikie, *Text-book of Geology*, 1903, pp. 1326–7. See also *Nature*, lii, 1895, p. 594; liii, 1895–6, pp. 29, 196, 220, 269, 295, 317, 340, 388, 460; and *Quart. Journ. Geol. Soc.*, lviii, 1902, pp. 37–45.

The wiser archaeologists have given up the quest of chronological precision; and they know that the imagination may be stimulated by more legitimate means. Go to Caversham and stand upon the gravels washed down by the Thames in his lusty youth:[1] one hundred and twenty feet below he is flowing now; think of the ages that passed while his waters were hollowing out that valley, which was as it is still before the Palaeolithic Age had passed away. Walk along the cliff near Bournemouth, and look out over the Solent Sea. That cliff was once a river bank; and even the cautious geologist who has described how Hampshire was wrought into its present form is willing to believe that man had then appeared in our land. Where you see salt water he would have seen dry land, bounded far away by a range of hills which linked the downs of the Isle of Wight to those that rise behind Weymouth Bay, and of which the Needles remain as lonely relics: he would have seen the Solent flow, a mighty river, enriched by the tribute of the Stour, the Avon, the Itchen, and the Test.[2] Ascend the hill on which stands Dover Castle, and gaze upon Cape Grisnez. Let the waters beneath you disappear: across the chalk that once spanned the Channel like a bridge men walked from the white cliff that marks the horizon to where you stand. No arithmetical chronology can spur the imagination to flights like these.[3]

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 592, 708–9.

[2] Clement Reid in *Vict. Hist. of . . . Hampshire*, i, 34; *Archaeologia*, lix, part ii, 1906, p. 286, and *Memoirs Geol. Survey,—The Geology of the Country around Ringwood*, pp. 31–2.

Sir John Evans (*Anc. Stone Implements*, 1897, pp. 634–5, 690–93) has argued that the implementiferous gravel which caps the cliff at Bournemouth was deposited by the Solent river; but Mr. Clement Reid thinks it 'very doubtful whether it was a deposit formed by ordinary river action' (*Vict. Hist. of . . . Hampshire*, i, 34); and (*ib.*, pp. 27–8, 34) he is inclined to believe that the continuity of the Isle of Wight with Hampshire and Dorsetshire was already interrupted in late Pliocene times, though the Solent may perhaps have been merely an estuary and not a strait even in the time of the so-called interglacial estuarine deposits. See p. 20, *supra*.

[3] Dr. A. J. Evans (*Report of . . . the Brit. Association*, 1904 [1905], p. 721) calculates that the earliest settlement at Knossos in Crete (which was *neolithic*) is about 12,000 years old; but he assumes that in the western court of the palace 'the average rate of deposit was fairly continuous'. Prof. Montelius

The dwellers on the plateau, if they did exist in pre-glacial times, have left us no memorial save their tools: but can we picture to ourselves the lineaments of the palaeolithic hunters who came after them? Human bones, including two perfect skulls, closely associated with the bones of hyenas, have been recovered from a cave near Plymouth. The average height of the people to whom they belonged was little more than five feet: the skulls have hardly been described with sufficient accuracy to enable us to compare them with others of the same period; but, in regard to breadth and to the degree of projection of the lower jaw, they were not very different from the majority of modern British skulls.[1] Two other human skulls have been found in England for which palaeolithic age has been claimed—one near Swanscombe in Kent, the other near Bury St. Edmunds; but the former may not be as old as the bed from which it was unearthed; and the other was so broken that its contour could hardly be restored.[2] But almost all the older palaeolithic skulls that have been found in Western Europe belong to the same type, which is generally called after the famous specimen that was exhumed nearly half a century ago in the Neander valley in Rhenish Prussia, and of which the most characteristic examples were derived from a cavern at Spy in the province of Namur. The Swanscombe skull has somewhat similar characters; and it has been supposed that the earlier palaeolithic inhabitants of Britain belonged to the Neanderthal race. Unfortunately, however, the

Palaeolithic skeletons.

(*L'Anthr.*, xvii, 1906, p. 137) argues from the stratigraphy of finds at Susa that the beginning of the Neolithic Age in the East may be dated about 18,000 B.C. But even if these calculations could be established, it would still remain doubtful whether our Palaeolithic Age was not partly contemporary with a neolithic civilization in more genial climates. Probably it was (*ib.*, p. 164). Against the theory which would minimize the antiquity of the Palaeolithic Age, see *ib.*, xv, 1904, p. 66, and *Quart. Journ. Geol. Soc.*, xliii, 1887, p. 410, and in favour of it *L'Anthr.*, xvii, 1906, p. 27, n. 1.

[1] *Trans. Devon. Association*, xix, 1887, pp. 419-37. Neither of the skulls could be removed intact, but one was photographed (*ib.*, p. 433). The forehead recedes, but not excessively: the supraciliary ridge is strong, but not abnormally developed.

[2] See p. 380, *infra*.

dates of the Neanderthal and Spy specimens cannot be fixed. The latter may belong to the comparatively advanced period in which the best palaeolithic stone implements of France were manufactured: the former was not seen in place by a competent observer, and its age is quite uncertain.[1] If the very few skeletons that we possess are typical, these men were short, big-boned, and powerfully built. Their heads were long and narrow, their foreheads amazingly low and retreating, and their jaws heavy and projecting. But their most striking features were enormously massive and outstanding brow ridges. Although the Neanderthal skull was described by Huxley as the most apé-like of all human skulls, and although for some time after its discovery it was the subject of animated discussion, it and its congeners were thenceforward regarded by all anatomists until the beginning of the present century as human in the strictest sense of the word. Within the last few years, however, a German anthropologist has endeavoured to prove that it and the two skulls of Spy may only be called human in a limited sense: he refuses to class them under the head of *Homo sapiens*, and refers them to an older species, which he calls *Homo primigenius*. This view, however, has not made influential converts: the Neanderthal skull was capacious enough to lodge a brain as large as that of many a living savage; and trained observers have pointed out that skulls of like contour have belonged in modern times to men of considerable mental power.[2] A considerable number of skeletons have lately been discovered in Moravia, which, although like the Neanderthal race they had long skulls and prominent brows, belonged to a higher type, and, as the length of their thigh-bones showed, were of great stature;[3] while the caves of Baoussé-Roussé, near Mentone, were the resting-place of very ancient men, in whose skeletons anatomists have detected certain negroid characteristics, although their skulls must have contained a large volume of brain.[4]

But the Palaeolithic Age was of such vast duration that

[1] *L'Anthr.*, xvii, 1906, pp. 70–3. See pp. 380–1, *infra*.
[2] See pp. 380–1, *infra*. [3] See p. 381, *infra*. [4] See pp. 382–3, *infra*.

before its close Britain may well have been invaded by new races. In the latest period there were living in the Riviera a people whose physical features connect them with the earliest French neolithic race; and in South-Western France skulls of like type have been found at Laugerie-Basse and Chancelade in the valley of the Lozère.[1] The relics of these men which have been discovered in the caves in which they dwelled show that some of them were worthy to be called forerunners of Pheidias and Praxiteles. With their tools of flint or chert they carved ivory dagger-handles, or, as we are now assured, objects of uncertain use,[2] adorning them with figures of the heads of reindeer, and scratched on horns or tusks drawings of mammoths, deer, horses, and hunters spearing salmon, of which the finer examples are recognized by modern artists as true works of art.[3] A single specimen, found in the Robin Hood Cave in Creswell Crags, is all that we can show:[4] but implements with which it was associated present points of likeness to those of the French caves which justify the assumption that the primitive artists of France sent emigrants to our land.

Palaeolithic artists.

The palaeolithic nomads, whether of the earlier or the later race, pushed their way as far north as Lincolnshire, Derbyshire, and Denbighshire, perhaps even into the East Riding of Yorkshire; and as far west as Glamorganshire, Caermarthenshire, and Devonshire:[5] but almost all the

Range of the palaeolithic hunters in Britain.

[1] See pp. 382–3, *infra*.

[2] Mr. C. H. Read (*Guide to the Ant. of the Stone Age* [Brit. Museum], pl. 1 and p. 49) has no doubt that they were dagger-handles; but the abbé Breuil (*L'Anthr.*, xvi, 1905, p. 632) affirms that it is 'démontré qu'aucune des sculptures dont on a voulu faire des manches de poignard n'ont eu ce rôle'.

[3] E. Lartet and H. Christy, *Reliquiae Aquitanicae, passim*; *L'Anthr.*, v, 1894, pp. 129–46; vi, 1895, p. 143; xiv, 1903, pp. 295–315; xv, 1904, pp. 129–76, 625–44. Among the palaeolithic artists were not only carvers and engravers but also draughtsmen and even painters. On the walls of caves in the Spanish Pyrenees are many-coloured frescoes, depicting animals as well as objects the meaning of which is still unknown. See *L'Anthr.*, xv, 1904, p. 629; xvi, 1905, pp. 437, 442; *Rev. de l'École d'anthr.*, xiv, 1904, pp. 320–5; xv, 1905, pp. 150–5; and *Man*, vi, 1906, No. 63, p. 96.

[4] *Quart. Journ. Geol. Soc.*, xxxiii, 1877, p. 582.

[5] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 581; *Guide to the Ant. of the*

remains of their handiwork have been found in the south-eastern district of England,—in Kent, especially the neighbourhood of Reculver, Sussex, Hampshire, Essex, Middlesex, and Surrey, Hertfordshire, Buckinghamshire, and Northamptonshire, Oxfordshire and Berkshire, Cambridgeshire, Norfolk, and Suffolk.

Where their tools have been found.

The places in which these relics lay buried may be grouped in four classes,—the plateau gravels, already described; gravels which were apparently deposited not by rivers but by heavy rains which, falling upon frozen chalk downs, destroyed the shattered surface and swept it away in floods;[1] the river-drift, and caves; and, unlike the belongings of the neolithic herdsmen, those of the older inhabitants are not to be found, except in special cases, on or near the surface of the earth. The amateur who has acquired the rudiments of geology and has learned to discern stone implements among the fragments of rock which surround them, knows that in the gravels and sands which rivers deposited at various elevations when they were flowing now here now there in higher and wider channels he may hope to find specimens to add to his collection. Common sense too teaches him that in the same valley the higher terraces were formed before the lower, and that the tools which they contain, however closely they may resemble

Stone Age (Brit. Museum), p. 6. Mr. Clement Reid (*Quart. Journ. Geol. Soc.*, lx, 1904, pp. 106–12), has described 'a probable Palaeolithic Floor [or old land surface] at Prah Sands, Cornwall'; but in the discussion which followed the reading of the paper he admitted that he 'would not like to speak confidently as to any one of the stones being an implement'.

[1] See *Nat. Science*, iii, 1893, p. 369; *Vict. Hist. of . . . Hampshire*, i, 35; *Mem. Geol. Survey,—The Geology of the Country around Ringwood*, 1902, p. 48; *Man*, iii, 1903, No. 29, p. 56; and *Quart. Journ. Geol. Soc.*, lx, 1904, p. 130.

Mr. Clement Reid, as those who are familiar with his writings must have seen, does not believe that many of the deposits classed as river-drift (see Evans's *Anc. Stone Implements*, 1897, pp. 662–709, especially 679) deserve that title. In a conversation which I had with him on April 11, 1906, he remarked that he could see no reason to suppose that palaeolithic man 'was an aquatic animal'; that much of the so-called river-drift would probably be found, under minute examination, not to be due to fluviatile action; and that the geology of the Thames Valley, which in the Glacial Epoch was on the edge of the ice, presented great difficulties. See, however, Mr. H. B. Woodward's article in *Vict. Hist. of . . . Buckingham*, i, 22.

those which are embedded below, are nevertheless, as a rule, far older.[1] If he asks himself how they found their way into these gravel beds, reflection will soon suggest the answer. It would seem that although the palaeolithic hunters dwelled sometimes near lakes or ponds, they usually settled on the banks of streams. Fishing, hunting, wading through fords, warned by swiftly rising floods to quit their habitations, they lost or abandoned the weapons which now serve our purpose instead of theirs. But in some cases beds which contained palaeolithic remains are so situated that a tiro would never suppose that they had been deposited by running water at all. Few even of professed geologists would have thought of searching on the hill-tops at Caddington, near Dunstable; yet old stone implements have been found there in profusion. When the men who made them were alive the hills were valleys, and the valleys which now lie below the hills did not exist. Nor would it have occurred to any but a geologist that the tools which were espied lying at the foot of the cliffs between Reculver and Herne Bay had fallen from the gravels which line their summit.[2]

Inhabited caves.

Kent's Cavern and the Brixham Cave, near Torquay, the Wookey Hole 'Hyena Den', near Wells, the Long Hole Cave in Glamorganshire, and the caves of Creswell Crags, on the north-eastern border of Derbyshire, are perhaps the most famous of their class. Heaps of bones have been found in all of them, which proved that the men who, from time to time, inhabited them were contemporary, like those whose tools are recovered from the river-drift, with animals of which some, like the mammoth, the straight-tusked elephant, and the 'sabre-toothed' tiger, have disappeared from the face of the earth, and many have long been extinct in Britain. Generally in the lower strata the stone tools are exactly like those found in the river-drift; while in the higher they are as a rule more elaborately finished, and are associated with needles, harpoons, and other implements of bone. The same sequence is dis-

[1] See, however, *Guide to the Ant. of the Stone Age* (Brit. Museum), p. 3.

[2] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 613–7

cernible in the palaeolithic caves of France and Belgium.[1]

Cave implements and river-drift implements.

Let us compare in some museum the sets of tools and weapons which have been taken from caves with those of the river-drift. Are the latter older than the former, and is it possible to establish in either or in both a chronological succession of types? Taken by itself, the form of palaeolithic implements, at least in this country, is not generally a criterion of their age; but neither the forms of those that have come from the caves nor the bones which accompanied them forbid us to believe that the oldest are at least as old as any that belonged to the drift. Generally speaking, the fauna of the caves and of the river gravels are identical.[2] It is therefore certain that, although in general aspect a collection of implements derived from the former source is unlike one from the latter because the two were deposited in different circumstances, some of the deposits in the drift and in the caves were contemporaneous.[3] Since a few implements of river-drift form have been found in caves along with those of higher types, it seems reasonable to conclude that the same men possessed both; and if those which are characteristic of the caves are almost entirely absent from the drift, is not the explanation partly that they were more perishable, partly that many of them would not have been used in the field? In other words, there is no reason to believe that the later occupants of the caves were men of different race or of different habits from the contemporary hunters whose lost tools have been given up by the drift.[4] Long ago Monsieur de Mortillet framed a chronological classification of French and Belgian palaeolithic implements according to their types, which, though of late years it has been modified, has been provisionally accepted; but in this country it has been found impossible to follow his example: the same types exist

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 479, 488–525; *Phil. Trans.*, clxiii, 1874, pp. 553–70; *Quart. Journ. Geol. Soc.*, xxxii, 1876, pp. 240–58; xxxiii, 1877, pp. 579–612; xxxv, 1879, pp. 724–35.

[2] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 485.

[3] *Ib.*, pp. 474–5. [4] See pp. 383–5, *infra*.

here, but the relative antiquity of the specimens can seldom be determined; for implements of the oldest French types have been found in deposits which belong to the close of our Palaeolithic Age.[1] Even when implements from the high-level terraces are compared with those of the lower, no marked distinction is observed. In certain cases of course a local classification has been established. Thus the stone implements in the upper strata of two of the caves of Creswell Crags belonged to the advanced type which is called after the settlement of Solutré in the department of Saône-et-Loire;[2] and the implements of North-East London which, from their position at the bottom of the excavations as well as their colour, were evidently the oldest, were also inferior in workmanship to newer specimens found above them some twelve feet beneath the surface, and far inferior to the newest of the same district, which were recovered from an old land-surface, two or three feet below the existing ground, generally called the 'Palaeolithic Floor.'[3] Again, in the brick-fields of Caddington excavation revealed an ancient land-surface on which a palaeolithic colony had made their tools. At a later time a new surface about two feet higher was formed by brick-earth, which must have been swept down by heavy rains from the hills above; and on this more implements appeared. Above it again is a bed of contorted drift, containing implements whose deep ochreous colour would seem to show that of the three series they are the oldest: evidently they were washed down from the hill-tops on which perhaps lived the earliest inhabitants of the district, and which, as they were gradually worn away, formed

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 475, 483–5, 528, 530, 575–6; *Quart. Journ. Geol. Soc.*, lx, 1904, p. 132. In de Mortillet's classification the oldest type was the Chellean (called after Chelles in the department of Seine-et-Marne): then followed successively the types represented in the cave of Le Moustier, at Solutré, and in the cave of La Madelaine. Dr. M. Hoernes (*Der diluviale Mensch in Europa*, 1903, pp. 21, 63, 185–6, &c.) combines the Chellean and Mousterian periods. See also *Rev. mensuelle de l'École d'anthr.*, v, 1895, p. 407, and *L'Anthr.*, xv, 1904, pp. 27, 196–8).

[2] *Quart. Journ. Geol. Soc.*, xxxii, 1876, pp. 252–3; Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 522–3.

[3] Worthington G. Smith, *Man, the Primeval Savage*, pp. 215, 220.

a deposit in what were then valleys, but are now in their turn hills. The lowest implements, which were of course older than those next above them, belong to the type called after the cave of Le Moustier in the valley of the Vezère, which is itself later than the type associated with the high-level gravels of the Somme.[1] It has been suggested that when the evidence of plants or of strata is wanting, the relative age of palaeolithic implements may be provisionally estimated by the animal remains with which they are found. The straight-tusked elephant, the 'big-nosed' rhinoceros, and the hippopotamus were characteristic, we are told, of the earliest palaeolithic times;[2] the mammoth, the woolly rhinoceros, the cave-bear, and the hyena of a later period; and the reindeer was specially abundant towards the close of the age. But it is now generally recognized that if this orderly succession of fauna existed in Aquitaine, it cannot be distinguished either in our island or in Northern Gaul. When we find Arctic and tropical animals commingled, when we see that the bones of big-nosed rhinoceros and woolly rhinoceros, of straight-tusked elephant and hyena and reindeer have been dug out of the same beds,[3] we may conclude that it is hardly worth

[1] Worthington G. Smith, *Man, the Primeval Savage*, pp. 60–89, 96–175. Cf. *L'Anthr.*, xvi, 1905, p. 27.

[2] Prof. Boyd Dawkins (*Early Man in Britain*, p. 192) says that, except at Pont Newydd, 'the association of traces of man with the remains of hippopotamus has, as yet, not been observed in any bone caves either in this country or on the Continent'. Sir John Evans, who does not mention such remains in his notice of Pont Newydd (*Anc. Stone Implements*, 1897, p. 521), records their discovery, without associated implements, in the 'mid-terrace gravels' near Acton (*ib.*, p. 591), and in gravels of the same character as those which yield implements near Bedford (p. 533) and at Folkestone (p. 621). Evidently (pp. 699–700, with which cf. Boyd Dawkins and W. Ayshford, *Brit. Pleistocene Mammalia*, 1866, p. xxviii) he has no doubt that the hippopotamus was contemporary in Britain with palaeolithic man; but Mr. Clement Reid, in a conversation which I had with him on April 11, 1906, questioned whether its bones had ever been found together with implements.

[3] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 528, 533, 536, 591; M. Hoernes, *Der diluviale Mensch in Europa*, 1903, p. 13. Readers who are interested in the question which is raised by the discoveries of Arctic in association with tropical mammalian remains should consult *Quart. Journ. Geol. Soc.*, xxviii, 1872, pp. 426–43; xxxv, 1879, p. 142; W. Boyd Dawkins, *Early Man in Britain*, pp. 113–14; E. Piette, *La France préhist. par M. Cartailhac*, 1890,

while to gauge the antiquity of the works of palaeolithic craftsmen by such tests as these.

On a general review it should seem that the French chronological classifications of palaeolithic implements, even applied to England, contain a measure of truth. The implements which are commonly found in the river-drift and other deposits in the open field undoubtedly began to be manufactured before those which are characteristic of the caves; and those of Mousterian type were first made, both in England and in France, long before the development of the elegant Solutrean forms and the period in which flourished the artists of South-Western France.[1] But both in France and in England Mousterian implements were still used during the later period;[2] and even drift implements of the oldest kind continued to be used by palaeolithic hunters of the latest generation.[3]

Divers forms of tools.

In order to apprehend the culture of the palaeolithic races, it is necessary to be conversant with the forms of their tools. The great majority were made of flint; but in places where flint was scarce or difficult to obtain other stones, for example, chert, quartzite pebbles, sandstones, and felstone, were used. The principal forms were flint flakes, which were probably intended to serve as knives, sometimes even as saws (for a few of them are serrated),[4] and, in certain instances, as scrapers for dressing hides; implements or weapons, pear-shaped or tongue-shaped in outline, more or less acutely pointed, and more or less truncated at the butt, some of which look like spear-heads, though they may have been grasped in the hand; and oval, almond-shaped, and occasionally heart-shaped or

pp. 5–6; *Nat. Science*, i, 1892, p. 432; iii, 1893, pp. 262–3; Lord Avebury, *Prehist. Times*, 6th ed., 1900, p. 290; *Guide to the Ant. of the Stone Age* (Brit. Museum), pp. 36–7; and R. Lydekker, *Mostly Mammals*, p. 269. See also, in regard to the contrast between the intermingling of tropical and Arctic animals in Britain and Northern Gaul and their succession in South-Western Gaul, M. Hoernes, *op. cit.*, p. 193, and *L'Anthr.*, xiii, 1902, pp. 305, 317.

[1] *Ib.*, xv, 1904, pp. 57–8; xvi, 1905, p. 67. [2] See p. 384, *infra*.

[3] *Man*, vi, 1906, No. 63, p. 94. Chellean implements have been found at Le Moustier, evidently *in situ*, in the second layer from the top, among those of the Madelaine period.

[4] *Report of . . . the Brit. Association*, 1903, pp. 804–5.

triangular implements, which have a cutting edge all round. Each of these forms of course comprises many varieties, not only in contour but also in the mode of chipping; and a few tools of abnormal shapes have also been found, as well as natural blocks of flint, called 'hammer-stones', which were used in the process of manufacture, and most of which were slightly trimmed in order to make them more serviceable. Near Ipswich a lady has recently discovered a tiny implement which, it has been fondly suggested, some hunter may have wrought as a toy for his child.[1] Among the bone implements were harpoons, barbed sometimes on one, sometimes on both sides, which have been found in Kent's Cavern and other caves, and which closely resemble those that are used by the Eskimos of our own day; and needles drilled by bone awls, with eyes so small that the threads of reindeer sinew which they received could hardly have exceeded a thirtieth of an inch in diameter. Moreover, it is more than probable that clubs, wooden tools, and utensils and vessels of skin were also used, which, from their perishable nature, have long since disappeared.[2]

Palaeolithic workshops.

The explorations of antiquaries have revealed more than one of the open-air workshops in which the primitive toolmakers plied their trade. Near Crayford, on a sandy beach beneath an old chalk cliff that overhung the Thames when on its southern side its bed was nearly two miles wider, excavation discovered the surface, strewn with flint flakes, in actual contact with mammoths' bones, on which the workers had lived and toiled until a great flood drove them away, leaving the sediment which for countless ages concealed their remains. The inferior quality of the flint showed that they had not known how to win it by mining from the rock, but had been obliged to content themselves with such stray blocks as they could find. The enthusiast who discovered the site was actually able to fit

[1] *Report of . . . the Brit. Association*, 1904 (1905), p. 726; *Journ. Anthr. Inst.*, xxxiv, 1904, p. 308.

[2] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 504–7, 512–7, 523, 565–6, 581, 640–9, 655–6; Worthington G. Smith, *Man, the Primeval Savage*, pp. 110–11, 121, 248–9.

Fig. 1.

Fig. 2. ½

Fig. 3. ½

Fig. 4. ½

many of the flakes together, and to reconstruct the original blocks from which they had been struck off.[1] At Caddington, where hammer-stones and punches, great blocks of flint which had been used as anvils, and innumerable flakes and cores bore their silent testimony, Mr. Worthington Smith inferred from the confusion in which finished and unfinished tools were left that the settlers, terrified perhaps by some violent storm, had suddenly quitted their abode. He found an implement which had been ruined by an ill-directed blow of the hammer, and one which had been re-flaked and re-pointed by a later worker; and his practised eye detected that the craftsmen had flaked their tools differently from those of Crayford.[2] Speaking generally, however, the methods of working were the same as those which are still followed by the 'knappers' of Brandon in Suffolk, who manufacture gun-flints for African savages. The flakes which were to be used as knives or scrapers were detached from the blocks by a stone hammer; and the larger implements were trimmed into the various shapes which have been described, by blows along their edges, which chipped off small splinters. The effect of the hammer was to produce on the flake, just below the point where the blow was delivered, a protuberance, which is called the 'bulb of percussion', and which of course left a corresponding cavity on the block from which the flake was detached. This bulb is the mark by which a manufactured flint may be recognized; but on tools whose artificial origin is manifest even to an untrained eye it has often been obliterated by the process of chipping.[3]

Handles. Inquisitive antiquaries have raised the question whether any palaeolithic implements were furnished with handles. The Tasmanians simply grasped their tools in their hands;[4] and there is little evidence that the Britons mounted theirs:[5]

[1] *Archaeol. Journal*, xxxvii, 1880, pp. 294–9.

[2] Worthington G. Smith, *Man, the Primeval Savage*, pp. 113–4, 116, 142–3, 165.

[3] *Ib.*, pp. 262–7; *Vict. Hist. of . . . Hertford*, i, 224; Sir J. Evans, *Anc. Stone Implements*, 1897, p. 274. Cf. J. Prestwich, *Controverted Questions*, pp. 76–7.

[4] *Journ. Anthr. Inst.*, xxiii, 1894, p. 145.

[5] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 645, 655; Worthington

but the triangular sharply-pointed flints which have been already described might sometimes have been used as arrow-points or javelin-heads.[1] Some were doubtless missiles and nothing more.

Uses of tools.

But, as experts who have passed their leisure in recovering, comparing, and classifying these things confess, it is impossible to define the various purposes to which this or that stone tool was applied. 'Who,' says Lord Avebury,[2] 'could describe the exact use of a knife?' We only know that with his rude implements the palaeolithic hunter did all the work that his hand found to do,—felled trees, chopped wood to feed his fire, dug up esculent roots, scooped out canoes, killed and cut up the animals on which he subsisted, skinned them and dressed their hides to clothe himself withal, encountered his enemies in battle, and defended himself in conflict with the beasts against which his keen sight and hearing, his intellect, and these weapons, which it enabled him to fashion, were his sole protection.[3] Yet as we look at the tools in a museum, nearly the same at the end as at the beginning of our immeasurably long Palaeolithic Age, we marvel even more at the mental stagnation of the primeval savage than at the skill which he had laboriously attained; and we wonder how it was that men who had learned to chip their blocks of flint so accurately remained content, generation after generation, with the art which they had acquired, and never thought of grinding the cutting edge against another stone and thus producing a better and sharper weapon. 'We see in our own times,' wrote Sir Charles Lyell,[4] 'that the rate of progress in the arts and

Culture of the palaeolithic inhabitants of Britain.

G. Smith, *Man, the Primeval Savage*, p. 222; *Vict. Hist of . . . Hertford*, i. 224. Professor Boyd Dawkins (*Early Man in Britain*, pp. 183–4) affirms that certain implements found in the upper cave-earth of Church Hole and the Robin Hood Cave at Creswell Crags 'had obviously been let into a handle . . . by which the edge of one side had been protected, while the other was worn away by use'; and in *Nature* (May 22, 1902, p. 77) it is stated that a palaeolithic implement, recently discovered near Ipswich, 'shows signs of having been worked for hafting.'

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 645, 655.

[2] *Prehist. Times*, 1900, p. 332.

[3] See *Journ. Roy. United Service Inst.*, xii, 1868, pp. 408–9.

[4] *Antiquity of Man*, 4th ed., 1873, p. 422.

sciences proceeds in a geometrical ratio as knowledge increases; and so, when we carry back our retrospect into the past, we must be prepared to find the signs of retardation augmenting in a like geometrical ratio.' It would seem that in the Palaeolithic Age men had no pottery and grew no corn: they certainly had no cattle; and, though they lived by hunting, they had no dogs.[1] Perhaps they sometimes dug pits to trap their game; for one of the engravings from La Madelaine may have been intended to depict a beast impaled upon a wooden stake.[2] Their numbers must have been very small; for people who live by the chase alone require for their sustenance forests of vast extent.[3] Some, as we have seen, lived in caves; others, as we may infer from the remains that have been picked up beneath the cliffs of Oldbury,[4] by Sevenoaks, under projecting ledges of rock; generally perhaps, and especially in districts in which no caves were available, the dwellings were huts or shelters made of trees and boughs. Some of the bones that were found in Kent's Cavern, some even of the gravels that have yielded eoliths,[5] show traces of fire, which was probably produced by the friction of sticks or by striking flint against iron pyrites;[6] and one is tempted to infer that the hunters or their women learned to make their food more palatable by cooking. The numberless fractured

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 486, 657–8. Traces of corn have been found in French palaeolithic caves, though there is no evidence that it was cultivated. See *Congrès internat. d'anthr. et d'archéol. préhist.*, 1900 (1902), p. 408.

[2] E. Lartet and H. Christy, *Reliquiae Aquitanicae*, B. 1, pl. ii, fig. 5. Prof. Boyd Dawkins (*Early Man in Britain*, p. 214) has no doubt about the subject of the drawing: I confess that I am not so certain.

[3] 'It is doubtful,' says H. R. Schoolcraft (*Indian Tribes of the United States*, i, 1851, p. 433), 'whether an area of fifty thousand acres, left in the forest state, is more than sufficient to sustain by the chase a single hunter.' One may be allowed, however, to suspect an exaggeration in this estimate; otherwise how could the communities who dwelled at Caddington and Crayford (see pp. 39, 42–4, *supra*) have escaped starvation? See also A. Lang, *The Secret of the Totem*, 1905, pp. 6–7, 88–9, 151–2.

[4] See Mr. Lewis Abbott's paper in J. Salmon's *Guide to Sevenoaks*, 1905, pp. 120–1. Cf. *Archaeol. Journal*, xxxix, 1882, p. 17.

[5] *Life of Sir J. Prestwich*, 1899, p. 376.

[6] Sir J. Evans, *Anc. Stone Implements*, p. 501.

bones which were strewed in the caves had evidently been pounded for the sake of the marrow, which in every age was a dainty dish for prehistoric folk; and in the closing period, when harpoons had been invented, men were able to vary their diet of meat and herbs and wild fruit with divers kinds of fish. By that time too they had acquired the art of sewing, and doubtless they made themselves coats of skins, perhaps even, like the cave-dwellers of the Pyrenees, long gauntlets of fur;[1] while fossils that have been found with natural holes artificially enlarged may justify the assumption that, like the cave-dwellers of France, they adorned themselves with necklaces.[2] The figure of a horse engraved on a bone that was disinterred from one of the Creswell caves suggests, as we have seen, that in this country, as in France, there were men who were not destitute of the artistic faculty: but this solitary specimen can hardly compare with the best of the drawings that delighted the explorers of the contemporary French caves. It is difficult for any one who looks at these life-like sketches to believe that those who made them were not inspired by love of art; but the ingenuity of a modern archaeologist, who observes that the Australian aborigines scratch on rocks the likenesses of animals as charms to promote their fecundity, has suggested that they were merely talismans intended to supply the hunter with abundant game. As he insists[3] that the animals which the artists represented were all edible, one may fairly ask whether they were accustomed to feed upon the glutton,[4] the serpent, and the wolf;[5] whether they counted each other as legitimate prey; what could have been the utilitarian motive for depicting an otter chasing a fish;[6] and what was the object of engraving the

[1] W. Boyd Dawkins, *Early Man in Britain*, p. 211. I must admit that I feel doubtful whether the illustration in *Reliquiae Aquitanicae* which Professor Dawkins reproduces really represents gloves.

[2] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 657.

[3] *L'Anthr.*, xiv, 1903, pp. 258, 263–5; *Comptes rendus . . . de l'Acad. des Sciences*, 1903, pp. 1536–7.

[4] E. Lartet and H. Christy, *Reliquiae Aquitanicae*, p. 209.

[5] *L'Anthr.*, xv, 1904, p. 174.

[6] *Ib.*, v, 1894, p. 146.

strange quasi-human creature which the antiquary who discovered it in the cavern of Mas d'Azil described as an 'anthropomorphic ape, nearer akin to man than the anthropoids that we know'.[1] Nevertheless it is not improbable that religion, which has stimulated savage as well as mediaeval and modern art, may have been one of the motives of the cave-dwellers; and perhaps the artist was sometimes a magician, though it would be idle to speculate on the purpose of his spells.[2]

Disciplined imagination, working upon a basis of ascertained fact, may help one to picture the lives of those primitive inhabitants of our island. We can see them returning at evening to the fires which their women had kindled, and which served at once to warm them, to cook their food, to keep off beasts of prey, and to scare away the malignant spirits of whom, if they were like other savages, they were yet more in dread. We may see a vast herd of reindeer crossing the ford at Windsor, and wolves watching for their chance to spring upon stragglers. We may hear the trumpeting of the elephant, the roar of the lion, the bellowing of the wild bull, the howl of the hyena, the snort of the hippopotamus, as it splashed or swam in the waters of the Thames or the Ouse. We may imagine the hunter striving by sign, or gesture, or rudimentary language, to express his delight when he has succeeded in the chase, his despair when ill success leaves him and his to pine with hunger, his terror when the eclipsed moon turning to red, when flood, or lightning, or pestilence warns him that the spirits of nature are wroth, his grief when bear, or bison,

[1] *Bull. et mém. de la Soc. d'anthr.*, 5e sér., iii, 1902, p. 771. It is remarkable that Ezekiel (viii. 10–11), speaking of seventy of 'the ancients of the house of Israel' who were worshipping in a court, says that he saw therein 'every form of creeping things, and abominable beasts . . . pourtrayed upon the wall round about'. These were 'unclean' animals, which were *not* to be eaten. Cf. A. Lang, *Custom and Myth*, 2nd ed., 1885, p. 115.

M. Reinach also insists (*L'Anthr.*, xiv, 1903, pp. 264–5) that the so-called sceptres, or *bâtons de commandement*—engraved and perforated instruments of reindeer-horn—which have been found in French palaeolithic caves, were used in magical ceremonies; whereas it has been proved by Dr. O. Schoetensack (*ib.*, xii, 1901, pp. 140–4) that they were merely dress-fasteners similar to those which are used by the Eskimos.

[2] See A. Lang, *Custom and Myth*, 1885, pp. 294, 296.

or famished wolf has slain his wife or child. How he disposed of his dead he has left no sign: but in the caves near Mentone, which were inhabited in successive periods of the Palaeolithic Age, there were evidences that the corpses had been decently interred;[1] and the skeletons found in Moravia[2] had been carefully protected by a rampart of stones.[3]

Had the primitive people of Britain any religion, or any ideas that contained the germs of religious belief? It is not enough to point to modern savages like the Tasmanians, whose material culture was lower than that of the palaeolithic Britons, but who certainly believed in a spiritual world.[4] The cave-dwellers of Mentone were interred with their implements and ornaments, perhaps intended for use in a future state;[5] but such evidence is not forthcoming here. The painted pebbles, however, and the 'bull-roarers' which were treasured in the caves of South-Western France may well have had analogues among the inhabitants of this island[6] who were in the same stage of culture; and doubtless, like the similar objects which are shown by the natives of Central Australia, they were connected, more or less closely, with religious ideas.[7] No savage tribe, indeed, has yet been observed of whom it can be proved that they were without religion; for some travellers who have affirmed the contrary have been unable to comprehend ideas which differed wholly from their own; some have recorded facts

Religion.

[1] *L'Anthr.*, xvii, 1906, p. 293.

[2] See p. 34, *supra*.

[3] *L'Anthr.*, xvi, 1905, p. 395. Cf. p. 321 of the same volume, and vol. iv, 1893, p. 550.

[4] *Journ. Anthr. Inst.*, xxi, 1892, p. 297; xxiii, 1894, pp. 147, 151.

[5] *L'Anthr.*, xvii, 1906, pp. 293–4. The Tasmanians 'placed weapons near the grave for the dead friend's soul to use' (E. B. Tylor in *Ency. Brit.*, xxv, 1902, p. 467). Cf. pp. 200–2, *infra*.

[6] See pp. 262–3, 464, *infra*.

[7] Mr. Andrew Lang (*Man*, iv, 1904, No. 22, p. 37), remarking that in the cave of Mas d'Azil, in the department of the Ariège, there has been found a pendeloque of bone which exactly resembles some Australian 'bull-roarers' (*L'Anthr.*, xiv, 1903, pp. 655–60), infers that 'palaeolithic and neolithic man . . . probably had such religious ideas as among savages are attached to bull-roarers'. There is an interesting chapter on bull-roarers (which in this country are more familiar to schoolboys than to scholars) in Mr. Lang's *Custom and Myth*, 1885, pp. 29–44.

which gave the lie to their own denial; some have confessed that after long intercourse they had discovered the existence of beliefs which they had never suspected; and all who have been qualified by tact and sympathy to deal with savages have recognized how hard it is to induce them to disclose their inmost thoughts.[1] But much depends upon the sense in which the word Religion is to be understood. The great anthropologist whose writings have given the most powerful impetus to the study of primitive culture has taken as his 'minimum definition of religion' the belief in spiritual beings;[2] and although it might be rash to affirm that materialism is inconsistent with religion, and no sympathetic reader would deny that the Latin poet who denounced 'foul religion' with such fierce earnestness had a religion of his own, Professor Tylor's words may serve as our guide.[3] It is true that the conception of a spiritual being formed by a primitive mind has hardly anything in common with that approved by a theologian or a philosopher: for the savage, as for Tertullian and Origen, spirits are not immaterial; they are exceedingly subtle, but still corporeal. Nor, indeed, are they necessarily immortal. Savage religion is utterly different from that which has been the guide of life to men who, though they had put away all hope of everlasting life, retained their sense of the nobility of human nature,—'to visit the fatherless and widows in their affliction, and to keep himself unspotted from the world'; utterly different from that which inspires the idealist to whom theology is a vain thing and the supernatural unreal, but who clings to his belief that man's punishment or reward hereafter is simply to be what he has become, that his destiny is to grow in grace, lapsing perhaps, but again aspiring, until his spirit becomes one with the indwelling spirit of God. Yet, although the orthodox may refuse the name of religion to an animism begotten of fear and unconnected with ethics, though idealists may scoff at the conception of spiritual beings which invests them with bodily form and ponderable and mortal albeit ethereal substance, that

[1] See E. B. Tylor, *Prim. Culture*, 4th ed., 1903, i, 417–24.
[2] *Ib.*, p. 424. [3] See pp. 461–3, *infra*.

animism was the seed out of which their own faith—its framework but not its nobler part—was evolved.

He whose mind is informed by the teaching of ethnography may conceive, if he has a sympathetic imagination, the mental state that gave birth to primitive religion; but if his reading has not been wasted, he will understand how vain would be the attempt to ascribe to this or that prehistoric people any known savage creed. For, alike in origin and in essence, the forms of modern animism are manifold. To the palaeolithic Briton fire, leaping roaring and devouring, devastating flood, rushing wind, lightning flash, disease, death itself,—all may have been animated by spirit, or have been themselves spiritual beings. Elves, goblins, phantoms may have been created by his brain, and have seemed to flit before him when prolonged fasting had stimulated the creative power of his fancy. The conceptions that were ultimately to become the greater gods of polytheism may have arisen in his mind as in the minds of other savage men. At least we may believe that, unless he differed greatly from the modern savages whose handiwork resembles his, he began to people the universe with spiritual beings when he became conscious of his own soul; that the phantasms which he saw in dreams were for him real and alive; that every spirit in which he believed originated in the curiosity that led him to seek the cause of every natural phenomenon; that, although social friction had compelled him to recognize a moral code, his religion and his morality were not one but two, not mutually supporting but distinct; and, finally, that no thought of future retribution or reward troubled or comforted his heart.

Totemism.

Intimately connected with primitive religion is totemism, that strange institution which has been observed in various stages of survival among the North American Indians, the forest tribes of South America, the aboriginals of Western and Central Australia, the Malays, the hill-tribes of Central India, certain Mongoloid tribes of Central Asia, in Bechuanaland, and in the Bantu district of South Africa;[1] which

[1] It is said that totemism exists in New Guinea (*Man*, v. 1905, No. 2) and on the Gold Coast (*Journ. Anthr. Inst.*, xxxvi, 1906, pp. 178–88).

in every case began before those whom it affected had come to domesticate animals, to till the earth, or to fashion pottery;[1] and which tends to decay when hunting gives place to pasturage.[2] One cannot but inquire whether an institution so widespread existed among the prehistoric inhabitants of Britain; and, although no distinct case of totemism has been found or recorded in Europe,[3] the inquiry is not perhaps so hopeless as it may at first sight appear.

The leading principles of totemism have been so often defined that they are doubtless familiar to many readers. Evidently it originated at a time when men were not possessed by the fancy that they were a distinct branch of creation, but felt their kinship with other animals, which they had hardly begun to regard as inferior.[4] The members of the clans which form a totemic tribe trace their descent generally from some animal, sometimes even from a plant or an object which we should call inanimate, and bear its name. But how did the conception of relationship between a clan and an animal or vegetable species arise? It has been suggested that metempsychosis may supply the explanation. Some great man perhaps gave out that after his death no hare was to be eaten by his clan because a hare would be possessed by his soul. Thus not only his own children and grandchildren but also hares would be his descendants; and he would be the founder of a totem-family, which might develop into a totem-clan.[5] On the other hand, it has been argued that when totemism began descent was necessarily reckoned in the female line, and that it is therefore useless to search for its origin in anything—for example, 'a paternal soul tenanting an animal'—which was deemed to be inherited from a male ancestor.[6]

[1] See A. Lang, *The Secret of the Totem*, pp. 2, 66.

[2] See W. Robertson Smith, *The Religion of the Semites*, 1901, p. 355.

[3] E. B. Tylor, *Prim. Culture*, ii, 1903, p. 237.

[4] See *Journ. Anthr. Inst.*, xxviii, 1899, p. 146.

[5] *Ib.*, p. 147.

[6] A. Lang, *The Secret of the Totem*, pp. 22–3. I confess that I cannot understand why descent should have been reckoned in the female line if, as Mr. Lang apparently holds, the master of each little primitive group was the only sire in that group. [I am glad to find that Dr. W. H. D. Rouse (*Folk-Lore*, xvii, 1906, p. 25) has argued in the same sense.]

Until a recent date it was an article of faith among anthropologists that, except in special circumstances, the life of a totem-animal was, in the eyes of the clan which belonged to it, sacred, and that marriage between the members of any one clan was absolutely tabooed. If a clansman of a Crocodile clan desired a wife, he must seek her from a Wolf clan or from some other. But within the last few years totemism has been carefully and minutely observed among the Arunta tribe of Central Australia; and the records of these observations mark a new era in anthropology. With the Aruntas totemism does not forbid the slaughter of the totem-animal and does not prescribe exogamy: it is based upon the belief that they are descended from 'quasi-human animal or vegetable ancestors, whose souls are still reborn in human form in successive generations'.[1] It has, however, been maintained that in the organization of this tribe there are still discernible traces of totemism of the primitive type, involving both exogamy and respect for the life of the totem-animal;[2] and also that their totemism is so decadent that nothing can be learned from it as to totemic origins.[3]

Totemism is indeed a subject of extraordinary difficulty: its literature is enormous and rapidly growing; and it is out of the question in this book to do more than point out its problems, and put the reader in the way of pursuing the study for himself. The problem of its origin can never be solved with certainty; for the institution cannot now be observed in its primitive state; and any attempt to trace it backward must start from conjecture as to the original social condition of man.[4] Perhaps the most plausible and

[1] E. B. Tylor, *Prim. Culture*, ii, 1903, p. 236; B. Spencer and F. J. Gillen, *Native Tribes of Central Australia*, 1899, pp. 73, 121; *Man*, iv, 1904, No. 93, p. 143. See also No. 98, p. 150.

[2] *L'Anthr.*, xiii, 1902, pp. 665–7.

[3] A. Lang, *The Secret of the Totem*, pp. 59–89, especially 66, 68, 70, 72–4, 89. See also pp. 7–8 in regard to the complex organization of Australian tribes. The Aruntas (*ib.*, pp. 17–18) do not inherit their totems, which are 'determined by *local* accident'.

[4] *Ib.*, p. 29. Mr. Lang conjectures (p. 114) that the master of a small group, actuated by sexual jealousy, 'expelled all his adult sons as they came to puberty.' Such a group, he remarks, would have been 'necessarily exogamous in prac-

ingenious theory rests upon the assumption, for which considerable evidence has been adduced, that groups of men originally designated one another by animal and plant names, and that these names were accepted even when they were bestowed in derision. Such a group, finding itself called, let us say, by the name of the pig, and not knowing how it had come by the name, would naturally believe that there was an intimate connexion between itself and the porcine species.[1] The taboos which forbade the slaughter of the totem-animal and marriage between a man and a maiden of the same kin would, it is argued, follow when once the universal belief, that 'the blood is the life' and therefore sacred, was evolved.[2]

There are superstitions and names which suggest that totemism may once have existed in Britain; but even if their evidence is accepted, it is of course impossible to point out the source from which they were ultimately derived. They may have belonged to our early Neolithic Age, or they may have been introduced later, when totemism had died out, by invaders who had received them from inferior tribes with whom they came in contact. We are assured that Cornish fishermen believe that drowning men sometimes assume the form of animals;[3] that in the village of Burchurch in Shropshire it is deemed unlucky to kill a bat;[4] that at Great Crosby in Lancashire the goose is held sacred;[5]

tice', and then (*ib.*, p. 143) would have come the rule, 'No marriage within the local group.' But would it have been to the interest of the master to expel sons who were useful? What would have become of them? Would not the same sexual jealousy that *ex hypothesi* prompted their expulsion have prevented the master of any other group from receiving them? And if the master was killed in hunting after he had expelled his sons, what became of the other members of the group?

In connexion with Mr. Lang's book, see *Man*, vi, 1906, No. 17, pp. 27-8, No. 34, pp. 51–4, No. 87, p. 131, and No. 112, p. 182.

[1] A. Lang, *The Secret of the Totem*, pp. 116, 127-8, 153. Cf. Lord Avebury's *Origin of Civilisation*, 1902, p. 275.

[2] A. Lang, *The Secret of the Totem*, p. 125.

[3] *Archaeol. Rev.*, iii, 1889, p. 220.

[4] *Ib.*, p. 227.

[5] *Notes and Queries*, 3rd ser., iv, 1863, pp. 82, 158. These passages, which are referred to by Mr. Gomme in vol. iii of *Archaeol. Rev.*, do not support the statement in the text about the geese of Great Crosby, for which he is responsible.

and that certain Scottish clans derived their names from animals.[1] The familiar passage in which Caesar observes that the Britons counted it impious to taste the flesh of hares, fowls, and geese[2] has also been interpreted as a survival of totemism.[3] But this is a mere guess. The greatest of anthropologists has warned us not to assume that every sacred animal is a totem:[4] the association with a clan of a species of animals is only one form of animal-worship. It is, however, quite possible that if these animals had once been totems, they were revered by clans with whom the ancestors of the British Celts had mixed before they emigrated from Gaul; for broken bones of the hare, which were found in one of the caves of Perthi-Chwareu in Denbighshire, show that at all events in that part of neolithic Britain the animal was eaten.[5]

Was the domestication of animals a

Some anthropologists have argued that the domestication of animals and even agriculture resulted from totemism.[6] Thus Monsieur Reinach insists that the domestication of

[1] *Archaeol. Rev.*, iii, 1889, p. 355. [2] *B. G.*, v, 12, § 6.

[3] M. S. Reinach's explanation of this passage (*Rev. celt.*, xxi, 1900, p. 275) was anticipated by Elton (*Origins of English Hist.*, 2nd ed., 1890, p. 288).

[4] *Journ. Anthr. Inst.*, xxviii, 1899, pp. 141, 143–4, 148. M. Reinach seems to make this assumption when he says (see preceding note) that we are justified in affirming that 'chez certaines tribus au moins de la Bretagne, le lièvre, l'oie et la poule étaient des animaux sacrés, c'est-à-dire, des totems'. I am glad to find that M. Camille Jullian (*Rev. des études anc.*, iv, 1902, p. 274) also rejects M. Reinach's guess; but he continues, 'Je suis, du reste, convaincu, avec M. Reinach, que les Celtes ont connu le totémisme . . . par exemple, si *bran(n)os* signifie . . . "le corbeau", une tribu gauloise avait pris cet oiseau pour totem . . . *Aulerci Brannovices*,' &c. On the much surer evidence of such names as Bull, Lamb, Herring, Roach, and many others, M. Jullian might conclude that 'les Anglais du vingtième siècle connaissent le totémisme'. It is perhaps reasonable to conjecture that the name *Brannovices* may point to a remote age when the ancestors of the historic Celts had totems: but it is quite certain that the Celts of whom M. Jullian is thinking knew nothing about totemism; and the superstitions which forbade the Britons to eat hares, geese, and fowls, may have been absolutely unconnected with totemism. See Lord Avebury's *Origin of Civilisation*, 1902, p. 19. Miss Eleanor Hull (*Folk-Lore*, xii, 1901, p. 49) observes that 'there is one example of what appears to be a true totemistic idea in those [Irish] stories. . . . It is in Cúchulainn's prohibition to eat the flesh of a hound because it was his namesake.'

[5] W. Boyd Dawkins, *Cave-Hunting*, p. 165.

[6] Notably Dr. F. B. Jevons (*Folk-Lore*, x, 1899, pp. 374–5) and M. S. Reinach (*Rev. celt.*, xxi, 1900, pp. 283, 299, 305).

result of totemism?

the boar is an irrefragable proof of its former sanctity; for, he argues, if men had always thought themselves entitled to kill and eat boars, boars would never have multiplied under human protection, and become the ancestors of domestic swine. Domestication, he considers, implies a long truce between men and animals, something analogous to the Golden Age, celebrated by poets of antiquity, in which men were vegetarians. One may be pardoned for maintaining a sceptical attitude towards a theory which is obviously incapable of proof, which to men who live remote from libraries but in the midst of animals presents insuperable difficulties, and which, moreover, seems to imply that prehistoric tribes were excessively stupid. If it were true, one would expect to find that oxen, sheep, and pigs had been reared in the Palaeolithic Age, and that modern totem groups had domesticated or were now domesticating totem animals. But the only animal which the cave-dwellers of South-Western France apparently domesticated was the horse, which was doubtless lassoed and fastened not because it was sacred but for food;[1] and the Aruntas have no domestic animals. A hungry Australian would have no scruple in killing and eating an animal, not belonging to his own totem-species, which by his wife would be deemed sacred: the Bantus have sheep and oxen, but neither the ox nor the sheep is among their totems. What motive could savages have had for keeping totem-animals in captivity in large numbers unless they had desired to eat their flesh or to drink their milk, and why should they have toiled to provide food for them in winter? Why should the domestication of any species be impossible unless the lives of the animals were spared for a long term of years; and why, if every bull and ram were suffered to gratify its sexual instincts unchecked, and cows and ewes were

[1] See W. Ridgeway, *Origin . . . of the Thoroughbred Horse*, 1905, pp. 90–1, 479, and *L'Anthr.*, xvii, 1906, pp. 27–53, especially 27, 29, and figs. 1 and 1*a*. If these illustrations, which purport to reproduce late palaeolithic engravings of horses, are accurate, they unquestionably depict halters, though M. Zaborowski (*Association franç. pour l'avancement des sc.*, 32e sess., 1903, 2e partie, p. 849) thinks that they only represent lassoes.

unmilked and unused, should they become tame.[1] It is surely not incredible that primitive hunters, not belonging to Bull or Boar clans, who saw that wild oxen and wild boars were good for food, should have conceived the idea of ensuring a more constant supply by trapping young animals, taming them, and breeding from them. Totemism may conceivably have had some influence upon the domestication of animals; but it seems probable that there was room for common sense.[2] And the mere fact that a piece of sculpture representing an ear of barley was found in a cave at Lourdes hardly seems sufficient to justify the conclusion that barley was an object of worship in the Palaeolithic Age, and that its subsequent cultivation was due to totemism.[3] What we may safely conclude is that exogamy, with which totemism is commonly associated, although they may have been originally distinct, was one of the chief factors in consolidating groups and allying them together.[4]

Magic.

The subject of totemism naturally leads on to that of magic; for in Australia totemic groups have developed into co-operative magic-working societies; and there is no rashness in assuming that magic flourished everywhere before the end of the Palaeolithic Age. We are often told that magic was based upon a confused association of ideas; that it was the embryo of science;[5] and that priest and magician have ever been foes. There is much truth in this: but magic is not to be so easily explained; and

[1] See a very interesting review [by Mr. Andrew Lang?] in the *Athenæum*, April 22, 1905, pp. 502–3, of M. Reinach's *Cultes, mythes et religions*, and also papers on the domestication of animals in the numbers for April 29 (p. 533), May 6 (p. 565), and May 13 (p. 597).

[2] It is impossible to tell whether in Ancient Britain oxen were at any time regarded as sacred, as they apparently were among the early Phoenicians, the Libyans, the ancestors of the Greeks, and other primitive peoples, their flesh being never eaten except in sacrificial feasts, partaken of by the whole clan. See W. Robertson Smith, *The Religion of the Semites*, 1901, pp. 296–311.

[3] *L'Anthr.*, xiv, 1903, pp. 355–7. Cf. *Congrès internat. d'anthr. et d'archéol. préhist.*, 1900 (1902), pp. 408–9.

[4] *Journ. Anthr. Inst.*, xxviii, 1899, p. 148. Cf. *Man*, v, 1905, No. 2, p. 6.

[5] *Chambers's Ency.*, vi, 1901, p. 795. M. S. Reinach (*L'Anthr.* xvi, 1905, p. 660) regards magic as 'la mère de toutes les vraies sciences'.

most of us are still far from sympathetically understanding the mental state in which it originated. To say that one kind of magic is an outgrowth of the law of similarity, the magician fancying, for example, that by making drawings of animals he can cause their species to multiply; that the other depends upon the law of contact, when, for instance, it is supposed that whatever is done to a weapon will correspondingly affect the person whom it wounded,[1]—to say this is not to fathom the magician's mind. Magic, notwithstanding the hostility with which priests have regarded magicians, cannot be separated from religion by a line of demarcation; nor indeed is it always possible to differentiate magicians from priests.[2] It has been well said that magic, as observed among primitive tribes, is 'part and parcel of the "god-stuff" out of which religion fashions itself'.[3] Australian magicians believe that their powers are conferred upon them by supernatural beings;[4] and the magicians of many tribes call upon spirits to aid them in working their spells.[5] One of the most important functions of the magician is to ensure an adequate fall of rain; but in New Guinea this duty belongs to the priest of the god by whose favour the rain is believed to fall.[6] Vast learning has been expended to prove that monarchy originated in magic;[7] but we only know that magicians have sometimes succeeded in making themselves kings;[8] and doubtless in certain cases magic may have helped to sow the seed out of which gradations of rank were evolved.[9] But this would be but one more illustration of the accepted truth that family, tribe, priesthood, monarchy—all our institutions—are rooted in savagery.[10]

[1] J. G. Frazer, *Early Hist. of the Kingship*, 1905, pp. 37–9, 43–4, 77–8, &c.

[2] See *Man*, vi, 1906, No. 40, p. 62, No. 112, p. 189 (for a criticism of Dr. Frazer's 'oil-and-water theory' of magic and religion), and Mr. Sidney Hartland's most interesting presidential address to the Anthropological Section of the British Association (*Times*, Aug. 7, 1906, p. 11, cols. 4–6).

[3] *Folk-Lore*, xv, 1904, pp. 159–60.

[4] *L'Anthr.*, xvi, 1905, pp. 574–5.

[5] *Ib.*, p. 660.

[6] *Man*, v, 1905, No. 10, pp. 18–19.

[7] J. G. Frazer, *Early Hist. of the Kingship, passim.*

[8] See *Man*, vi, 1906, No. 29, p. 46.

[9] See A. Lang, *Custom and Myth*, 1885, p. 237.

[10] *Ib.*, p. 242; J. G. Frazer, *Early Hist. of the Kingship*, pp. 2–3, 36–7.

Was there a 'hiatus' between the Palaeolithic and the Neolithic Age?

The close of the British Palaeolithic Age is veiled in obscurity. 'Mesolithic' implements, whose form might show that they belonged to a period of transition between the Palaeolithic and the Neolithic Age, have been diligently sought for; and some of the seekers insist that they have found them:[1] but the claim has not won general acceptance; and even if it could be established, a doubt would remain whether the makers of those implements belonged to the palaeolithic race of Britain or to a race which had come from abroad after our Palaeolithic Age had passed away. In the words of a high authority[2] 'there appears, in this country at all events, to be a complete gap between the River-drift and Surface Stone Periods, so far as any intermediate forms of implements are concerned; and here at least the race of men who fabricated the oldest of the palaeolithic implements may have, and in all probability had, disappeared at an epoch remote from that when the country was again occupied by those who not only chipped but polished their tools.' It has been urged by those who would extend this characteristically guarded conclusion that out of forty-eight mammalian species which were living in Britain in the older, only thirty-one survived into the later period; that Britain was united with the Continent in the former, and was an island in the latter; and that in caves which were inhabited in both periods the strata that contained palaeolithic remains were separated by a layer of stalagmite, the formation of which would have required many centuries, from the upper neolithic stratum. But all these arguments do not prove that there was a breach of continuity between the two ages. If seventeen mammalian species perished, thirty-one did survive. If Britain was continental in the Palaeolithic Age and insular in the Neolithic, the contrast does not exclude the possibility that man survived with his fellow animals from the former into the latter: at the time when the Hoxne implements were lost the land stood only a few feet above its present level,[3] and a strait must have separated Britain from Gaul;

[1] See pp. 387–8, *infra*. [2] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 704.
[3] See p. 20, *supra*.

nor, on the other hand, is it absolutely certain that the earliest neolithic immigrants did not cross the Channel valley on foot. And if the stalagmite which lay between palaeolithic and neolithic implements proved that in certain caves the stage of culture represented by the lower strata was separated by a vast gulf of time from that represented by the higher, it still remains possible that some descendants of the primitive hunters may have survived to meet the neolithic invaders. Whoever maintains that there was a 'hiatus' between the two stone ages in Britain must frame some theory to account for the disappearance of the palaeolithic race. Either they must have been utterly destroyed by some cataclysm which could hardly have been less fatal to the thirty-one mammalian species that survived; or they must have been struck down by a pestilence, such as has never been recorded, that spared none; or they must have died out, although there was no civilized race to expedite their fate; or they must one and all have emigrated for some reason which cannot be explained. It is true that in the valley of the Lea near London and at Caddington the old land-surface on which they lived is covered by 'contorted drift', above which no undisturbed palaeolithic relics have been found; and it has been supposed that the cold to which the formation of this deposit was due forced the inhabitants to migrate southward. But this evidence has not been taken seriously; and it has also been suggested that the emigration, if it took place, was caused by an outbreak of disease, which, if it was real, may have been merely local. Again, it has been asserted by the most persistent advocate of discontinuity that the 'cave men' fled in terror before neolithic persecutors;[1] that their line of retreat is indicated by implements in the caves of Germany and in refuse heaps of Siberia; and that the extinction of certain mammals and the flight of others was due to the change of climate which resulted from the new-born insularity of Britain.[2] But if the cave-men were driven away by neolithic invaders, what becomes of the alleged hiatus? why

[1] See p. 398, *infra*.

[2] *Vict. Hist. of . . . Somerset*, i, 178.

should implements in Germany and Siberia be connected with British fugitives? and if mammals abandoned Britain because it had become an island, how did they get away? Somewhere or other the newer was evolved from the older culture: the palaeolithic skeletons which have been found in the caves near Mentone are not distinguishable from those of the same Ligurian coast which were interred in the Neolithic Age;[1] and evidence from stratified deposits in the valley of the Seine, lying one above another in unbroken succession, as well as the remarkable discoveries at Mas d'Azil and in the Riviera, have convinced the anthropologists of France that in their country a hiatus did not exist.[2] Therefore those of us who cling to the belief that the neolithic immigrants who first ventured to launch their frail canoes on the narrow Channel and ran them aground on the Kentish coast may have found the new-born island inhabited by men of an older race have some reason to show for our pious faith.[3]

[1] See pp. 382, 389, n. 6, *infra*.

[2] See p. 389, *infra*. M. L. Siret (*L'Anthr.*, xvii, 1906, p. 127) says the same for Spain.

[3] See pp. 385–90, *infra*. 'The... transition,' says Mr. Clement Reid (*Origin of the Brit. Flora*, p. 45), 'from the Palaeolithic to the Neolithic is, unfortunately, one of the most obscure, and I can only suggest that the break is more apparent than real, and that one follows the other in close succession.' See also p. 93 of the same book.

CHAPTER III

THE NEOLITHIC AGE

The early neolithic immigrants.

No one can say how long after the close of the Ice Age the first neolithic immigrants appeared;[1] nor can it even be positively affirmed that in Northern Britain the last glacier had then melted away. If they sailed across the Dover Strait, it was, as we have seen, extremely narrow; and we can hardly be sure that it existed at all.[2] Neolithic hunters, who may not have belonged to the earliest horde, roamed in forests which now lie buried beneath the Bristol Channel and the waves that break upon the Land's End;[3] and from the depths at which their remains have been dug up it may be reasonably inferred that Southern Britain then extended at least as far as the line which is marked upon our maps and charts by the ten-fathom contour. But while in England the land stood above the modern level, in Scotland it lay below; for along the margin of the fifty-foot raised beach there are heaps of refuse left by men who lived at a time when the estuary of the Forth ran up to Falkirk, and the lands which form the Carse of Stirling were submerged:[4] dug-out canoes have been found embedded in the basin of the Clyde more than twenty feet above the present high-water mark;[5] and in a cave which was discovered by quarrymen in a cliff facing the bay of Oban, a hundred yards from the existing beach, dwelled hunters

[1] See Mr. Clement Reid's chapter in *Vict. Hist. of . . . Hants*, i, 35–6.

[2] See A. J. Jukes-Browne, *The Building of the Brit. Isles*, p. 300. Mr. Clement Reid (*Origin of the Brit. Flora*, p. 46) states that in the early part of the Neolithic Age 'the land stood . . . some 60 or 70 feet above its present level'. Cf. p. 20, *supra*.

[3] *Journ. Ethn. Soc.*, N. S., ii, 1870, pp. 141–5; J. Prestwich, *Geology*, ii, 523–4; A. J. Jukes-Browne, *The Building of the Brit. Isles*, pp. 300–2; Clement Reid, *Origin of the Brit. Flora*, p. 46.

[4] *Nature*, Jan. 6, 1898, p. 235; *Archaeol. Journal*, lv, 1898, p. 271.

[5] *Ib.*, p. 272.

and fishermen, whose mode of life is attested by their deer-horn harpoons, the remains of the oxen and deer on which they partly subsisted, and the bone pins with which they fastened their clothing.[1] The character of the relics has led experts to the conclusion that the people to whom they belonged were among the earliest of the neolithic inhabitants of Western Europe ; indeed it may be that they were descendants of a British or a Pyrenaean palaeolithic stock. The harpoons are of the same type as those which in the caves of South-Western France are assigned to the close of the Palaeolithic Age and to a time of transition between it and the following epoch, and which in recognized neolithic deposits have never been found either in Britain or in Gaul ; and the general aspect of the Scottish and the Gaulish remains is virtually the same.[2] There are, moreover, other indications that the British Neolithic Age began long before the period to which the great majority of the antiquities that lie in our museums belong. A few years ago there were brought to light traces of a settlement which some primitive clan had formed on the bank of a stream that flows through Blashenwell Farm, hard by Corfe Castle. These settlers had lived in great part upon limpets, which they must have eaten raw, since the broken shells showed no trace of fire : they did not till the soil ; they had no domestic animals and no pottery ; and their tools were of the rudest kind.[3] Moreover, besides the implements that lay beneath the submerged forests, there have been found in the bed of the Trent, and in the Ham Marshes, thirty feet below the surface, skulls which are so far different from those that have been recovered from barrows and cairns as to suggest that the oldest neolithic invaders may have belonged to another stock.[4]

But whoever they may have been, whatever the date The origins

[1] *Archaeol. Journal*, lv, 1898, p. 270; *Proc. Soc. Ant. Scot.*, xxix, 1895, pp. 223–7, 431–2, 438; R. Munro, *Prehist. Problems*, 1897, p. 72.

[2] See *L'Anthr.*, vii, 1896, pp. 319–24, and M. Hoernes, *Der diluviale Mensch in Europa*, p. 185.

[3] *Proc. Dorset Nat. Hist. and Ant. Field Club*, xvii, 1896, pp. 67–75.

[4] See pp. 395–7, *infra*.

of British civilization were neolithic.

of their arrival, it was an era since which the history of this country has been continuous. Their descendants are with us still: they or later comers brought with them the seeds of cereals and plants which are cultivated still, and animals the descendants of which still stock our farms; they practised handicrafts and arts from which the industries of modern Britain have been in part evolved.[1]

Geography of neolithic Britain.

The subsidence which is proved by the submerged forests was going on throughout the Neolithic Age, and only ceased about three thousand years ago. While the forests were insensibly sinking, the valleys that stretched behind them were flooded by the advancing sea, which penetrated through the chalk downs into the Weald in long fiords, and doubtless often carried the canoes of the later invaders.[2] But we cannot fix even approximately the period at which these people began to arrive.[3] All that can be said is that it was many centuries before the Bronze Age, which probably began in this country about eighteen hundred years before the Christian era.[4]

Who were the later neolithic invaders?

These hordes doubtless set out from various parts of northern Gaul; but to determine their origin is perhaps impossible.[5] The skeletons that have been exhumed from the neolithic tombs of England, Scotland, and Ireland, except some which were interred in the very latest period, when invaders of a widely different race were beginning to arrive, belong, for the most part, to the same general type. All, or almost all, had long narrow skulls: their faces were commonly oval, their features regular, and their noses aquiline: most of them were of middle height, and their limbs, as a rule, were rather delicate than robust. Men

[1] See *Vict. Hist. of . . . Hants*, i, 256.

[2] *Ib.*, p. 37; *Vict. Hist. of . . . Sussex*, i, 25.

[3] See *Vict. Hist. of . . . Hertford*, i, 229, and *Archaeol. Journal*, lv, 1898, p. 285. Dr. A. H. Keane's extravagant estimates of the length of the Neolithic Age in Europe, which vary between the limits of 'scarcely less than 60,000 years' (*Ethnology*, 2nd ed., 1896, p. 55) and 'over 100,000 years' (*ib.*, p. 116), are based upon obsolete calculations of the chronology of the Glacial Period. See pp. 31-2, *supra*.

[4] See pp. 126-7, *infra*.

[5] See *Rev. de l'École d'anthr.*, xv, 1905, pp. 408-14, especially p. 412.

with the same physical characters lived contemporaneously in Gaul and the Spanish peninsula, and are still numerous in the basin of the Mediterranean; and the race to which they belonged is often called the Iberian, though there is no reason to believe that its British representatives belonged to the Iberian rather than to some other branch of the Mediterranean stock.[1] But it is remarkable that while early in the Neolithic Age Gaul and Spain, as well as Central Europe, were overrun by invaders of a totally different kind, who were extremely short and sturdy and had broad round heads, there is no evidence that men of this race reached Britain until the very end of the period, and then only in comparatively small numbers.[2] One would be inclined to infer that tribes of the Mediterranean stock began to migrate into Britain before many of the round-headed race had settled in Gaul. Vain attempts have been made to trace the migration to its original starting-point by the distribution of the dolmens, or rude stone sepulchres,[3] which are found in many European countries. A dolmen, in the strict sense of the word, is composed of large stones set on end, which wholly or partially enclose a space, and are covered by other stones or by a single stone, which rests upon their upper ends. Most of the chambers in our chambered barrows virtually answer to this definition; and if the enclosing mounds were removed, would appear as dolmens.[4] Some few, however, as well as chambers which have been explored in Brittany, were roofed over, like the so-called beehive huts, by layers of stones, which, as they rose, gradually approached each other, the highest supporting a flat slab whose weight kept them in place, while the pressure of the superincumbent

Evidence from dolmens.

[1] See pp. 398–407, *infra*.

[2] See pp. 427–8, 433, 443, *infra*.

[3] This use of the word 'dolmen', which obtains in France, although megalithic chambers enclosed in tumuli are there sometimes called by the same name (*Archaeol. Cambr.*, 5th ser., xvii, 1900, p. 221), is becoming common in this country; but in Wales dolmens are still known as cromlechs, a name which in France is applied only to stone circles.

[4] See *Archaeologia*, xlii, 1869, p. 214. The kistvaens of Dartmoor are really small dolmens.

cairn or barrow gave solidity to the whole.[1] But although the dolmens which are generally so called may be older than the chambered barrows,[2] they also were almost always covered or at least fenced by earthen mounds or cairns, which, in many cases, were still visible little more than a century ago.[3] There is no reason to suppose that in this country or in Ireland they were built by tribes of a different stock: it is impossible to draw a sharp distinction between the two classes of graves;[4] and for our present purpose they may safely be grouped together. They abound in Syria and Northern Africa, along the western side of the Spanish peninsula, over nearly the whole area of France, in Northern Germany, Wales[5] and the west of England, Ireland, South-Western and Northern Scotland, Denmark, and Scandinavia. Some archaeologists conclude that a dolmen-building race gradually moved westward from Syria, crossed the Straits of Gibraltar, and thence passed through Spain and Gaul into Britain; while others insist that the place of their departure was Scandinavia. But it is not improbable that dolmens, which exist also in India, Japan, and many other countries, and which might have been

[1] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, 1886, p. 232; W. C. Borlase, *Dolmens of Ireland*, ii, 1897, pp. 461–2.

[2] *Archaeol. Cambr.*, 5th ser., xvii, 1900, p. 222; W. C. Borlase, *Dolmens of Ireland*, ii, 446.

[3] *Ib.*, p. 426; *Proc. Soc. Ant.*, 2nd ser., v, 1870–3, pp. 367–70; viii, 1879–81, pp. 287–9; *Dict. des sc. anthr.*, 1883, pp. 388, 1078; *Archaeol. Cambr.*, 5th ser., xvii, 1900, p. 221; B. C. A. Windle, *Remains of the Prehist. Age*, pp. 174–7; *Rev. de l'École d'anthr.*, xiv, 1904, pp. 259–62. In the eighteenth century the famous Kentish dolmen called Kit's Coty House was still partly enclosed within a sepulchral mound. The Rev. W. C. Lukis, in a letter to Mr. George Payne (*Collectanea Cantiana*, 1893, p. 127), says, 'I have a letter written . . . in 1723 by one Hercules Ayleway [in which] . . . Kit's Coty is represented as being partly in a long barrow.' See also Borlase, *op. cit.*, iii, 752–3.

Mr. A. L. Lewis (*Man*, vii, 1907, No. 26, p. 38) says that a dolmen on Great Orme's Head shows 'that there certainly were dolmens that were never buried, but were intended to be "free-standing"'.

[4] W. C. Borlase, *Dolmens of Ireland*, ii, 424–6, 612–3.

[5] In Pembrokeshire, Glamorganshire, Merionethshire, Carnarvonshire, and Anglesey. Elsewhere they are almost entirely wanting, perhaps owing to the lack of suitable stones (*Archaeol. Cambr.*, 6th ser., iv, 1904, p. 199).

built all over the world if stones had been everywhere available for their construction, were not originally designed by any one people, and that the resemblances which have been pointed out between those of widely separated regions were simply due to the similarity with which different tribes acted in similar circumstances. The neolithic skulls and the neolithic sepulchral pottery of Scandinavia are unlike those of Britain; while, on the other hand, the British dolmens belong to an earlier stage of culture than those of Africa. Everything points to the conclusion that the earliest dolmen-builders of Britain retreated from Gaul before the sturdy round-headed invaders;[1] and it is useless to inquire whether the Mediterranean stock, to which the British, like the earlier French dolmen-builders, belonged, originated in Europe, in Asia, or in Africa. We only know that the oldest traces of the race were discovered in the Riviera.[2] Some philologists, however, affirm that the modern Celtic dialects are distinguished by peculiarities of syntax which show that they were influenced by contact with an older language akin to the Hamitic dialects of Africa.[3]

Relics of the neolithic population have been found over the whole extent of Great Britain and in the adjacent islands, from Kent to Cornwall, from the Isle of Wight to Shetland, not only in barrows and cairns, but also in caves in which they lived and died, in the neighbourhood of the quarries from which they obtained flint for manufacturing their tools, in pit-dwellings, on the margins of lakes, in the beds of rivers, in ditches, in peat-mosses, in sandy wastes where the sand had been blown away from the soil which it had long concealed, in fens, on open downs, and in fields by the accidental impact of a plough. Their sepulchres, as we shall afterwards see,[4] remain in comparatively few regions; but on the more cultivated lands many have doubtless been destroyed. It is reasonable to suppose that the settlements were made successively throughout a long period; and that the earliest comers took possession of the choicest lands in the south. Those who came later would

Relics of the neolithic population: their settlements.

[1] See pp. 402–5, *infra*.
[2] See pp. 405–6, *infra*.
[3] See p. 382, *infra*.
[4] See pp. 101–2, *infra*.

displace their predecessors if they had the power, and if the prize seemed worth a struggle: otherwise they would move on to the nearest vacant lands; and so in the course of ages, and after much bloodshed, the whole island came to be occupied. But each successive horde found large tracts of the country through which they plodded overgrown by forests or covered by morasses; and they must often have had to travel far before they could obtain a suitable abode. Except the gigantic Irish elk and the wild ox known as the aurochs, which survived into the Bronze Age, and which, later still, Caesar found roaming in the German forests,[1] the great beasts which had lived in Britain with palaeolithic man were no more; but brown bears and grizzly bears, beavers and wild cats, still survived; herons, swans, and cormorants flitted over the fens; red deer, wild boars, and even a few reindeer remained to supply the new comers with game; and in every forest wolves were lurking to prey upon their cattle.[2] If we were to mark upon a map all the places at which neolithic implements have been found, it would correspond more or less closely with one constructed *a priori* by a geographer, ignorant of the results of archaeological research, who appreciated the requirements of early settlers. He would expect to find that they had avoided as far as possible the toil of cutting down woods, and that they had selected dry uplands, where the subsoil was porous and their cattle could find pasture, and which overlooked river-valleys, where they themselves could get water and fuel, and on the slopes of which they could build sheltered dwellings. He would not therefore be surprised to learn that the traces of occupation are most numerous on the chalk downs, the Derbyshire moorlands, the Pennine Range and the Yorkshire Wolds, the Malvern Hills, and other high lands which fulfilled the necessary conditions.[3]

[1] *B. G.*, vi, 28.

[2] *Journ. Brit. Archaeol. Association*, xxxiv, 1878, p. 351; W. Boyd Dawkins, *Early Man in Britain*, pp. 257–62, 484; R. Munro, *The Lake-Dwellings of Europe*, 1890, p. 488; *Archaeologia*, lv, 1897, pp. 130–1, 158.

[3] See *Journ. Anthr. Inst.*, v, 1876, p. 359; the topographical index in Sir J. Evans's *Anc. Stone Implements*, 1897; and the *Victoria County Histories of*

Without his tools the settler could not build his hut, cut his firewood, or kill and dress a calf or a kid from his herd. Let us therefore try to ascertain how he made them, and how far he had improved as a craftsman upon the rude methods of his palaeolithic predecessor.

Flint mines and implement factories.

Within the last half-century archaeologists have succeeded in revealing some of the factories in which the prehistoric cutlers wrought. The nature of their materials of course still depended upon the rocks which were to be found in the district where they lived. Those who could get no flint used quartzite, basalt, felstone, greenstone, porphyry, diorite, or whatever stone they could obtain.[1] But flint was still the staple material. The palaeolithic hunters were obliged, as we have seen, to use stray blocks: their successors had learned how to win the flint from the bed of chalk in which it lay. Among the chief centres of mining and manufacture were Brandon in Suffolk and Cissbury, which is on the South Downs, about three miles north of Worthing. Grime's Graves, the mines which supplied the famous factory of Brandon, are situated in a fern-clad wood, and occupy more than twenty acres. The so-called graves are circular shafts, about twenty-five feet in diameter at the mouth, from thirty to fifty deep, and on an average twenty-five feet apart. Most of them were connected by galleries, which had been tunnelled in directions that followed the seams of the flint. The tools with which the excavations were made were stone 'celts', or hatchets, and picks made of the brow-tines of the antlers of reindeer. Unlike modern picks they were one-sided; and a specimen encrusted with chalk on which the owner's finger-prints

Berks. (i, 276), *Hants* (i, 257), *Lancs.* (i, 212), *Northampton* (i, 139), *Sussex* (i, 311, 313, 470), and *Worcester* (i, 180).

[1] Sir J. Evans, *Ancient Stone Implements*, 1897, pp. 65–6, 104–6, 107–9, 129–30, 213, &c. Implements of jade and jadeite, which are common in the lake-dwellings (F. Keller, *Lake Dwellings of Switzerland* [trans. J. E. Lee], i, 1878, pp. 72, 195–6, 215–6), are very rare in Britain (Evans, *op. cit.*, p. 109), and were doubtless imported, as jade apparently does not exist *in situ* in Europe, except in Silesia and Styria (*Journ. Anthr. Inst.*, x, 1881, p. 359; xx, 1890–1, pp. 332–42, especially 334 and 338; *Report of . . . the Brit. Association*, 1890, p. 971; *L'Anthr.*, iv, 1893, p. 555).

are still visible, is now lying in the Prehistoric Room of the British Museum. More than one of the lamps were found by the aid of which the workmen had groped their way through the galleries,—small cups hollowed out of chalk, which they had evidently filled with oil or fat and furnished with some kind of wick.[1] When the flint had been hewn out with the hatchets, which have left their marks upon the sides of the galleries, it was hauled up to the surface, perhaps in baskets made of wicker or hide, and carried to the workshops, where it was wrought into implements, which were afterwards bartered for such articles as the manufacturers required. Innumerable flakes and chips of waste flint were found, which testified to their activity. One of them at least was a sculptor as well. A fragment of a human limb, modelled out of chalk, was discovered by the antiquary who first explored the site ; and he tells us that the anatomical features were ' rendered with an accurate knowledge of the parts '.[2] But what most impressed him was to find in one of the galleries a set of tools lying upon a piece of unfinished work in the position in which they had been laid some four thousand years ago.[3] Walking through the wood to the open heath of Broomhill, he came to the pits that yield the material which the ' knappers ' of Brandon still manufacture into gun-flints for African tribes. The industry has been carried on since neolithic times, and even then it was ancient ; for Brandon was an abode of flint-workers in the Old Stone Age. Not only the pits but even the tools show little change : the picks which the modern workers use are made of iron, but here alone in Britain the old one-sided form is still retained. Only the skill of the workers has degenerated : the exquisite evenness of chipping which distinguished the neolithic arrow-heads is beyond the power of the most experienced knapper to reproduce.[4]

[1] Similar lamps have been found in neolithic caves in France (*Ass. franç. pour l'avancement des sc.*, 32e session, 1903, 2e partie, pp. 896–900), and are still used in China (*Proc. Soc. Ant. Scot.*, xxii, 1888, p. 81). Cf. p. 258, *infra*.

[2] *Journ. Ethn. Soc.*, N. S., ii, 1870, p. 430. [3] *Ib.*, p. 427.

[4] *Ib.*, pp. 419–39 ; S. B. J. Skertchly, *Memoirs of the Geol. Survey,—On the Manufacture of Gun-Flints*, pp. 39–41, 71, 74.

The flint works at Cissbury have a general resemblance to those of Grime's Graves; but the pits were sunk on a different principle.[1] They are contained in an entrenchment which did not exist at the time when the earliest were made, but was almost certainly constructed in the Neolithic Age.[2] The extreme rudeness of the tools which were found in them has led to the belief that they are older than Grime's Graves;[3] but, on the other hand, stone implements of the rudest kind were manufactured for special purposes long after the Stone Age had passed away.[4] Moreover, many of the ruder Cissbury tools appear to be unfinished; and it may have been intended that they should be perfected by the people with whom they were exchanged. Many of the smaller pits contained not only stone implements but also fragments of pottery and remains of horses, goats, deer, and horned cattle; and from this Pitt-Rivers, who first explored them, concluded that they had been used as dwellings after they had ceased to serve their purpose as quarries, or had been inhabited by the workers who obtained their flint from the larger pits. On this site also deer-horn picks were found; and Pitt-Rivers, wishing to test their value, provided a set of similar tools, with which he and one of the labourers whom he employed dug a pit three feet square and three feet deep in an hour and a half.[5]

Difficulty of determining age of stone implements.

With the better material which was thus obtained the neolithic craftsmen fashioned implements of which some can hardly be distinguished, even by experts, from those of the older period, though the greater number are recognizable even by a tiro. It must, however, be remembered that in many cases one cannot tell whether a find of stone implements belongs to the Neolithic or to the Bronze Age; and some are probably later still. Indeed it would be impossible

[1] *Journ. Ethn. Soc.*, N. S., ii, 1870, p. 439.

[2] *Journ. Anthr. Inst.*, v, 1876, pp. 368–74. See p. 98, *infra*.

[3] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 80, 85–6; *Guide to the Ant. of the Stone Age* (Brit. Museum), p. 79.

[4] See p. 214, *infra*.

[5] *Journ. Anthr. Inst.*, v, 1876, pp. 357–62, 382, 479; *Archaeologia*, xlii, 1869, pp. 59–60, 66, 68–9, 73–4; xlv, 1880, pp. 337–8, 340–7; *Guide to the Ant. of the Stone Age* (Brit. Museum), pp. 69–70; *Vict. Hist. of . . . Sussex*, i, 315.

to point to any kind of stone implements which ceased to be manufactured in Britain when bronze was introduced.[1]

Indefiniteness of the prehistoric 'Ages'.

One of the first cautions which the student of archaeology gives himself is that the epochs into which it has been found convenient to divide the Prehistoric Period were not definitely separated. It has been well said that they shade into one another like the colours in the solar spectrum.[2] The age in which we are now living affords an illustration. In one sense what might be called the Mechanical Age began when the first motor-car appeared on a London street; but we are still living in an era of transition, which will not end until, if ever, horses shall have ceased to be used for traction. Similarly stone tools continued to be used throughout the Bronze Age and the Late Celtic Period; and in certain remoter parts of the British Isles they are being used to-day.[3] When they are found associated with primary interments in long barrows or chambered cairns, or when they are met with in large numbers in other deposits which there is no reason to assign to a later period, they may as a rule be safely referred to the Neolithic Age; but, as we shall presently see,[4] there are certain implements of stone which were undoubtedly used in the Bronze Age, and of which it cannot be said with certainty that in this country they were used before. Some interments, however, which are ascribed to the Age of Bronze may have belonged to the older race, who still remained in their neolithic age although they were glad to use any bronze tools upon which they could lay their hands. Similarly the grave of an Australian savage who was buried some sixty years ago was found to contain, besides a piece of flint, a clay pipe, an iron spoon, and the handle of a pocket-knife.[5]

[1] See J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 306.

[2] *Journ. Anthr. Inst.*, xxxii, 1902, p. 395.

[3] See pp.131-2, 230, *infra*; E. B. Tylor, *Early Hist. of Mankind*, 2nd ed., 1870, p. 194; A. Lang, *Custom and Myth*, 1885, p. 11; O. Schrader, *Prehist. Ant. of the Aryan Peoples*, 1890, p. 234; and *Journ. Anthr. Inst.*, xxxiii, 1903, pp. 246–58.

[4] pp. 78–81, *infra*.

[5] *Nature*, Jan. 13, 1898, pp. 257–8.

Stone implements.

The several kinds of tools that first began to be used in the Neolithic Age present numerous varieties of form which, in this book, it would be irrelevant to describe. To deal with them is the province of archaeology; and the reader who wishes to make himself acquainted with them can do so, after he has mastered the literature of the subject, by visiting the collections in our museums and by himself becoming a collector. Here we desire only to learn so much as may help us to understand how neolithic man lived, and from what origins the culture which succeeded his was evolved.

The Neolithic Age is sometimes, especially on the other side of the Channel, called the period of polished stone: [1] but most of our flint implements were neither ground nor polished; they were merely chipped. Many specimens indeed, from one cause or another, have never received their finishing touches; but many others were of such a kind that grinding or polishing would have been labour lost.[2]

The two main divisions of flint implements.

Neolithic flint implements may be grouped in two classes. In one, which comprises the larger kinds—axes, hammer-stones, and the like—the implement was made out of a block of flint, and the splinters struck off during the process of manufacture were either mere waste or utilized for making smaller tools.[3] The other class consists of tools which were made out of flakes, the core, after all the required flakes had been detached, being thrown away.[4]

How flint implements were made.

Flint fresh from the quarry was easier to manufacture; and accordingly the cutlers established their workshops close by the mines. Their methods were perhaps not everywhere the same; but it is easy to form a general idea

[1] Traces of polishing are said to have been found on French implements of late palaeolithic age (*Ass. franç. pour l'avancement des sc.*, 13e sess., 1884, 1re part., p. 212; *L'Anthr.*, iv, 1893, p. 550).

[2] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 73, 85–6, and *Guide to the Ant. of the Stone Age* (Brit. Museum), pp. 69–70. Much depended upon the nature of the material. Certain hard stones, for instance granite and diorite, were necessarily ground and polished. See *L'Anthr.*, iv, 1893, p. 550, and *Guide to the Ant. of the Stone Age* (Brit. Museum), p. 69.

[3] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 276.

[4] *Ib.*, pp. 28–9, 31.

of them from observing the processes which are followed by tribes which are still in their stone age and by the knappers who ply their trade near Grime's Graves. Sometimes, like the Cloud River Indians, the workers may have applied a pebble or a punch of deer-horn to the surface of the flint block, and produced flakes by striking it with his stone hammer; but Sir John Evans believes that the flakes were generally struck off with a hammer or a pebble alone; and he has found experimentally that by this simple method a practised hand can attain almost perfect precision. Laying the flakes which he had thus removed with the flat face uppermost upon a smooth block of stone, he has succeeded by blows of a pebble in chipping their ends into whatever form he desired. Similarly hatchets were first rough-hewn by striking splinters from the flint block, and afterwards gradually chipped into the proper shape. Whether the material was flint or some other stone, the method would have remained the same. When it was desired to attain the utmost perfection, the implements were ground, not upon a revolving but upon a fixed stone, and polished by stone rubbers in conjunction with sand.[1] The process by which the arrow-heads and spear-heads were manufactured, whose exquisite workmanship entrances all who see them, cannot be described; for the modern tribes who make such weapons work in various ways. Small stone tools, however, are often found, with blunted ends, made out of thick flakes, which may have been used in arrow-flaking, and which accordingly have been termed 'fabricators'; and as they are most numerous in the districts which have yielded the greatest number of arrow-heads, the appellation is probably correct. Arrow-heads have indeed been recently made with them, but with somewhat obtuse edges; and it has therefore been suggested that the fabricator was only used for removing irregularities from the flake, and that the final chipping was accomplished with a tool of deer-horn, which, pressed deftly against the edge of the flake, detached minute splinters. The surface of many flint arrow-heads

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 14–37, 43. Cf. *15th Ann. Report American Bureau of Ethn.*, 1893–4 (1897), p. 25.

and javelin-heads is, however, covered with beautifully uniform fluting, like ripple-marks on sand; and the most experienced modern operators confess that they do not understand how this effect was produced.[1]

It may be well to enumerate the various tools which would have formed a complete outfit for a neolithic household. The kinds which were made from a block of stone were celts, which comprised hatchets and adzes, and of which some may have been used as chisels and knives; Celts.

FIG. 5. ½

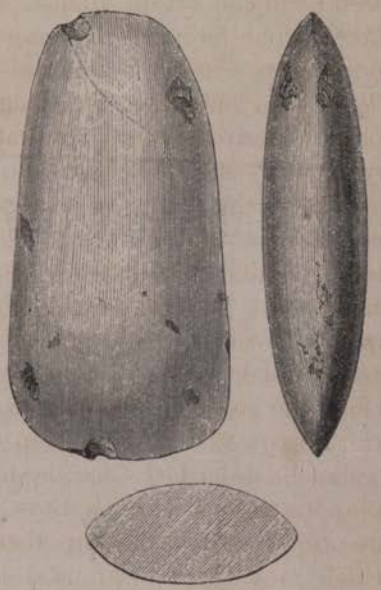


FIG. 6. ½

axes perforated for the insertion of a handle; chisels and gouges; hammer-stones, pestles, and whetstones. Most readers are familiar with the term 'celt'; but not every one is aware that it has no connexion with the name of the people who were the latest prehistoric invaders of these islands, and is simply an Anglicized form of a Latin word, meaning a chisel, which does not occur except in the Vulgate.[2]

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 37–43, 412, 414–6; *Journ. Anthr. Inst.*, xxxiii, 1903, p. 47.

[2] J. A. H. Murray, *New Eng. Dict.*, ii, 215. Cf. Sir J. Evans, *Anc. Stone Implements*, 1897, p. 55.

Some celts were ground or polished only on the edge; some over their whole surface; and a few are so exquisitely finished on both sides that the labour which was devoted to them would have seemed excessive unless it had been a labour of love.[1] On the other hand, many were neither ground nor polished; and some of the ruder ones may have been used as agricultural implements.[2] Several have been found with pointed butts and extremely elongated oval sections, which have the closest resemblance to celts from the West Indies, and illustrate the truth of the observation that identity in form of implements, weapons, and other objects belonging to widely separated lands does not necessarily prove community of origin, but as a rule merely shows that similar wants in similar circumstances produce similar results.[3] Although those celts which were used as hatchets or adzes were evidently mounted, there are some that show grooves on both sides or notches on one side, which seem to have been intended to enable them to be easily grasped.[4] Most of the handles, having been made of wood, have naturally perished; but two hatchets, now in the British Museum, have been found with their handles complete,—one in Solway Moss by a man digging peat for fuel,[5] the other in the bed of a Cumbrian lake called Ehenside Tarn.[6] Unlike

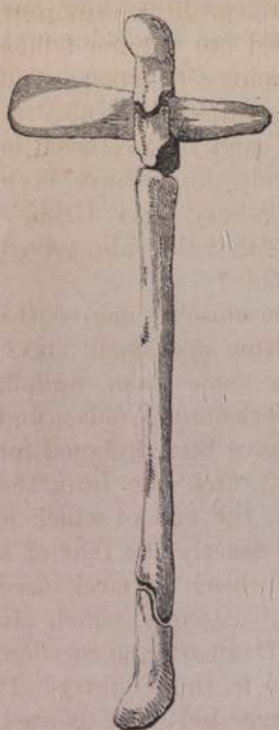


FIG. 7.

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 66, 107.

[2] *Ib.*, pp. 71, 172, 205; *Proc. Suffolk Inst. of Archaeology*, xi, 1903, p. 329.

[3] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 129, 235. See also E. B. Tylor, *Early Hist. of Mankind*, 1870, pp. 205–6; *Prim. Culture*, 1903, i, 65.

[4] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 136, 171.

[5] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 353–4.

[6] *Archaeologia*, xliv, 1873, pp. 281–3.

the Swiss lake-dwellers, who had learned to fix their blades in deer-horn sockets, which were sufficiently elastic to prevent the wooden hafts from being injured by concussion,[1] the makers of these hatchets had simply mounted them in a hole which fitted the butt, but which, by the jar of repeated blows, must soon have become split.[2]

Their uses.

Like the stone hatchets of the Maoris, neolithic celts were doubtless used not only for felling trees,[3] chopping firewood, and slaughtering cattle, but also as battle-axes; and the profusion in which the ruder kinds have been found at Cissbury and Grime's Graves shows that they also served as miners' tools.[4]

FIG. 8.

Chisels and gouges.

Among the chisels some of the most interesting are small specimens, which came from Suffolk and the Yorkshire Wolds, and which may have been designed for wood-carving, and one from the Fen country, the end of which is described as exactly like that of a narrow 'cold chisel' of steel, used by engineers.[5] Gouges, which are abundant in Denmark and Sweden, are very rare in this country. It has been suggested that canoes, for making which they were perhaps chiefly used, were more necessary in Scandinavia than in Britain; and it is significant that the best British gouges all

[1] F. Keller, *The Lake Dwellings of Switzerland*, i, 1878, pp. 21–2, 38, 57, 90, &c.

[2] An axe-hammer has, however, been found in the Liverpool Docks, scored with a groove, along which a withy was perhaps twisted to serve as a handle (Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 168–9; *Vict. Hist. of . . . Lancs*, i, 218).

[3] M. Hippolyte Müller (*L'Anthr.*, xiv, 1903, pp. 424–6) has cut down numerous trees with flint axes, which were uninjured by the experiments. Two of the trees were felled in thirteen and fourteen minutes respectively.

[4] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 79, 171–2, 195–6.

[5] *Ib.*, pp. 175–6.

come from the fens, where canoes must have been needed for crossing the floods.[1] It is probable, however, that although gouges may have been used in finishing the vessels, the heavy work of hollowing the trees out of which they were formed was largely performed by the agency of fire, as among the North American Indians of comparatively recent times.[2]

Axes, axe-hammers, anvils, and mullers.

No stone implements are more familiar to students of antiquities than the axes, axe-hammers, and hammers, in which, as in those of our own day, holes were drilled for the insertion of handles. Many of them were probably used as weapons of war. Some of the axes are double-edged, though the edge is often blunted, as though it had been intended rather for striking than for cutting; while the axe-hammers resemble an ordinary hammer at one end, and are sharpened at the other.[3] It would perhaps be impossible to prove that any of these tools were used in Southern Britain in the Neolithic Age, although they

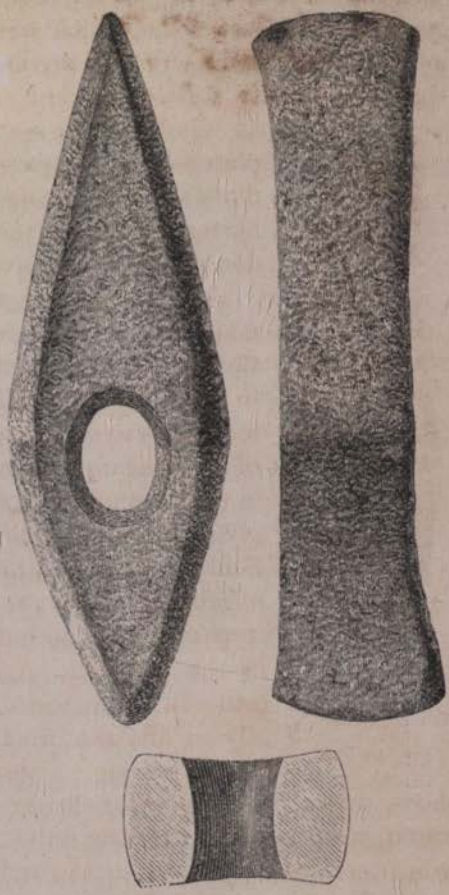


FIG. 9. ½

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 178.
[2] *Journ. Anthr. Inst.*, iv, 1875, p. 403.
[3] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 183–4.

were not uncommon on the Continent ;[1] and most of those which are to be seen in our museums undoubtedly belong to the time when bronze was common :[2] but some few have been found in Scotland in chambered cairns.[3] Not one of them is made of flint.[4] Of the implements which are known as hammer-stones some which have deep cup-shaped depressions may have served as anvils or mortars ; and others again—quartzite pebbles or flint cores, which were found at Cissbury, Grime's Graves, and other places—were apparently used for chipping flints. Some nearly globular stones, whose battered surfaces testify to hard wear, were doubtless for triturating grain or edible roots.[5]

FIG. 10. ½

Implements made by flakes.

The varieties of tools which have been made out of flakes are too numerous to particularize. Simple flakes, flat or triangular in section, varying in length from nine or ten inches to one inch, are the most abundant of all stone implements, and are to be found in every quarter of the globe. Here they are generally made of flint and are rarely ground. Some of them may have been used as surgical instruments ; for, as we shall presently see, trepanning of the human skull was practised in the Neolithic Age.[6] Others were made into saws, the teeth of which are occasionally so fine that to the unaided eye they are hardly visible.[7] Many, shaped like horse-shoes, ducks' bills, oyster-shells, or short spoons, or nearly round, were used for dressing hides, for scraping haematitic iron ore in order to obtain the red pigment which served

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 215 ; *L'Anthr.*, iv, 1893, p. 489.

[2] See *Archaeologia*, xliii, 1871, p. 409.

[3] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 246 ; *Archaeol. Cambr.*, 6th ser., iii, 1903, p. 234 ; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 355.

[4] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 183–4, 195, 215, 231.

[5] *Ib.*, pp. 238–9, 245, 247–8, 250–2.

[6] *Ib.*, pp. 275–6, 289. See pp. 92–3, *infra*.

[7] See *Anthr. Rev.*, iii, 1865 (*Journ. Anthr. Soc.*, p. lxvi) ; *Archaeologia*, xlii, 1869, pp. 229–30 ; and *Anc. Stone Implements*, 1897, p. 294. Dr. R. Munro

primitive man as rouge,[1] and perhaps, in conjunction with nodules of iron pyrites, for producing fire.[2] Some were fashioned into awls and drills;[3] others into knives, daggers, and curved blades, which may perhaps have been sickles.[4] But the most beautiful weapons made out of flakes were javelin-heads and arrow-heads, which in this country are almost always of flint. If British neolithic workmanship did not on the whole reach the level of that of Denmark, in fashioning missile weapons our armourers could hold their own. Whether any given specimen was an arrow-head or a javelin-head, a javelin-head or a spear-head, can generally be decided only by size. Many are so small that no one can mistake the purpose for which they were intended; but it is not certain whether the largest were attached to spear-shafts, properly so called, or served as javelins. Arrow-heads and javelin-heads may be grouped in four classes, each of which has several varieties,—leaf-shaped, lozenge-shaped, stemmed, and triangular; but some five or six arrow-heads have been picked up whose outline was characterized by ogee curves. The stemmed heads are generally, and the triangular, which are rare, occasionally barbed. Although the various kinds were used contemporaneously,

Javelin-heads and arrow-heads.

FIG. 11. ½

(*Prehist. Problems*, pp. 325–30, 359) would refer all the British flint saws to the Bronze Age, on the ground that 'bronze saws have never yet been found in the British Isles': but this statement is inaccurate (see p. 132, *infra*); and, as we have seen (p. 41, *supra*), serrated palaeolithic flints have been unearthed in a gravel-pit at Swanscombe.

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 299–300, 311–2.

[2] *Ib.*, pp. 312–9; *Mem. Geol. Survey,—On the Manufacture of Gun Flints*, p. 39.

[3] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 321.

[4] *Ib.*, pp. 326–32, 356–7.

barbs were perhaps of comparatively late invention,[1] and may have been evolved in the struggle for existence as the population became more dense.[2] Not a single barbed arrow-head or javelin-head has ever been found

FIG. 12.

FIG. 13.

in a long barrow;[3] but they occur in the chambered cairns of Scotland, as well as in certain English round barrows which were erected towards the end of the

FIG. 14.

FIG. 15.

Neolithic Age;[4] and a fine specimen was associated with many beautifully finished implements in a neolithic village at West Wickham.[5] A leaf-shaped arrow-head was found in a peat-moss at Fyvie, in Aberdeenshire, still fixed

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 360–1, 369–98; *Man*, vii, 1907, No. 25, p. 37; No. 37, p. 56.

[2] See *Journ. Anthr. Inst.*, xxxiii, 1903, p. 54.

[3] Sir J. Evans, *Anc. Stone Implements*, p. 377.

[4] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 272.

[5] *Journ. Anthr. Inst.*, xxix, 1899, p. 131.

in a cleft in its shaft; but the cord or sinew by which it had doubtless been secured had disappeared.[1] Arrow-heads may also have been made of hardened wood or bone, which holds poison better than flint.[2]

Bone implements.

The archers of many countries use wrist-guards to protect their arms against the recoil of the bowstring; and for this purpose the prehistoric Britons made rectangular plates of stone or bone, curved to fit their wrists and perforated near the angles with holes to enable them to be fastened. Most of those which have been collected belonged to the Bronze Age; but they probably came into use before.[3] Various other implements of bone—awls, needles, chisels, and perhaps daggers and lance-heads—were also common in the Neolithic Age;[4] and it is worth noticing that a well-known collector has found palaeolithic tools which, as his practised eye discerned, had been picked up and reflaked by neolithic men.[5]

Pygmy flints.

Of all stone implements the most curious are the tiny objects which are known as 'pygmy flints', and which have been found not only in certain parts of Britain[6] and Ireland,[7] but also in France, Belgium, Spain, North Africa and Egypt, Palestine and India. They are all made of minute flakes; and in one of our collections the marks of working could not be detected without the aid of a microscope, while sixty-four specimens, many of which were no more than a quarter of an inch long, weighed less than half an ounce. Numerous guesses, which need not be repeated, have been made as to their use. Everywhere their forms are identical; and, partly for this reason, partly because

[1] *Proc. Soc. Ant. Scot.*, xi, 1876, p. 509.

[2] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 361.

[3] *Ib.*, pp. 428–30; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 355, 361.

[4] Sir J. Evans, *Anc. Stone Implements*, pp. 431, 433–4.

[5] Worthington G. Smith, *Man, the Primeval Savage*, p. 304. Messrs. W. Johnson and W. Wright (*Neol. Man in N.-E. Surrey*, 1903, pp. 49, 169), who have been diligent in collecting tools from North-Eastern Surrey, remark that 'some were fitted for use in the right hand, and others for the left', and conclude that the people who used them were ambidextrous. But surely the more natural conclusion would be that some were left-handed!

[6] On the moors near Sheffield and in East Lancashire, in Staffordshire and Lincolnshire, and at Hastings. [7] Donegal.

in many places no other implements were associated with them, it has been supposed by lovers of the marvellous that they were the work of a peculiar race.[1] If the latter reason were valid, we should be compelled to assume that the Lilliputians had sent out many colonies from the land where Gulliver found them. But every archaeologist knows that tools and other articles of identical form are to be found in divers continents; and pygmy flints may often have lain with others and have escaped observation.

Specialization of industries.

A survey of the implements and other relics arranged in a representative collection teaches us that men had already learned the necessity of a division of labour. Some clans who used flint implements could only have obtained them by barter. Even in the great factories of Grime's Graves and Cissbury the miners were evidently distinct from the cutlers, as were both from the herdsmen. But in other settlements, where mining and cutlery were apparently not predominant industries, implements have been found of such perfect finish that their manufacture would seem to have been the special or the sole occupation of skilled members of the community.[2]

A lost art.

But there was one thing which the forerunner of neolithic man had done, and which he could not do. Among his relics we may look in vain for the carved dagger-handles, the engraved antlers, and the other works of art of the palaeolithic caves. Except in Grime's Graves, not a single attempt to portray the human figure, or animal, or plant has ever been found among the deposits of the Neolithic Age. If the artists of Derbyshire and Aquitaine had left descendants, perhaps they were massacred or enslaved, perhaps their individuality withered under oppression: whatever may have been the cause, the old creative art was dead.[3]

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 324–5; *Proc. Roy. Irish Acad.*, 3rd ser., vi, 1900–2, pp. 362–3; *Reliquary*, N. S., vii, 1901, pp. 123–6; *Proc. Soc. Ant. Scot.*, xxxv, 1901, pp. 98–101; *Man*, ii, 1902, No. 15, pp. 18–22.

[2] See *Journ. Anthr. Inst.*, xxix, 1899, p. 136.

[3] See *L'Anthr.*, v, 1894, pp. 20–1, 146, and *Guide to the Ant. of the Stone Age* (Brit. Museum), pp. 77–8. M. Salomon Reinach (*Rev. celt.*, xiii, 1892, pp. 193–9) attributes the absence of such artistic remains in France to the influence of Druids.

Dwellings. Provided with their tools, the neolithic herdsmen were able to construct dwellings which, humble as they were, must have been comfortable in comparison with the shelters that had satisfied the hunters of the older time. Unfortunately, however, the evidence relating to the domestic life of the neolithic people is far less complete than that which has been preserved in regard to their Swiss contemporaries. In that age and for many centuries after it had come to an end the inhabitants of northern and western Europe, like the ancient Romans whom Horace [1] eulogized, were content to live in habitations which were small and mean, while, under the influence of superstitious terror as much as of reverence, they constructed the mansions of their dead chieftains on a magnificent scale. Thus, while neolithic sepulchres are still conspicuous upon the western hills, few buildings have left traces which can be referred with absolute certainty to the same period.[2] Many of the 'hut-circles' and pit-dwellings which have been excavated contain no trace of metal; but it is generally

[1] *Carm.*, ii, 15, 10–20.

[2] Professor B. C. A. Windle (*Remains of the Prehist. Age*, p. 257) affirms that excavations in the stronghold of Eggardûn in Dorsetshire have proved that 'pit-dwellings were in use in the Pre-metallic period'; and he remarks (*ib.*, p. 258) that there was 'no trace of any metallic object in the pits examined by Stevens at Hurstbourne or in those at Standlake' in Oxfordshire. It is shown on p. 97, *infra*, that there is no sufficient reason for referring Eggardun to the Neolithic Age: bronze was discovered at Standlake (*Archaeologia*, xxxvii, 1857, p. 368), which, according to Pitt-Rivers (*Excavations in Cranborne Chase*, i, 20–1), bore such an 'exact resemblance to the [Romano-British] Woodcuts village', that, in his judgement, 'further excavations would have proved it to have been of the Roman or Late-Celtic period': Romano-British pottery was found in the pits at Hurstbourne; and Dr. Stevens himself (*Parochial Hist. of St. Mary Bourne*, 1888, p. 34) only claimed that 'the flint implements . . . establish that the site, if not the dwellings, was occupied by the people of the Neolithic Age'. Professor Boyd Dawkins (*Vict. Hist. of . . . Hants*, i, 262) rightly refers the dwellings to the Iron Age.

Bone weaving-combs, which were found in pit-dwellings at Highfield, near Fisherton in Wiltshire, evidently belonged, like the querns with which they were associated (J. Stevens, *Parochial Hist. of St. Mary Bourne*, p. 25), to the Early Iron Age (cf. Sir J. Evans, *Ancient Stone Implements*, 1897, p. 251, and *Reliquary*, N. S., vii, 1901, p. 115); and although Professor Boyd Dawkins (*Early Man*, &c., p. 268) pleads that the pottery, which was 'ornamented with incised curves', was 'not turned in the lathe', that does not prove that it was made in the Neolithic or even the Bronze Age (see p. 244, *infra*); while the 'curves' suggest that it was Late Celtic.

impossible, in any given instance, to dismiss all doubts as to their antiquity when we find others, precisely similar, which were certainly occupied, if not built, by people who used implements of bronze. Still it is not credible that such dwellings were constructed for the first time after the introduction of metal working; and it is reasonable to believe that they were common before the earliest bronze implement was imported into Britain. Indeed a pit-dwelling has been found at the eastern end of a long barrow near the village of Hanging Grimston on the Yorkshire Wolds; and, as it was proved by excavation to be older than the barrow,[1] it must have been dug in the Neolithic Age. There were of course villages of some sort at Cissbury and Grime's Graves; and at Grovehurst, near Sittingbourne, are the remains of huts which were occupied by implement-makers.[2] A group of pits on the sheltered southern slope of Croham Hurst, about a mile south of Croydon, the fields near which are thickly strewn with flint flakes, probably formed the winter abode of a small community: [3] on Hayes Common a village has been explored, comprising about one hundred and sixty pits, the period of which was determined by the discovery of a neolithic workshop, on the floor of a pit of identical form, at Millfield in the immediate neighbourhood; and the neolithic age of a settlement at West Wickham was as clearly proved by the nature of the implements.[4] At these places, at Weybourne in Norfolk, on the Hampshire Downs, and elsewhere, the sites of such dwellings are indicated by circular depressions, ranging in diameter from six to thirty feet and from two to six feet deep, which, though they generally occur in groups, are sometimes isolated. Each is surrounded by a bank, formed of the excavated earth, in which the entrance is marked by a gap. The bank was in certain cases prevented from falling in by a stone circle; and upon it was reared a hut, sometimes

[1] *Journ. Anthr. Inst.*, xi, 1882, pp. 472–3.
[2] *Archaeol. Cant.*, xiii, 1880, pp. 122–6.
[3] *Journ. Anthr. Inst.*, xxix, 1899, p. 128; *Vict. Hist. of . . . Surrey*, i, 237.
[4] *Proc. Soc. Ant.*, 2nd ser., xii, 1887–9, pp. 258–63; xvii, 1897–9, pp. 216–21; *Journ. Anthr. Inst.*, xxix, 1899, pp. 124, 127, 134.

perhaps formed of stones, but more often of interlaced boughs, while the roof, in which a hole was left for the escape of smoke, was probably thatched with fern or heather or turf, and, if it happened to be large, supported by a pole or the trunk of a tree, the position of which seems to be indicated by a mound in the centre of the pit.[1] A cluster of huts was apparently sometimes surrounded by an entrenchment, which protected the inhabitants and their cattle from night attacks.[2] Rude as these structures were, they fulfilled their purpose. The soil on which they were built was generally dry: the pit not only ensured warmth but also enabled the roof to be carried to a sufficient height: the bank, by throwing off the rain, kept the interior dry; and while in certain cases the remains of a hearth made of flints are found in the centre, in others it would seem that cooking was performed outside. Thus one group of pits on Hayes Common, the dimensions of which are within the ordinary range, is associated with smaller depressions, which apparently contained cooking-hearths.[3] A small fire might have been safely lighted inside the hut to warm the inmates; but a large one, such as would have been necessary for cooking a joint or an entire hare or sucking-pig, might have ignited the inflammable roof.[4]

A remarkable group of pits has recently been excavated in Wigtownshire.[5] Piles had been driven into them to

[1] *Norfolk Archaeology*, iii, 1852, pp. 232–6; *Journ. Anthr. Inst.*, xxix, 1899, p. 127; *Vict. Hist. of . . . Surrey*, i, 236. Cf. *Archaeol. Cambr.*, 6th ser., iv, 1904, p. 200.

[2] *Vict. Hist. of . . . Hants*, i, 258–9.

[3] *Journ. Anthr. Inst.*, xxix, 1899, pp. 127, 139.

[4] *Ib.*, p. 140. Stone mounds have been discovered in South Wales by Messrs. T. C. Cantrill and O. T. Jones, who regard them as 'probably the remains of prehistoric hearths or cooking-places' (*Archaeol. Cambr.*, 6th ser., vi, 1906, p. 17); but, as they 'range in diameter from 6 feet or so to as much as 50 feet' (*ib.*, p. 19), I would suggest that the cooks must have been of Brobdingnagian stature.

For descriptions of other pit-dwellings which may perhaps be of neolithic age, see G. Young, *Hist. of Whitby*, ii, 1817, pp. 666–83; T. Bateman, *Vestiges of the Ant. of Derbyshire*, 1848, p. 126; *Journ. Brit. Archaeol. Association*, xi, 1855, pp. 305–13; *Anthr. Rev.*, v, 1867, p. 253; and *Archaeologia*, xlii, 1869, pp. 223-4.

[5] *Proc. Soc. Ant. Scot.*, xxxvii, 1903, pp. 370–415.

support a wooden floor, the object of which was doubtless to keep them dry; and the marks on the piles seemed to their discoverer to show that they had been cut with stone hatchets.[1]

Three entirely subterranean chambers, of a kind which has been met with nowhere else in the British Isles, have lately been discovered by navvies who were digging a sewer-trench at Waddon, near Croydon. They were about twelve feet in diameter and seven feet high; and although they contained fragments of Romano-British pottery, the flint flakes and blocks which lay upon the floors were assigned by the experienced antiquary who explored them[2] to the Age of Stone. While he was impressed by their exact resemblance to certain Portuguese neolithic chambers which were used for burial,[3] he suggested that they might also have served as shelters in times of excessive heat or cold.

Unlike their Swiss contemporaries, who built their huts on platforms, supported by piles driven into the beds of lakes, the neolithic Britons lived mainly if not exclusively on land. Lake-dwellings indeed abound in the British Isles; but exploration shows that almost all were erected in the Late Celtic Period; and the only one in Britain which can with any show of reason be referred to the Age of Stone is in Holderness, which, before it was drained, was covered with marshes and shallow meres. One of a group of five, called the West Furze dwelling, contained a large number of flint flakes: but a bronze spear-head was also found in it; and the evidence is not sufficient to show that it was built in a pre-metallic period.[4]

[1] A chambered mound in Stromness, Orkney, which was not a sepulchre but a dwelling, has also been recently excavated, and contained a stone implement (*Proc. Soc. Ant. Scot.*, xxxvii, 1903, pp. 352-9). Mr. Christison (*ib.*, xxxviii, 1904, pp. 5–6) tentatively refers it to the Neolithic Age, while the discoverer more cautiously says that it must have been built in 'a remote period, not . . . because the implement is made of *stone*, but because the type is an ancient one'.

[2] Mr. George Clinch. See his article in *Surrey Archaeol. Collections*, xvii, 1902, pp. 181–3.

[3] *Matériaux pour l'hist. . . . de l'homme*, 3e sér., ii, 1885, pp. 1–18.

[4] R. Munro, *The Lake-Dwellings of Europe*, 1890, pp. 470–1, 489. Cf. *Journ.*

Food and cookery.

The food of the neolithic population has left more abundant traces than their homes. The bones which are strewed in their sepulchres and settlements show that they lived in great part on venison and the flesh of the wild boar ; [1] and the skull of an aurochs, which was found in the Fen country with a stone weapon sticking in it,[2] proves that they also followed the largest game. Unlike the palaeolithic hunters, they used dogs in the chase ; and it has been plausibly conjectured that these animals were the first to be domesticated. For man was a hunter before he was a herdsman ; and the dog would soon begin to lick the hand that rewarded it with a share of the slaughtered boar or deer.[3] It would seem, however, that when with advancing age dogs had become too slow for hunting, they were killed and eaten ; for canine bones, apparently of old animals, were found at Grime's Graves.[4] Neolithic immigrants introduced sheep,[5] goats, and pigs as well as horned cattle ; and all the bones of the latter which have been collected from their refuse-heaps and graves were those of small oxen, the scientific name of which—*Bos longifrons*—is familiar to all students of antiquities, and which resembled their living descendants, the Kerry cattle of Ireland and the small black animals of the Welsh mountains. Some authorities believe that these and all our varieties of domestic oxen are descended from the aurochs, which, as we have seen, was living in this country in palaeolithic times, and suggest that its calves were trapped and tamed ; [6] while others maintain that *Bos longifrons* was introduced by neolithic immigrants. The extreme smallness of the prehistoric domestic oxen is as easily accounted for as that of the mountain cattle of the present day. The tribes who kept them had but limited pasturage : forage

Roy. Soc. Ant. Ireland, 5th ser., x, 1900 (1901), pp. 208, 235, and *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 142–3. See p. 154, *infra*.

[1] *Mem. Anthr. Soc.*, iii, 1870, p. 76 ; W. Greenwell, *Brit. Barrows*, 1877, p. 742.

[2] *Journ. Brit. Archaeol. Ass.*, N. S., v, 1899, p. 285.

[3] W. Greenwell, *Brit. Barrows*, p. 742.

[4] *Journ. Ethn. Soc.*, ii, 1870, p. 431.

[5] W. Greenwell, *Brit. Barrows*, pp. 740–1. Cf. p. 151, *infra*.

[6] *Archaeologia*, lv, 1897, pp. 132–3, 150 ; *Archaeol. Journal*, liv, 1897, p. 379 ; R. Lydekker, *Mostly Mammals*, pp. 52, 299.

in winter was probably scanty; and the milk which was needed by their calves was largely consumed by their owners.[1] The broken bones of cattle which were found at Grime's Graves belonged to very young animals, which the implement-makers who bred them evidently could not afford to rear.[2] The meat was boiled in rude hand-made vessels of earthenware heated by red-hot flints, or, as we may infer from the frequent occurrence in barrows of charred bones, roasted or broiled over the fire; and the remains of each meal were left to accumulate in the huts.[3] It has been suggested by one of the most eminent of living anthropologists that the ornament, so often observed on prehistoric earthenware, which was produced by impressing a cord upon the clay while it was soft, may be traceable to an earlier time when the art of the potter had not been evolved, and vessels were made of plaited cords and also perhaps of skins and hollowed wood.[4]

Agriculture.

Although agriculture was practised by the later neolithic inhabitants of Denmark[5] and the lake-dwellers of Switzerland,[6] there is very little evidence that their contemporaries in this country tilled the soil. A few of the stone pestles which have been found belong, it is true, to that period,[7] but it is impossible, except perhaps in a very few instances, to affirm that they were used for grinding corn;[8] and although, as we have seen, certain rough-hewn celts may have been agricultural implements,[9] it is doubtful whether they all belong to the Age of Stone. Cereals and textile flax-fabrics, which are abundant in the lake-dwellings, are

[1] W. Greenwell, *Brit. Barrows*, pp. 743–4; F. Keller, *Lake Dwellings of Switzerland*, i, 1878, p. 479.

[2] *Journ. Ethn. Soc.*, ii, 1870, p. 431.

[3] *Journ. Anthr. Inst.*, xxix, 1899, pp. 135–6; J. R. Mortimer, *Forty Years' Researches*, p. lxx. Cf. E. B. Tylor, *Early Hist. of Mankind*, 1870, pp. 262–70, and *Ency. Brit.*, xxv, 1902, p. 467.

[4] *Archaeol. Review*, i, 1888, p. 6.

[5] *Congrès internat. d'anthr. et d'arch. préhist.*, 1900 (1902), p. 407.

[6] F. Keller, *Lake Dwellings*, &c., i, 1878, pp. 518–36.

[7] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 257.

[8] A stone which appears to have been used as a grain-crusher was found in the neolithic village at West Wickham (*Journ. Anthr. Inst.*, xxix, 1899, p. 133).

[9] See p. 76, *supra*.

absolutely wanting in every British neolithic deposit that has been explored.[1] Negative evidence of this kind may not be worth much: nevertheless there is reason to believe that agriculture was rare in Britain before the introduction of bronze. Barrow-diggers have often noticed that the teeth of neolithic skeletons are, as a rule, remarkably perfect; while those of the skulls found in round barrows and unchambered cairns are very much worn down; and it has been reasonably argued that the difference was due to food. The people of the Bronze Age, who were undoubtedly cultivators, subsisted in great part upon grain, which was probably ill cooked, and must have been largely mixed with stony grit from contact with the rude mullers by which it had been ground. The neolithic people, on the other hand, lived mainly upon milk and flesh-meat.[2] Pastoral tribes do not turn to agriculture until their numbers have increased to such a degree that they have no prospect of being able to live by hunting and on the produce of their flocks and herds alone: they prefer an easy life; and agriculture, especially to those whose implements are primitive, is difficult and laborious.[3] If corn was grown, it was probably on the open chalk downs.[4] The richer soils were covered with forest; and, although the stone axe was a better tool than any which the primitive hunters had possessed, the neolithic herdsman must have shrunk from the labour of cutting down the trees and dragging them

[1] W. Greenwell, *Brit. Barrows*, pp. 744–5.

[2] *Archaeologia*, xix, 1821, p. 48; *Mem. Anthr. Soc.*, i, 1865, p. 144; *Anthr. Rev.*, iii, 1865 (*Journ. Anthr. Soc.*, p. lxvii); W. Greenwell, *Brit. Barrows*, pp. 701–5; *Brit. Med. Journal*, 1903, pp. 809–10; *Vict. Hist. of . . . Derby*, i, 168. It must be admitted that in some cases the teeth of neolithic skulls are as much worn as those of the Bronze Age.

Messrs. W. Johnson and W. Wright (*Neol. Man in N.-E. Surrey*, 1903, pp. 53–4), referring to *Science Gossip*, July, 1901, p. 36, affirm that under the tartar which covered the teeth of a skeleton in a neolithic barrow on Warminster Downs were found particles of quartzite,—'apparently the rubbings from the mortar in which the corn was ground.' But the writer of the article in *Science Gossip* states that bronze was found in the barrow.

[3] See O. Schrader, *Prehist. Ant. of the Aryan Peoples*, p. 286, and *Journ. Roy. United Service Inst.*, xiii, 1870, p. 518.

[4] See *Journ. Anthr. Inst.*, iii, 1874, pp. 35–6, and Mr. Clement Reid's article in *Vict. Hist. of . . . Sussex*, i, 9–10.

away. Fire would have been of no avail. Men who have cleared forests in New Zealand will tell you that the fiercest flames will not destroy standing trees: twigs and leaves burn like tinder; but the trunk remains unconsumed.

Treatment of women.

There is evidence, though it is hardly needed, that the inevitable hardships of life were not equally shared, and that the lot of the women was worse than that of the men. Judging from the measurements of the neolithic skeletons, the disparity between the sexes in stature was as great as it is among modern savage tribes. The average height of the men was about five feet six inches, of the women only four feet ten inches: the difference in civilized communities is about half as much.[1] It is perhaps safe to conclude that when food was scarce, the men thought first of themselves, and that the women not only suffered from the effects of early child-bearing,[2] but had more than their share of toil.

Duration of life.

No doubt disease, the attacks of wild beasts, and frequent accidents, as well as intertribal wars, tended to shorten the duration of life: at all events Thurnam calculated that the average age of the people whose skeletons he had examined was not more than forty-five years.[3]

Clothing and ornaments.

The sheep and goats and the wild red deer which supplied the tribes with food doubtless clothed them as well; and it may be questioned whether in this respect they had advanced much beyond the primitive denizens of caves. The lake-dwellers of Switzerland were expert spinners: the textile fabrics which lay unnoticed for millenniums in their settlements show what they could achieve.[4] Our own forefathers may have been as skilful: but evidence is lacking; and their pottery was so inferior to that of the Helvetians, they lagged so far behind them as tool-makers, that we may reasonably assume that their women also were less proficient in domestic arts.[5] The perforated disks of stone and baked clay, called spindle-whorls, by which the

[1] *Journ. Anthr. Inst.*, v, 1876, pp. 121, 478. See also p. 152, *infra*.
[2] W. Greenwell, *Brit. Barrows*, pp. 659–60, 704.
[3] *Nature*, Nov. 22, 1894, p. 92.
[4] F. Keller, *Lake Dwellings of Switzerland*, i, 1878, pp. 44, 46, 56, 63–4, 67, 69, 505–17, &c.
[5] W. Greenwell, *Brit. Barrows*, pp. 744–5.

spindle was made to rotate, have indeed been found in great numbers here; but not a single specimen can be assigned with confidence to the Neolithic Age.[1] British ornaments too of that period are very rare.[2] No doubt the Britons were as fond of display as other barbarians: there is, as we have seen,[3] some evidence that they decorated their bodies with red paint; but a few lignite beads, found in the long horned cairn of Yarhouse,[4] and a single bead of shale, found in a long barrow in Gloucestershire,[5] are all the personal ornaments that we can unhesitatingly refer to the Age of Stone. An ingenious archaeologist, who perhaps knows less of human nature than of books and museums, has argued that the origin of jewellery was rooted in superstition;[6] and those who know that natural holed stones are still prized as amulets in the more primitive villages of this country[7] may easily persuade themselves that savage men and women had faith in the prophylactic properties of the perforated teeth and beads which they hung round their necks: but nobody who can understand the passion for sparkling gems which possesses many women and some men will believe that the love of adornment for its own sake was not as deep-seated in primitive human nature as superstition.[8]

Trepanning.

But amulets of a different kind, which are abundant in other lands, appear to be almost entirely wanting in our own. It is not difficult to understand that in material culture the prehistoric inhabitants of Britain should have been outstripped by those of the Continent; but it is

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 436–9. [2] *Ib.*, p. 465.

[3] See p. 80, *supra*, and *Anc. Stone Implements*, 1897, p. 312.

[4] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 240.

[5] W. Greenwell, *Brit. Barrows*, pp. 519–20, 543.

[6] 'The fact is,' says Professor Ridgeway (*Man*, iii, 1903, No. 97, pp. 171–2), 'that mankind was led to wear such objects by magic rather than by aesthetic considerations . . . the use of all the objects still employed in modern jewellery has primarily arisen from the magical powers attributed to them, by which they were thought to protect the wearer.' M. Salomon Reinach's review (*L'Anthr.*, xiv, 1903, pp. 711–12) of Professor Ridgeway's article is worth reading.

[7] A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 179; *Folk-Lore*, xii, 1901, p. 175.

[8] See Lord Avebury's *Origin of Civilisation*, 1902, pp. 54–8.

remarkable that a practice, the motive of which was mainly superstitious, and which was prevalent not only in every European country but also in America, has in this island apparently left but one vestige, which belonged to the Late Celtic Period. Sixty trepanned skulls were found in the cavern of Baumes-Chaudes in the department of Lozère; and twenty years ago a French physician had collected one hundred and sixty-seven. The operation was evidently performed either by scraping the skull with a stone implement or with a stone saw;[1] for an eminent surgeon has remarked that saw-cuts are distinctly visible on some of the French trepanned skulls. In a few cases the object was to remove dead bone; but as most of the skulls show no trace of disease, it has been conjectured that the patients were afflicted with epilepsy, and that the operator's aim was to relieve them by permitting the escape of the demon who was believed to be the author of their sufferings. It is, however, certain that the skull of a corpse was sometimes trepanned; and the edge of the perforation in specimens of this class generally shows signs of an old cicatrization. The explanation may easily be found. Some of the fragments which had been removed from trepanned skulls were evidently used as amulets, for they are carefully rounded, polished, and perforated for suspension; and one was actually found hanging from a Gallic torque, or gold collar, of the Early Iron Age. Most probably, as the famous anthropologist, Paul Broca, concluded, these amulets were taken posthumously from the skulls of persons who had survived the operation, being regarded as potent prophylactics.[2]

[1] The primitive method was apparently scraping (*L'Anthr.*, viii, 1897, p. 204). M. Hippolyte Müller (*ib.*, xiv, 1903, pp. 430–4) has performed the operation on four skulls by scraping with a flint implement; and he concludes that this method was adopted in the case of living patients. It appears (*ib.*, p. 434) that the distinguished anthropologist, M. Capitan, has been impelled by scientific ardour to experiment 'sur plusieurs chiens vivants'. What will happen if the Anti-Vivisection Society hears of this?

[2] *Bull. de la Soc. d'anthr. de Paris*, 3e sér., iv, 1881, p. 107; vi, 1883, pp. 318–9; *Journ. Anthr. Inst.*, xi, 1882, pp. 9, 12–4, 16; xvii, 1888, pp. 101, 106; *Proc. Soc. Ant. Scot.*, xxvi, 1892, pp. 5, 8, 14–5, 17–8, 21, 28, 30–2; Sir J. Evans, *Anc. Stone Implements*, 1897, p. 289; R. Munro, *Prehist. Problems*,

The *couvade.*

Folk-lore societies have collected countless instances of beliefs or customs preserved by the lower classes of modern nations, many of which are certainly of very remote origin, although it is generally impossible to say where they originated, or whether they belonged to this or that people of antiquity. But there is evidence that one custom which appears utterly meaningless to those who have not inquired its original meaning, which is retained by peoples who have long forgotten what that meaning was, but which with others is still or was in comparatively recent times not merely a survival but a reality, existed among our neolithic ancestors. Every one has heard of the *couvade*, or hatching, which ordains that when a child is born the father should take to his bed, and there remain for days or weeks after the mother has resumed her ordinary mode of life. We learn from Greek writers that it prevailed among the ancient Corsicans,[1] the Tibareni of Pontus in Asia Minor,[2] and the Iberians of Northern Spain;[3] and with various modifications it exists or has existed among the Basques and the Caribs of the West Indies, in South America, California, Greenland, West Africa, Southern India, the Indian archipelago, and Eastern Asia. It originated in a belief that the real parent was the father, and that between him and his child there was a physical union so intimate that unless he rested and were nursed and abstained from ordinary food, his child would suffer. But this belief was not primitive. Matriarchy, it would seem, was the root of family life: descent was reckoned

pp. 191–232; E. B. Tylor, *Prim. Culture*, i, 1903, p. 295; *Bull. et mém. de la Soc. d'anthr.*, 5e sér., v, 1904, pp. 67–73; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 58. See also *Man*, v, 1905, No. 27, p. 49. A perforated skull which was found in an interment in Bute may perhaps show that the practice of trepanning existed in this country as early as the Bronze Age; but a physician who has examined the perforation believes that it was not produced by trepanning (*Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 67–8). Artificially perforated skulls were found just outside the Late Celtic fortified village of Hunsbury near Northampton. See A. Pitt-Rivers, *Excavations in Cranborne Chase*, vol. iii, unnumbered page following pl. ccxxviii.

[1] Diodorus Siculus, v, 14, § 2.

[2] Apollonius Rhodius, ii, 1011–4.

[3] Strabo, iii, 4, § 17.

through the mother, for the father was often unknown. It has been conjectured that when paternal relationship began to be acknowledged, fathers felt the need of insisting upon their rights, and that accordingly a parody of lying-in gradually became a custom.[1] An Irish legend shows that the *couvade* survived in Ulster into the Christian era ;[2] and a few years ago a similar custom was observed in a remote district of Yorkshire.[3] Although the peoples who have retained the *couvade* in modern times, like those among whom its existence was noted by ancient writers, are, with hardly an exception, neither of Aryan nor of Semitic origin, it is perhaps conceivable that it may have been brought into the British Isles in post-neolithic times by invaders who had accepted it from races whom they had subdued ; but it is far more probable that it was a widespread custom of the Neolithic Age belonging to tribes of the Mediterranean race, to which the neolithic Britons, as well as the Iberians and Corsicans, belonged.[4]

Hill-forts.

Although the neolithic tribes of Britain had common customs and superstitions,[5] and were, for the most part, sprung from one stock, they were not of course a nation. Arriving in successive hordes, and settling wherever they could find room, they were separated by mountain, stream, forest, and morass, as well as by the lack of horses, vehicles, and roads. But as their numbers multiplied and it became more and more difficult to find sufficient food, the struggle for life must have led to intertribal war, and men's minds must have been exercised to improve their weapons and to fortify their settlements and cattle-pounds not only against the wolves, which they had ever with them, but also against depredation. Every one who knows the

[1] See Lord Avebury's *Origin of Civilisation*, 1902, pp. 25, 157–68, and *L'Anthr.*, v, 1894, pp. 352–7. According to Dr. G. A. Wilken (*ib.*, p. 356), certain communities in the Indian archipelago who practised the *couvade* in 1894 were in a state of transition from matriarchy to father-right. Cf. *Man*, vi, 1906, No. 74, p. 112.

[2] *Rev. celt.*, vii, 1886, p. 227. [3] *Academy*, xxv, 1884, p. 112.

[4] E. B. Tylor, *Early Hist. of Mankind*, 1870, pp. 293–304 ; *Prim. Culture*, 1903, i, 84.

[5] See, however, pp. 117–8, *infra*.

South Downs and the hilly districts of the midlands, the west, and the north, has noticed the camps and earthworks which crown almost every height; but, as we have already seen, there are only a few of these entrenchments of which the period of construction is known, although we have abundant evidence that many have been occupied by successive races or in successive stages of culture. Almost all of them have been superficially explored, and implements of neolithic form have been found in many; but the reader knows that such implements were used in the Bronze Age and in the Iron Age even in those parts of the country where bronze and iron were common. If stone tools were found in the original body of a rampart or beneath the silt in a trench, without any objects of metal or any such tools or pottery as were characteristic of the Bronze Age or of later times, it might fairly be presumed that the people who built the camp were in their neolithic stage.[1] Except the camps that are known to be Roman, and others which have been proved by excavation to be Norman, most of those that have been thoroughly explored were evidently constructed after the art of metal-working had become known; and this is also true of those that have been scientifically examined in France.[2] There are, however, not a few British strongholds for which neolithic age has been claimed, though perhaps in some instances on insufficient grounds. Thus it has been asserted that Whit Tor camp on Dartmoor has yielded ample evidence of neolithic origin;[3] but all the excavations of hut-circles, kistvaens, and barrows that have been made on Dartmoor tend to show that it was not occupied before the Bronze Age.[4] A few of the pits which abound in the hill-fort of Eggardun in Dorsetshire have been explored; and it is said that one of them contained 'typical neolithic pottery'.[5]

[1] See *Trans. Epping Forest . . . Field Club*, ii, 1882, p. 60; iii, 1884, p. 228.

[2] *Bull. et mém. de la Soc. d'anthr. de Paris*, i, 1900, p. 53.

[3] *Journ. Brit. Archaeol. Ass.*, N. S., vii, 1901, p. 17.

[4] See p. 156, *infra*.

[5] *Proc. Dorset Nat. Hist. and Ant. Field Club*, xxii, 1901, pp. 28–42; *Proc. Soc. Ant.*, 2nd ser., xviii, 1901, pp. 258–62.

But, in the absence of an exact description of the vessels, such an argument is unsatisfactory, although it might have some weight if they resembled the coarse unornamented bowls which were found in the long barrow of North Bavant in Wiltshire [1] or the neolithic bowls of the Scottish chambered cairns.[2] Still there are entrenchments, such as Chanctonbury Rings,[3] on the downs some six miles north of Worthing, Beltout,[4] within which stands the Beachy Head lighthouse, the Maiden Bower camp near Dunstable,[5] and some on the Surrey Hills,[6] in and around which flint implements have been found in such profusion that they may be provisionally referred to the Neolithic Age.[7] Even

[1] *Archaeologia*, xlii, 1869, pp. 195, 232.

[2] It should be noted that pottery of the types that characterized the Bronze Age was still manufactured in the Early Iron Age. See p. 244, *infra*. Fragments of pottery were found in the long barrow of West Kennet in Wiltshire; but Thurnam (*Archaeologia*, xlii, 1869, p. 231) regards it as 'very doubtful whether they belong to the people by whom the chamber was erected'. They seem to have been portions of 'food-vessels', which belong to the Bronze Age (see p. 191, *infra*); and Pitt-Rivers (*Excavations in Cranborne Chase*, iv, 1898, pp. 100, 163) says that they 'must probably have got in subsequently to the construction of the barrow'. See also *ib.*, p. 147 and 162 (fig. 8); and, for examples of round-bottomed domestic pottery which have been found both in long barrows and in certain round barrows that may have been erected in the Stone Age, see *Brit. Barrows*, pp. 488–9, 509, and J. R. Mortimer, *Forty Years' Researches*, p. lxviii.

The Scottish chambered cairns have yielded more pottery than the English barrows. Most of the vessels lacked decoration; but some were ornamented either with cord patterns or by impressions of the potter's finger-tips and nails, or with vertical flutings; while a chambered cairn at Unstan, in Orkney, contained a vessel with triangular ornament of a kind which, as we shall see (pp. 197–8, *infra*), was characteristic of the Bronze Age. It may, however, have been manufactured at a time when bronze was coming into use in Southern Britain. See *Proc. Soc. Ant. Scot.*, xix, 1885, pp. 346–8, and J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 240, 248–9, 252, 272–3, 294–7.

Some of the 'drinking-cups' which have been exhumed from round barrows doubtless belong to the end of the Neolithic Age (see pp. 192–3, *infra*); and a curious vessel, which Bateman (*Vestiges of the Ant. of Derbyshire*, 1848, p. 43) described as 'a small drinking or incense cup of novel and unprecedented shape', was found in a round barrow the neolithic age of which is certain. See *Man*, vi, 1906, No. 44, pp. 70–1.

[3] *Archaeologia*, xlii, 1869, p. 44.

[4] *Ib.*, pp. 32–3. See also *Vict. Hist. of . . . Sussex*, i, 458, 461, 463.

[5] Worthington G. Smith, *Man, the Primeval Savage*, pp. 319–20; *Vict. Hist. of . . . Bedford*, i, 160.

[6] *Vict. Hist. of . . . Surrey*, i, 237–8.

[7] Gen. Pitt-Rivers (*Journ. Anthr. Inst.*, vi, 1877, pp. 359–60), after remarking

Cissbury camp, which contained numerous relics of the Early Iron Age, may have been constructed in the age of stone: a single cutting, only eleven yards long, revealed numerous worked flints lying, without pottery or metal, on the chalk bottom; and Pitt-Rivers suggested that the entrenchment might have been made for the protection of the mines.[1] It is true that no bronze implement was found, from which it might be argued that the camp was not constructed before the Iron Age: but, for aught that we can tell, bronze may still be lying beneath the soil; for the cost of excavating the whole camp, without which it is impossible to prove the negative, would be enormous. Certain small entrenchments in Franche-Comté were unquestionably constructed in neolithic times;[2] and it may be safely said that in an age when life and property were so insecure every isolated settlement must have been in some way fortified. Many of the entrenchments on the South Downs are, however, so slight that they could only have protected flocks and herds against wolves; and this may also have been the purpose of the thickset hedge, undoubtedly of prehistoric origin, that marks the line along which the downs were bounded by the Wealden Forest.[3]

that in the region between Seaford and Beachy Head 'the *débris* of flint manufacture is so far abundant on the surface . . . that the fact of finding flint flakes in the interior of these entrenchments [Seaford and Beltout] is no proof whatever of their being of the age of these entrenchments', goes on to say that 'this does not apply to other parts of the Downs of Sussex and elsewhere. There, worked flints are found in patches here and there; but considerable distances may be traversed without coming to these patches, and the fact of finding them in unusual numbers in the insides of these earthworks remains to testify to the probability of their having been used by the inhabitants of them.'

[1] *Journ. Anthr. Inst.*, v, 1876, pp. 383–4. Mr. George Clinch (*Vict. Hist. of . . . Sussex*, i, 316), referring to certain implements which he found himself at Cissbury, says, 'The position of the flint flakes and chips upon [the side of one of the mounds] proves that the earthworks were completed during the Neolithic Age'.

[2] *L'Anthr.*, xiv, 1903, pp. 444–62, and especially 450–2. See also A. Bertrand, *Archéol. celt. et gaul.*, 1889, p. 105, n. 1, and *Congrès internat. d'anthr. et d'arch. préhist.*, 1900 (1902), pp. 430–1.

[3] *Vict. Hist. of . . . Sussex*, i, 469–70. The authors of an interesting article in the *Cornhill Magazine* (May, 1906, pp. 611–2), which, however, contains some unverifiable statements, assert that earthworks of a peculiar form,

Primitive writing.

Although the historian who endeavours to press archaeology into his service is struck by the general similarity in material culture between the peoples of different lands, and is sometimes inclined, overlooking the differences in detail, to think that in describing one he would be describing all, he presently remembers that if historical records were to be destroyed, much the same state of things would confront the archaeologist of the remote future; and in his own researches he meets with differences which lead him to believe that in every land the first beginnings of a national culture and of a national character were already being evolved. In this country or in that significant relics are discovered of which in others there is not a trace. One of the more sensational discoveries of recent years may set us wondering whether in prehistoric Britain vestiges of primitive writing will ever come to light. Many people have heard vaguely of the painted pebbles and the frescoes of Mas d'Azil and the other caverns in the Western Pyrenees which the veteran archaeologist, Edouard Piette, has for many years diligently searched. On one of the objects found in the cavern of Lorthet—a spirited engraving on reindeer-horn representing reindeer and salmon—are to be seen two small lozenges, each enclosing a central line: 'justly proud of his work,' says Monsieur Piette, 'the artist has appended his signature.'[1] Be this as it may, other explorers have exhumed from the Placard cave at Rochebertier and the caves of La Madelaine and Mas d'Azil antlers incised with signs which exactly resemble various Greek and Phoenician letters, and may be compared with signs that have been found in an island of the Pacific. These signs are not letters but symbols: they are not combined in such a way as to form words or inscriptions.[2] But, says Monsieur Piette, being symbols, they do con-

'usually at the base of a hill on the edge of a plain,' were designed as a protection against wolves.

[1] *L'Anthr.*, xv, 1904, p. 159.

[2] *Ib.*, pp. 162, 165; *Man*, iv, 1904, No. 22, p. 37; *Fort. Rev.*, Oct., 1904, pp. 635–9. M. Piette, however (*L'Anthr.*, xvi, 1905, pp. 6–7), holds that certain symbols which he himself discovered in the cave of Gourdan form a real inscription.

stitute a kind of primitive writing.[1] True writing is, however, evident on a potsherd taken from a neolithic settlement at Los Murciélagos in Portugal.[2] If this fragment could itself be proved to be of neolithic age, it would follow that in that remote time the art of writing was already known to at least one branch of the Mediterranean stock. But not a trace of writing, not even one of the alphabetiform symbols which were so widespread in the Pyrenees even in the late Palaeolithic Age, has yet been found in any prehistoric deposit in this island.

Sepulture: barrows and cairns.

So far we have been trying to piece together an account of the life of neolithic man. But it is of the last scene of all that the vestiges which he has left behind are most unmistakable. His sepulchres have been thoroughly and scientifically explored. Moreover, it is from them that much of the knowledge which we possess of his daily life has been gleaned. They afford evidence about his political and social organization, his religion, and his customs; and when we have examined them we shall be able to form a more vivid idea of the way in which he lived.

We have seen that the dead were sometimes buried in caves wherein they or their forefathers had dwelled;[3] and the humbler folk who had not the means of erecting sepulchral monuments must have dug graves of which no apparent trace remains; but the funerals that have told their own tale were those of chieftains, their families, and perhaps their favourite slaves, who were buried beneath mounds which, in divers forms, are found all over the world.[4] Savage communities indeed are commonly ruled by councils of elders; but in the period when the neolithic barrows were being erected the Britons had certainly passed

[1] *L'Anthr.*, xv, 1904, p. 162.

[2] Gongora y Martinez, *Antigüdades prehist. de Andalucia*, 1868, p. 40, fig. 24. Cf. *Fort. Rev.*, Oct., 1904, p. 643. See also in regard to primitive writing *Journ. Anthr. Inst.*, xxix, 1899, pp. 204–6, *Rev. arch.*, 4e sér., i, 1903, pp. 231–2, and *Man*, iv, 1904, No. 22, p. 37.

[3] *Archaeol. Cambr.*, 4th ser., iii, 1872, p. 25; W. Boyd Dawkins, *Cave Hunting*, pp. 155–9; *Quart. Journ. Geol. Soc.*, lx, 1904, pp. 335–48.

[4] See *Mem. Anthr. Soc.*, i, 1865, p. 133, and W. Greenwell, *Brit. Barrows*, pp. 1–2.

beyond this stage. The means by which the revolution was effected were probably various. If the most adroit magician in a community of which every member practised magic may sometimes by force of character have made himself a chief,[1] it is certain that when property accumulated and group began to prey upon group, the instinct of self-preservation must have led men to submit to the rule of him who was marked out as the fittest to command in war.[2] Those who love to look for the places in this land that are hallowed by their associations with an older world may have seen the long barrows which are conspicuous on the hills that command Salisbury Plain and on other western heights, the chambered cairns of Scotland, and the dolmens of Cornwall and Wales. These sepulchres are far rarer than those of the Bronze Age, not more than sixty having been counted in Wiltshire, where they are most numerous, while the round barrows of the same county number nearly two thousand;[3] and the area of their distribution is far less extensive. In Gloucestershire, Somersetshire, and Dorsetshire they are not uncommon; a few are to be seen in the East Riding of Yorkshire; and Kent, Berkshire, Oxfordshire, Durham, Cumberland, and Westmorland have each one.[4] Chambered cairns which are related to chambered long barrows are found near

[1] See p. 58, *supra*.

[2] See *Folk-Lore*, xii, 1901, pp. 28–9.

[3] *Archaeologia*, xlii, 1869, p. 169. Mr. J. R. Mortimer (*Forty Years' Researches*, &c., 1905, p. lxxxi) is 'slightly inclined to consider that the long barrows [of the Yorkshire Wolds] . . . are more recent than the greater number of the round ones'; but the only reason which he gives for this singular opinion, namely, that he has frequently found both long and round skulls in the round barrows of the same district, has no weight against the facts which have led all other investigators to regard the long as earlier than the round barrows. See p. 393, *infra*. Not only has no metal ever been found with a primary interment in a long barrow, but sepulchral pottery is also wanting. See *Man*, v, 1905, No. 86, p. 159. If the contents of certain long barrows 'do not show any features of interest differing from those found in [some] round barrows' (*Forty Years' Researches*, p. xix), that only suggests that long barrows were still made for some time after the first interment in a round barrow took place. See W. Greenwell, *Brit. Barrows*, pp. 509, 556.

[4] J. B. Davis and J. Thurnam, *Crania Britannica*, ii, 1865, pl. 33, p. 2; *Archaeologia*, xlii, 1869, pp. 170, 176, 202, 206–7; *Mem. Anthr. Soc.*, iii, 1870, p. 41; W. Greenwell, *Brit. Barrows*, pp. 479, 484–511, 550–6; B. C. A. Windle, *Remains of the Prehist. Age*, pp. 155, 157, 159–63, 166–71; *Vict. Hist. of* . . .

St. Asaph and in Caithness;[1] and other chambered cairns and chambered round barrows, which belong to the latest period of the Stone Age or to a time of transition, exist in Orkney, Inverness-shire, Argyllshire and some of the adjoining islands, the Holm of Papa Westray, Derbyshire, Wales, Cornwall and the Scilly Isles, and the islands of the Channel.

The materials of which these monuments are composed vary of course according to the nature of the country in which they were erected. Stone was used where it was abundant, and earth or rubble where stone was not to be obtained. The significance of the barrows lies not in their substance but in their form; but it is probable that the absence of chambered barrows in South Wiltshire and Dorsetshire, where unchambered ones are common, is due simply to lack of the necessary stones.[3] The eminent Swedish archaeologist, Nilsson, argued that the 'passage-graves', or chambered barrows, of Scandinavia were designed on the model of subterranean dwellings; but the little evidence that remains tends to show that no such analogy existed here; and the Eskimos and Lapps, whose dwellings Nilsson had in view, bury their dead in tombs of a different kind.[4] Antiquaries who have had experience in opening

Durham, i, 200, 207. A few long barrows are said to exist in Lancashire, but it is doubtful whether they can really be classed as such (*Vict. Hist. of . . . Lancs.*, i, 211).

[1] W. Boyd Dawkins, *Cave Hunting*, p. 162; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 232–67; *Proc. Soc. Ant. Scot.*, xxxvi, 1902, pp. 39–42.

[2] *Archaeologia*, xlii, 1869, pp. 206–7; W. C. Lukis, *Prehist. Stone Monuments of the Brit. Isles,—Cornwall*, 1885, p. 13; J. Anderson, *op. cit.*, pp. 268–303; *Journ. Brit. Archaeol. Association*, N. S., vi, 1900, p. 7; *Journ. Anthr. Inst.*, xxxii, 1902, p. 404.

[3] *Archaeologia*, xlii, 1869, pp. 206–7; *Mem. Anthr. Soc.*, iii, 1870, p. 41.

[4] See S. Nilsson, *Primitive Inhabitants of Scandinavia*, 1868, pp. 124–58; *Journ. Ethn. Soc.*, N. S., ii, 1870, p. 448; W. Greenwell, *Brit. Barrows*, p. 536; *Archaeol. Review*, ii, 1889, p. 314; E. Cartailhac, *La France préhist.*, 1889, p. 195; and A. H. Keane, *Ethnology*, 1896, p. 126, note. The 'Picts' houses', whose resemblance to chambered *tumuli*, according to Thurnam (*Archaeologia*, xlii, 1869, pp. 223–4), is such that 'in particular instances it has been doubted whether the structure . . . was a dwelling or a tomb', belong to a much later period than that of the *tumuli*. See pp. 261 and 391, *infra*.

chambered and unchambered barrows consider that the two classes were erected in the same period;[1] and the nature of the interments, as we shall presently see, justifies this conclusion.

The orientation of the long barrows and of the chambered cairns which are classed with them seems to show that the builders intended that the spirits of the dead might look upon the rising sun. The axis of the barrow or cairn generally lies either due east and west or in a direction approximating more or less closely thereto; and the broader and higher end of the barrow, where, as a rule, the sepulchral deposits are found,[2] generally faces eastward. In a few instances the axis lies between the north and the south, the broad end pointing sometimes northward, sometimes southward. When the direction is not due east, it varies between north-north-east and south-east; and one may reasonably conclude that this variation depended upon the place of sunrise at the time of the year when the barrow was erected. Similar varieties, combined with the same general tendency to point the barrow towards the east, have been observed in the neolithic tombs of other countries.[3]

Long barrows vary greatly, not only in their materials and orientation, but also in their size and shape. Many of them exceed a hundred feet in length; and the chambered barrow of West Kennet is three hundred and thirty-five feet long and seventy-five broad at its eastern end.[4] More

[1] *Journ. Anthr. Inst.*, v, 1876, pp. 130–1; W. Greenwell, *Brit. Barrows*, p. 536, n. 2.

[2] Canon Greenwell (*ib.*, p. 485, n. 1) says that in North Gloucestershire 'the rule of the primary interment having been made at the larger end of the mound by no means holds good in all cases'. See also p. 504. In the Wor Barrow on Cranborne Chase the primary interments lay south of the centre of an oblong enclosure, which is described on p. 106, *infra* (A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 20–1 [preface]).

[3] *Archaeologia*, xlii, 1869, pp. 181, 208–9; *Mem. Anthr. Soc.*, iii, 1870, p. 41; W. Greenwell, *Brit. Barrows*, pp. 484, 487–8, 491, 497, 501, 505, 509, 511, 513, 515, 521, 524; *Dict. des. sc. anthr.*, 1883, p. 387; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 264–5; E. Cartailhac, *La France préhist.*, 1889, p. 183; *Proc. Soc. Ant.*, 2nd ser., xv, 1893–5, p. 404; W. C. Borlase, *Dolmens of Ireland*, ii, 489–90.

[4] *Archaeologia*, xlii, 1869, pp. 172, 208; J. B. Davis and J. Thurnam, *Crania Britannica*, ii, pl. 50, pp. 1–2.

striking, however, than the mere dimensions of a long barrow is the disproportion between its whole extent and that part of it in which alone the dead were laid. The immense toil which must have been expended in constructing such a monument by labourers who had only deer-horn picks and stone tools proves not only density of population, effective organization, and the despotism which the chiefs must have exercised, but also a religious

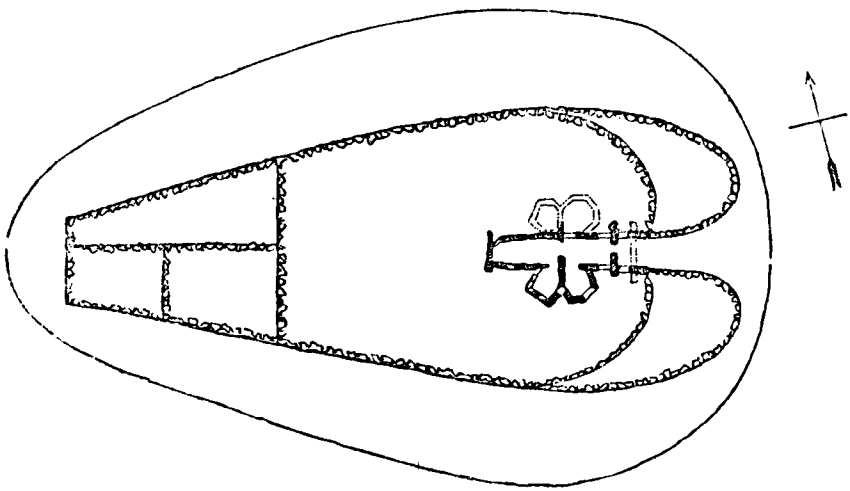


FIG. 16.

awe the compelling force of which we, who live in a world that has grown old, can hardly conceive. Some of the mounds might in outline be compared to a very elongated egg, others to one-half of a pear cut lengthwise and laid upon its flat side.[1] The trenches from which the material was excavated extend along their sides, but never encircle the ends.[2] The chambered barrows are of many kinds, no two being exactly alike. Some have a central gallery, entered by a doorway at the broad end, so low that it is necessary to stoop or even to crawl. Generally the cham-

[1] J. B. Davis and J. Thurnam, *Crania Britannica*, ii, pl. 5, p. 2; *Archaeologia*, xlii, 1869, p. 212, pl. xiv.

[2] *Ib.*, pp. 172-3, 209; W. Greenwell, *Brit. Barrows*, p. 544. See p. 177, *infra*.

bers, placed opposite one another in one, two, three, or even six pairs, open out of the gallery like the chapels in a Gothic cathedral; while occasionally, as at West Kennet, the gallery leads to a terminal chamber; and in other instances both lateral and terminal chambers are found. At Rodmarton and Nether Swell in Gloucestershire there is no gallery; and the chambers open externally. Galleries and chambers are alike built of stones set on edge, which (the interstices being filled in with dry walling) support flags laid horizontally across; though occasionally, as at Stoney Littleton in Somersetshire, the roof is constructed of converging layers of stones which form a rude arch.[1] Some so-called chambered barrows, for instance Littleton Drew in Wiltshire, have no chambers, but only cists, or shallow graves excavated in the soil and built up with stone slabs. The mounds were generally faced with dry walling; and on the chalk downs of North Wiltshire, where blocks of sandstone abounded, the wall was often, as at West Kennet, surrounded by a peristalith formed of stones erected at regular intervals. These stones have disappeared; but drawings, made in the seventeenth and eighteenth centuries, show what they were like.[2] The architects were inspired by a vivid sense of beauty. The enclosing wall, as it approached the broad and high end of the barrow, was turned inwards by gradual and graceful curves, which generally terminated in great stones that served as the jambs of the entrance. Even when there was no gallery, this symmetrical curve was still adopted, and its termination marked by monumental pillars.[3] The Wor Barrow on Cranborne Chase, an oval mound of such uncommon form that Pitt-Rivers, before he opened it, felt doubtful whether

[1] See p. 65, *supra*.

[2] Oval neolithic barrows, which were not only fenced by peristaliths, but also had ellipses of stone on the surface, and which, like the West Kennet barrow, were each surmounted by a dolmen, exist in Northern Germany, west of the Vistula (*L'Anthr.*, iv, 1893, p. 487), in Denmark (A. Bertrand, *Archéol. celt. et gaul.*, 1889, pp. 163–4), and in France (*ib.*, p. 166). Cf. *Archaeologia*, xlii, 1869, p. 165 (pl. xii, figs. 3 and 4), 211, note b.

[3] J. B. Davis and J. Thurnam, *Crania Britannica*, ii, pl. 24, pp. 2–3, pl. 5, p. 2; *Archaeologia*, xlii, 1869, pp. 209–21; W. Greenwell, *Brit. Barrows*,

it did not belong to the Bronze Age, appears to have been a chambered sepulchre of an abnormal kind. When the tumulus had been removed, a trench, enclosing an oblong space, appeared in the chalk which had formed the old surface. Stake-holes were detected in the trench; and the famous antiquary concluded that the stakes had been simply 'a wooden version of the long chambers of stone'.[1]

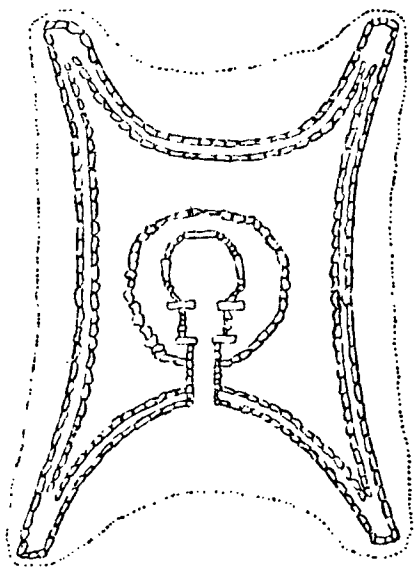


FIG. 17.

Intimately related to certain chambered long barrows are the famous horned cairns, which exist only in Caithness. Although their forms also are various, the larger cairn of Yarhouse being extremely elongated while that of Ormiegill might be almost exactly contained within a perfect square, the ruling idea remained the same. The exterior wall, which is always double, develops eastward and westward into horn-shaped projections, which curve outwards. Thus the four sides form four symmetrical concave curves; whereas in English chambered barrows, like that of Uley and some of the barrows at Upper Swell in Gloucestershire,[2] the curvilinear projections which correspond with the horns exist only at the eastern end. An opening between the eastern horns in the Scottish cairns gives access to the chamber, which is commonly divided into three partitions by two pairs of stones, crossing the side walls and leaving a passage between.[3]

p. 544; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 232.

[1] *Excavations in Cranborne Chase*, iv, pp. 20–1 (preface).

[2] *Journ. Anthr. Inst.*, v, 1876, p. 153, fig. 1, 165, fig. 1.

[3] W. Greenwell, *Brit. Barrows*, p. 536; J. Anderson, *Scotland in Pagan*

Just as the long are earlier than the short horned cairns, so the latter are earlier than the round chambered cairns of Scotland; for no horned cairns were erected after the Scottish Bronze Age had begun, whereas, although the round chambered cairns were developed towards the close of the Neolithic Age, and although metal has never been found in them,[1] their external form was reproduced in the Bronze Age, when chambers were no longer built.[2] The chambers of the round cairns also are divided into sections; and one of them, near Loch Etive in Argyllshire, shows traces of an encircling trench and rampart.[3] In Southern Britain the chronological sequence was probably the same: the round chambered cairns seem to be later than the chambered long barrows. The Park Cwm tumulus in the peninsula of Gower, which has a central avenue and two pairs of opposite chambers opening out of it,[4] has been likened to the Uley barrow; but its form is round. The chambered tumulus of Plas Newydd Park in Anglesey, which is roughly oval,[5] may possibly represent an earlier and transitional form.[6]

Times,—the Bronze and Stone Ages, pp. 232, 266–7; *Proc. Soc. Ant. Scot.*, xxxvi, 1902, pp. 39–42.

[1] J. Anderson, *op. cit.*, p. 300.

[2] See, however, p. 108, and p. 109, n. 2, *infra*. According to Dr. R. Munro (*Prehist. Scotland*, 1899, p. 325), 'Although many [Scottish] graves have been examined which contained . . . stone and nothing of bronze, it does not follow that they were earlier than others in which bronze articles were found. It seems to me', he continues, 'that the vast majority of the sepulchral memorials hitherto explored within the Scottish area date from the introduction of bronze'.

The evidence that the Scottish chambered tombs belonged to the Scottish Stone Age is precisely the same as that which is almost unanimously accepted for the English long barrows. Since we find that not a single article of bronze has ever been found with a primary interment in a Scottish chambered cairn, while bronze is abundant in the short cists and unchambered cairns of the same country; that the skeletons in chambered cairns belong to the same stock as the people who built the long barrows (see pp. 393–4, *infra*); and that the chambered cairns and the chambered long barrows are structurally akin, we may infer that the former, like the latter, belonged to the Stone Age. That, however, some of them may have been built after bronze had been introduced into Southern Britain is not improbable.

[3] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 249–50, 258, 264, 272, 274.

[4] *Journ. Ethn. Soc.*, N. S., ii, 1870, pp. 416–9.

[5] *Archaeol. Journal*, xxviii, 1871, pp. 85–96.

[6] An oval cairn, however, at Pawton in East Cornwall (W. C. Lukis, *The*

Round chambered barrows exist in Derbyshire, the design of which is purely local. Thus the Five-Wells barrow, near Taddington, has two chambers, each of which was approached by a gallery entered through a kind of port-hole on either side of the mound. The skulls that have been found in these tombs are of the neolithic type: but a barrow on Derwent Moor, which is commonly assigned to the same period,[1] contained an urn, ornamented with designs characteristic of the Bronze Age, in which a piece of copper was found;[2] and an experienced antiquary has remarked that in cataloguing the remains found in the Derbyshire barrows he 'found it almost impossible to separate the Neolithic from the Bronze Age interments'.[3] In West Cornwall also there are gigantic chambered cairns, round or oval, the date of which is uncertain. No bronze has been found in them, but abundance of pottery, and cists which are undoubtedly later than the chambers. One, standing on the cliff which rises above Cape Cornwall, contained a double-walled dome, and reminded its explorer of the huge tope at Bhojpur.[4]

Chambered cairns of a peculiar kind remain in Argyllshire and the islands of Islay and Arran, the like of which have been discovered nowhere else except on the opposite coast of Ireland.[5] Nearly all the pottery that has been

Prehist. Stone Monuments of the Brit. Isles,—Cornwall, 1885, p. 11) contains a cist, apparently contemporary with its erection, and is therefore presumably later than the chambered round barrows. It has been suggested (*Vict. Hist. of . . . Derby*, i, 175–6) that the oval form of some barrows may be due to the addition, following secondary interments, of new material.

[1] *Ib.*, pp. 166–9.

[2] T. Bateman, *Ten Years' Diggings*, &c., 1861, pp. 253–4. The Derwent Moor barrow was opened in 1780, when the art of excavation was in its infancy; and the urn in question may have belonged to a secondary interment.

[3] *Report of . . . the Brit. Association*, 1888 (1889), pp. 289–316. In regard to the chambered round barrows of Derbyshire, see also *Journ. Brit. Archaeol. Association*, N. S., vi, 1900, p. 7, and cf. W. Greenwell, *Brit. Barrows*, pp. 447–52.

[4] *Archaeologia*, xlix, 1885, pp. 189–92, 194–7; W. C. Borlase, *Dolmens of Ireland*, i, 145, 149, ii, 418, 441–2, 445–6, 448, 451, 462. See also Borlase's *Nenia Cornubiae*, 1872, p. 3, and *Vict. Hist. of . . . Cornwall*, i, 358.

[5] *Proc. Soc. Ant. Scot.*, xxxvi, 1902, pp. 74–181; xxxvii, 1903, pp. 36–67; xxxviii, 1904, pp. 17–81; *Journ. Anthr. Inst.*, xxxii, 1902, pp. 398–406. These cairns have no passages of entrance. Their outline was commonly rectangular,

found in them closely resembles that of the dolmens of North-Western France and the Pyrenees, while none exactly like it has been exhumed in England; and, combining these facts with the geographical position of the sepulchres themselves, the antiquary who has explored them concludes that their builders came late in the Neolithic Age from Brittany, and, sailing up St. George's Channel, settled on the opposite shores of Scotland and Ireland.[1] Physically, however, they belonged, as their skeletons show, to the same stock as the great majority of the neolithic people of Britain.[2]

the ground-plan being defined by flagstones, arranged at one end in a semi-circle, the space within which led to a low portal that gave access to the chamber. The latter consisted of two sections, one above the other, of which the lower was built of large lateral slabs, covered by flagstones, and divided by other slabs into compartments, while the upper was formed of small flags laid horizontally.

A chambered cairn of abnormal form in the island of Ronsay, Orkney, which has been described by Sir William Turner (*Proc. Soc. Ant. Scot.*, xxxvii, 1903, pp. 73–82), 'consisted of a central part and four recesses' (*ib.*, pp. 74–5, fig. 1); and on its roof were cremation cists 'quite different in character from the short cists so frequently found in Scotland' (*ib.*, p. 79).

[1] *Journ. Anthr. Inst.*, xxxii, 1902, pp. 398, 405; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 78.

[2] *Ib.*, xxxvi, 1902, pp. 154–5. The chronology of the peculiar chambered cairns of South-Western Scotland is somewhat perplexing. On the one hand the structure of the cairns is presumptive evidence that they were built in neolithic times (J. Anderson, *Scotland in Pagan Times,–the Bronze and Stone Ages*, pp. 271–2; *Proc. Soc. Ant. Scot.*, xxxvi, 1902, p. 136); the successive interments which were made in them were characteristic of the same period (*ib.*, p. 134); the pottery which they contained is almost all of neolithic type (*ib.*, pp. 165–7; xxxviii, 1904, pp. 78–9); and the presence of drinking-cups does not necessarily point to a later date (p. 193, *infra*). On the other hand a perforated stone hammer, which was found in one of the chambers (*ib.*, xxxvi, 1902, p. 100), belongs to a class of implements which in this country were generally post-neolithic (p. 78, *supra*); an elegant bowl which was obtained by Canon Greenwell in a cairn on Largie Farm, near Crinan (*Proc. Soc. Ant. Scot.*, xxxvi, 1902, pp. 165–7), although neolithic in form, is somewhat similar to a food-vessel figured in *Brit. Barrows*, p. 88, fig. 73, which, like it, is ornamented with vertical flutings; and one of the drinking-cups deposited in the same cairn is 'almost identical in size, shape, and ornamentation' with a specimen that was associated with a bronze dagger in a barrow on Roundway Hill, Wiltshire (*Proc. Soc. Ant. Scot.*, vi, 1867, pp. 345, n. 1, 347). I conclude that these cairns were locally of neolithic age, but that the influence of the later culture had made itself felt in the district,—in short, that they belonged to a period of transition.

Inhumation and incineration.

Here, as also in France[1] and Northern Germany,[2] funerals were performed both by inhumation and incineration. In the barrows of South-Western Britain, cremation, although not unknown, was very rare;[3] in Yorkshire[4] and the chambered cairns of Bute,[5] almost universal. Judging from the analogy of other countries and from the fact that inhumation persisted into the Bronze Age, and then for a long period was generally superseded by cremation,[6] it seems probable that the latter was not introduced until a comparatively late epoch.[7] The two modes of burial were, however, contemporaneous not only in different parts of the country but in the same district and in the same grave. Burnt and unburnt bones have been found lying together in such a manner as to prove that they had been interred at the same time.[8] Cremation was generally performed in the chamber or on the floor of the barrow where the body was deposited.[9] When the corpse was buried entire, it was usually laid upon the ground[10] with the knees doubled up towards the chin, or placed sitting in a similar posture by the side of the tomb.[11] This custom, which was

[1] M. de Baye exaggerates when he says (*L'archéol. préhist.*, 1888, p. 108) that in France inhumation in the Neolithic Age was almost universal. M. E. Cartailhac (*Matériaux pour l'hist. . . . de l'homme*, xxii, 1888, pp. 1–2, 4, 6–7; *La France préhist.*, 1889, pp. 270–6) gives numerous instances of incineration in neolithic tombs in the departments of the Aisne, the Marne, the Morbihan, &c.

[2] W. C. Borlase, *Dolmens of Ireland*, ii, 520.

[3] *Archaeologia*, xlii, 1869, pp. 191–2, 224–6; *Journ. Anthr. Inst.*, v, 1876, p. 129.

[4] *Ib.*; *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 53.

[5] *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 81. Cremation was common in the Neolithic Age in Derbyshire (*Report of . . . the Brit. Association*, 1888 [1889], p. 316).

[6] See, however, p. 187, *infra*.

[7] See *Journ. Anthr. Inst.*, v, 1876, pp. 130–1.

[8] W. Greenwell, *Brit. Barrows*, pp. 690–1; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 244–5, 250, 274, 293, 300; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 42.

[9] W. Greenwell, *Brit. Barrows*, p. 506; *Guide to the Ant. of the Stone Age* (Brit. Museum), p. 72.

[10] In a long barrow at Upper Swell in Gloucestershire (W. Greenwell, *Brit. Barrows*, pp. 524, 526, 536) the primary interment was in a true grave. No similar interment, so far as I know, has been found in any long barrow.

[11] *Archaeologia*, xlii, 1869, pp. 189, 224–5. Sir J. Evans (*Archaeol. Journal*, xxxv, 1878, p. 266) gives an instance of a skeleton of neolithic age found in the extended position near Daventry in Northamptonshire.

almost universal in prehistoric times, and is still practised by many savages, is best explained by the assumption that it was thought seemly to bury the dead in the position in which they had slept, and that, for the sake of warmth, they had commonly lain down to rest in an attitude which most of us have occasionally adopted for the same reason.[1] In some barrows only single skeletons have been found; but generally in unchambered barrows, where more than two persons had been buried in one grave, the bones lay heaped together as though the bodies had been unceremoniously flung down;[2] while in certain cases they were found disjointed in such wise that it was evident that the dead had not been buried entire, or, as is often the case in savage countries and even in Brittany and the Catholic cantons of Switzerland, until long after the flesh had decayed.[3] The Balearic islanders, in the time of Diodorus Siculus,[4] used to sever the bodies of their dead in pieces and inter them in urns; and the same practice prevailed in Spain in the Age of Bronze.[5] British explorers, moreover, have often noticed, in opening barrows, that skeletons were incomplete, many of the bones being absent.[6] Since the piled skeletons belonged to old and young, male and female, it can only be concluded that corpses were often stored, as in a mortuary, until a sufficient number had accumulated, and then buried all together.[7] In a barrow

[1] *Archaeologia*, xlii, 1869, p. 189; W. Greenwell, *Brit. Barrows*, pp. 23–4.

[2] *Archaeologia*, xv, 1806, p. 339; xix, 1821, pp. 43–4; xlii, 1869, pp. 184–5.

[3] *Ib.*, pp. 190–1; W. Greenwell, *Brit. Barrows*, p. 504; A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 20–1 (preface); *Guide to the Ant. of the Stone Age* (Brit. Museum), pp. 72–3. In the Wor Barrow on Cranborne Chase (see p. 105, *supra*) Pitt-Rivers discovered six skeletons 'huddled together beneath a small mound', which was within and distinct from the monument itself. Three of them were crouched, and the rest 'put in with them as bones, with the long-bones laid out by the side of the skull'. Referring to a discovery made in Egypt by Professor Flinders Petrie, Pitt-Rivers observes that the bones of a skeleton of the Fifth Dynasty (about 3500 B.C.) 'had been cut up and put in a box, with an effigy of the deceased by the side of it. Something of this sort', he continues, 'may have occurred here'.

[4] v, 18, § 2.

[5] *Guide to the Ant. of the Stone Age* (Brit. Museum), p. 73.

[6] *Archaeologia*, xlii, 1869, pp. 184–5; W. Greenwell, *Brit. Barrows*, pp. 500–1.

[7] *Journ. Anthr. Inst.*, v, 1876, pp. 134–8. Even in cremation deposits the

situated at Upper Swell in Gloucestershire, Rolleston found evidence which convinced him that interments were sometimes made successively upon the same spot. An undisturbed skeleton was here surrounded by a great quantity of bones, the arrangement of which was such that he was forced to conclude that they had been displaced in order to make room for it.[1] In chambered barrows successive interments were of course regular, gallery and chamber being designed to admit them.

Human sacrifice.

Thurnam was convinced that in the barrows which he explored there were unmistakable evidences of human sacrifice. In nearly all of them he found fractured skulls, the broken edges of which were so sharp that he inferred that the skull had been cleft in life by a club or a stone axe; while in some cases one skull only was unmutilated. His conclusion was that the few entire skulls were those of chiefs or their relatives, while the others belonged to slaves or captives who had been sacrificed. In one instance, in which only two interments were met with, the broken skull was that of a woman, while the bones of the other corpse, which belonged to a man, had been imperfectly burned. Thurnam argued that the burnt bones belonged to a chief, and that the woman was his wife.[2] Rolleston, on the other hand, could see no reason for believing that the broken skulls had been cleft deliberately.[3] He pointed out that the fragments were so numerous that if the persons to whom they belonged had been sacrificed, they must have been slaughtered by a succession of wanton blows; that the fractures were utterly different from those of skulls which are known to have been broken by deliberate blows, and resemble those which have been caused by the shifting

bones are often imperfect and disconnected; and, previous to cremation, the bodies must have been stored in an ossuary (W. Greenwell, *Brit. Barrows*, p. 547).

[1] *Ib.*, pp. 527, 533–4, 547–8

[2] *Archaeologia*, xlii, 1869, pp. 185, 191–2, 222, 227; *Mem. Anthr. Soc.*, iii, 1870, p. 76.

[3] W. Greenwell, *Brit. Barrows*, pp. 686–93. See also Canon Greenwell's remarks on pp. 544–5. Although he agrees in the main with Rolleston, he admits the probability that Thurnam 'found signs of violent breakage upon a few skulls'.

of soil or the collapse of stones; and he argues that from what we know of the sentiments of savage and barbarian peoples it is in the last degree improbable that slaves or captives, if they had been sacrificed, would have been allowed to repose side by side with their lords. Nevertheless it is not safe to reject all the evidence which Thurnam adduced. In a round barrow near Stonehenge Hoare found a skull which appeared to have been cut in two as deftly as by a surgical instrument;[1] and one may believe that what was done in the Bronze Age was not unknown in the Age of Stone. When we remember that evidences of human sacrifice have been detected in French neolithic tombs,[2] and that the practice was universal in ancient times,[3] we shall be safe in assuming that neolithic Britain was no exception to the rule that after a chieftain's obsequies his dependents were immolated in order that their souls might be set free to minister to his.[4]

Traces (?) of cannibalism.

But Thurnam also believed that the long barrows contained evidences of cannibalism.[5] The numerous passages in which ancient writers accused the inhabitants of the British Isles of devouring their own kind refer mainly to the Irish:[6] but they were speaking of their contemporaries; and when some of the Yorkshire barrows were opened it was evident that the flesh had been removed from the bodies before they were interred.[7] But even if cannibalism was practised in our Neolithic Age, the motive was not hunger. The numerous bones of oxen, swine, red deer, goats, and horses[8] which are found in the barrows, mingled

[1] *Archaeologia*, xxxviii, 1860, p. 421.

[2] Gen. Pothier, *Les tumulus du plateau de Ger*, 1900, pp. 30–1.

[3] See the references in Greenwell's *Brit. Barrows*, p. 685.

[4] See E. B. Tylor, *Prim. Culture*, i, 1903, pp. 458–67.

[5] *Archaeologia*, xlii, 1869, pp. 188, 222. Cf. *Bull. de la Soc. d'anthr. de Paris*, 2e sér., ii, 1867, pp. 326–32; J. R. Mortimer, *Forty Years' Researches*, p. xxiv, and W. Greenwell, *Brit. Barrows*, p. 15; and, for a valuable caution against forming hasty inferences as to cannibalism, *Journ. Derby. Archaeol. and Nat. Hist. Soc.*, xv, 1893, p. 162.

[6] See p. 268, n. 1, *infra*.

[7] W. Greenwell, *Brit. Barrows*, pp. 486, 499, 544.

[8] 'Remains of the horse,' says Lord Avebury (*Prehist. Times*, 6th ed., 1900, p. 160, with which cf. p. 152, n. 5, *infra*), 'are very rare in English barrows,

with fragments of pottery, prove that a funeral was an occasion for a feast, and may show that, as in later times, offerings were made to the ghosts of the dead.[1] If human flesh was eaten, it was doubtless in the hope that moral qualities which had distinguished the dead might be absorbed by the living.[2]

Interment of animals.

Perhaps the most curious feature in neolithic interments is that animals were sometimes buried entire.[3] It is not indeed surprising that at Eyford in Gloucestershire there was buried with a woman a dog which may have been her companion;[4] but in a long barrow near Stonehenge was found the skeleton of a goose which had evidently not been eaten.[5] Was it a sign that neolithic people had the same religious prejudice against eating geese which Caesar noted,[6] or had this goose been sacrificed?[7]

Religion.

We can hardly err in regarding the sepulchral monuments on which such stupendous labour was expended as witnesses of a belief which may be called religious, and perhaps as a further illustration of the apophthegm, 'The first begetter of gods on earth was fear'.[8] For if the spirits of ancestors are believed by savage tribes to be on the whole well disposed towards those whom they leave behind, yet when their bodies do not receive due burial their wrath is terrible.[9] The most eminent of modern French archaeologists maintains that the dolmens, chambered tombs, and standing stones of France were erected under the influence of Druids;[10] and in

and I know no well authenticated case of their occurrence in a long barrow'. See, however, *Archaeologia*, xlii, 1869, pp. 228–9. In Gaul at all events in the Neolithic Age horses abounded (*Association franç. pour l'avancement des sc.*, 32e sess., 1903, 2e part., p. 851).

[1] *Archaeologia*, xlii, 1869, pp. 182–3, 227–8, 237–8, 241?

[2] See E. B. Tylor, *Early Hist. of Mankind*, 1870, p. 131.

[3] *Trans. Ethn. Soc.*, N. S., iii, 1865, p. 317.

[4] W. Greenwell, *Brit. Barrows*, p. 735.

[5] *Archaeologia*, xlii, 1869, p. 183.

[6] *B. G.*, v, 12, § 6.

[7] See W. Robertson Smith, *The Religion of the Semites*, 1901, p. 350.

[8] Primus in orbe deos fecit timor (Statius, *Theb.*, iii, 661). Cf. E. B. Tylor, *Prim. Culture*, ii, 1903, p. 230.

[9] *Ib.*, pp. 28, 111, 113.

[10] *Acad. des inscr. et belles-lettres*,—comptes-rendus des séances de l'année 1892, 4e sér., xx, 6–7.

this country also the belief has long been growing that Druidism was of non-Celtic and neolithic origin: but since our knowledge of it is confined to the period when it was a Celtic institution, we must defer our consideration of it.[1] But, apart from the graves themselves, there is hardly any certain evidence in our neolithic interments of religious belief. While in France, Scandinavia, Northern Germany, and other lands, the tombs of this period were stored with implements, ornaments, and weapons, the spirits of which were doubtless consecrated to the service of the dead,[2] such relics are so rare in Britain [3] that unless the barrows were despoiled in bygone days by heedless explorers, we can only suppose that it was not generally thought necessary to provide those who had passed away with the means of continuing their life in another world; and it may be that the few arrow-heads, flakes, and other objects which have been found in graves were rather intended as marks of reverence or affection than for use.[4] On the other hand, some of the implements found in neolithic barrows are said to have been intentionally broken; [5] and this is often done by savages in the belief that the souls of the implements [6] may thus be set free to be of use to the spirits of the dead.[7] The holes that are to be seen in the stones of dolmens in many lands are here so rare [8] that we may hardly regard

[1] See pp. 289–98, *infra*.

[2] *L'Anthr.*, iv, 1893, p. 488; *Archaeologia*, xlii, 1869, p. 232.

[3] *Ib.*, pp. 193, 229, 232; W. Greenwell, *Brit. Barrows*, p. 543.

[4] See *Trans. Ethn. Soc.*, iii, 1865, p. 316, and cf. W. Greenwell, *Brit. Barrows*, pp. 57–60, 120–1, whose remarks, though they apply to the round barrows of the Yorkshire Wolds, are relevant.

[5] *Journ. Anthr. Inst.*, vi, 1877, p. 500 (with which cf. p. 293); W. Boyd Dawkins, *Early Man in Britain*, p. 287. Cf. J. R. Mortimer, *Forty Years' Researches*, p. 1.

[6] See pp. 201–2, *infra*.

[7] See Lord Avebury's *Origin of Civilisation*, 1902, pp. 35, 301. Professor Boyd Dawkins misunderstands the custom.

[8] See pp. 288, 403, *infra*; *Archaeol. Cambr.*, 3rd ser., x, 1864, pp. 292, 296, 298; *Archaeologia*, xlii, 1869, pp. 216–7; and W. C. Borlase, *Dolmens of Ireland*, i, 68, 75, 95, 174. Numerous holed stones exist in the vicinity of barrows and stone circles, which were probably erected in the Bronze Age, in Cornwall, Ross-shire, Inverness-shire, the Orkneys, and the island of Arran; but their significance is unknown. There are holed megaliths in Britain (one of which has in recent times been used for curing weakly children, whose mothers passed

them as evidence of a belief that spirits must be allowed an exit from their graves; although such a belief has been common to many peoples, and may even linger on among ourselves, as in France and Germany, in the superstition which often impels survivors to open door or window when life is ebbing away [1]. It must be confessed that we know little more of neolithic than of palaeolithic religion. Fetichism, which is ubiquitous—the belief that spirits inhabit or operate through stocks and stones and what not; the belief by which the Dorsetshire peasant who treasures his holed pebble for luck is still animated—may be assumed to have belonged to both.[2] The worship of saints may be a survival of the worship of ancestors.[3] The traces of the adoration of wells and lakes and rivers which may still be observed in the remoter parts of Great Britain and Ireland, where peasants offer pence to the spirit of the spring, and children were lately bidden to beware of the river-sprite who was waiting to drown them, are undoubtedly linked to a prehistoric faith; [4] and so is that superstition which prevails in New Zealand, in the Malay Archipelago, and on the banks of the Ganges, and which among the islanders of St. Kilda and the Shetlanders of Scott's day impelled men to refuse aid to a drowning comrade because they feared to balk the marine demon of his prey.[5] Nor need we doubt that, like other savages, our neolithic forefathers saw sun, moon, and stars as living beings, or that, like the Australian aboriginals and the nameless tribes who passed on to the Greeks the myths which were by them invested with poetic form, they invented stories to account for the wonders which they saw in the starry heavens.[6]

them through it) that do not belong to dolmens (W. C. Lukis, *Prehist. Stone Monuments of the Brit. Isles,—Cornwall*, p. 17).

[1] E. B. Tylor, *Prim. Culture*, i, 1903, p. 454. Cf. *Rev. des études anc.*, vii, 1905, pp. 31–2, and p. 288, *infra*.

[2] See E. B. Tylor, *Prim. Culture*, 1903, i, 477, ii, 144–5, 159, 242–3.

[3] *Ib.*, i, 120.

[4] *Ib.*, ii, 209–14; G. L. Gomme, *Ethnology in Folk-lore*, 1892, pp. 78–9; Lord Avebury, *Prehist. Times*, 1900, pp. 207–8. See also W. Robertson Smith, *The Religion of the Semites*, 1901, p. 182.

[5] E. B. Tylor, *Prim. Culture*, i, 1903, pp. 108-10; ii, 209.

[6] *Ib.*, i, 357–8. Cf. A. Lang, *Custom and Myth*, 1885, pp. 124, 131, 137, 142.

Neither need we hesitate to believe that, as each clan had its chief, so the clansmen saw, above elves and kelpies, gnomes and goblins, rock-spirits and tree-spirits, the mightier deities of Heaven and Earth, Sun and Moon, Fire, Water, and Thunder.[1] We may believe, if we please, that they prayed, as savages, nay Christians, often pray, not that they might become better, but that they might be better off.[2] We may suppose too that magic, which is even now used in remote villages as an engine of extortion,[3] was still a power by which men strove to ensure supplies of food or to make rain fall in time of drought, perhaps also a weapon by which the man of intellect made himself obeyed. But when we consider the infinite variety of forms which superstition assumes, we see that it would be vain to contend that any one belief now held by this or that savage tribe was identically part of the faith that was professed in Britain in the Neolithic Age. Even the fancy that an ethereal soul survived bodily death may not have been universal; and as the Tonga islanders and the Virginians are said to have believed that only the souls of chiefs would live again,[4] so it is conceivable that the slaves by whose sweat were built the barrows in which their lords were to be interred were regarded as doomed to annihilation. And when we are told that some quaint superstition which the folklorist discovers in Devonshire or the Highlands is non-Aryan, and must therefore be traceable to the people who were here before the first Celtic invader arrived, we may ask how it is possible to disprove that it had been inherited by the Celt from remote ancestors or had been borrowed by him from non-Aryan tribes while he was still a wanderer. We must be content, if we can but catch something of the spirit of neolithic religion, to remain in blank ignorance of its details. We must keep in mind that in unnumbered centuries it cannot have remained the same, and that in

[1] E. B. Tylor, *Prim. Culture*, ii, 1903, pp. 186, 248–9, 255.

[2] *Ib.*, p. 364. Cf. *Journ. Anthr. Inst.*, xxxiv, 1904, p. 264, and Mr. R. R. Marett's interesting article, 'From Spell to Prayer' (*Folk-Lore*, xv, 1904, pp. 132–65).

[3] *L'Anthr.*, xv, 1904, p. 120; A. Lang, *The Clyde Mystery*, 1905, pp. 82, 89, 99.

[4] E. B. Tylor, *Prim. Culture*, ii, 1903, p. 22.

diverse regions its manifestations must have been various. We must not ask for more than the assurance that to the herdsmen who pastured their cattle on our downs all Nature was animated; that in their eyes 'as the human body was held to live and act by virtue of its own inhabiting spirit-soul, so the operations of the world seemed to be carried on by the influence of other spirits';[1] and that, like all savage and half-savage peoples, they were enslaved by custom, fettered by taboos, and compelled, when they were driven by necessity to violate them, to expiate their offence by complex rites.[2] It may, however, be presumed that the religion of neolithic man progressed when he ceased to be a wanderer, and especially when he began to till the soil. Supernatural beings were not of necessity gods to be worshipped; but when the god of a community became the lord of its land, he was its protector, nay, its father, who, in return for due reverence and sacrifice, would do his utmost to guard it against human enemies and hostile deities.[3]

And perhaps, since primitive worship concerned the community rather than the individual,[4] common superstitions and participation in sacrificial feasts were already beginning to do their work of creating the sense of kindred between divers groups, out of which, ages later and after successive new invasions, war and policy were to develop a state.[5]

We have gathered some scraps of information from the tools and weapons and pottery, the dwellings and mines, the graves and the skeletons of neolithic man. Can these dry bones live? Only for him who has imagination, which, as the historian whose own was supported by a vast armoury

[1] E. B. Tylor, *Prim. Culture*, ii, 1903, p. 185.

[2] See W. Robertson Smith, *The Religion of the Semites*, 1901, p. 152; Lord Avebury, *Origin of Civilisation*, 1902, pp. 466–70; *L'Anthr.*, xvi, 1905, p. 660; and A. Lang, *The Secret of the Totem*, p. 2.

[3] See W. Robertson Smith, *The Religion of the Semites*, 1901, pp. 38, 41, 92, 119, &c.

[4] *Ib.*, pp. 253–6, 263.

[5] *Journ. Anthr. Inst.*, xxviii, 1899, p. 145; E. B. Tylor, *Prim. Culture*, ii, 1903, p. 370.

of solid knowledge declared with splendid paradox, 'is the mother of all history as of all poetry.'[1] It is not when we are reading the memoirs in which discoveries are recorded, not when we are wandering through the galleries of a museum, that those happy moments come in which we discern the faint outlines of the prehistoric world, but rather when we are roaming over sand or moor or upland, looking for the tools that those old workers wrought, in the midst of the monuments which their hands upreared. Not the outward life alone comes back to us—the miner with lamp and pick creeping down the shaft; the cutler toiling amid a waste of flints; herdsmen following cattle on the downs; girls milking at sundown; lithe swarthy hunters returning from the chase; fowlers in their canoes gliding over the meres; serfs hauling blocks up the hillside to build the chambers in yonder barrow; the funeral feast; the weird sepulchral rites; the bloody strife for the means of subsistence between clan and clan:—we think also of the meditations of the architects who created those monuments in memory of the dead and of the adventurous lives of those who were thus honoured; of their survivors' desperate denial of death's finality; of the immeasurably slow, age-long movement of expanding civilization; of the influence of superstition, paralysing, yet ever tending to consolidate society; of the enthusiast whose thoughts soared above the common level; of the toil that spent itself in millenniums past, but is still yielding fruit; of unrecorded deeds of heroism and of shame; of man's ambition and of woman's love.

An alien invasion; period of transition.

Before the Neolithic Age came to its end invaders began to appear who had not yet learned the art of metal-working, but who belonged to a race of which the people in possession knew nothing.[2] Sepulchral customs began to change. Long barrows were erected still, but, as in France, Holland, and other lands,[3] mounds of circular form were rising, and at

[1] '. . . die Phantasie, welche wie aller Poesie so auch aller Historie Mutter ist' (Th. Mommsen, *Röm. Gesch.*, v, 1885, p. 5).

[2] See pp. 408–9, *infra*.

[3] W. C. Borlase, *Dolmens of Ireland*, ii, 516–7, 549, 563; *Rev. de l'École d'anthr.*, xv, 1905, pp. 213–4. The rarity of long barrows may partly be

last supplanted them. It was a time of transition; and although in the far west and the far north the Stone Age lingered on, another was approaching, which had long since dawned in more favoured lands,—the Age of Bronze.

explained by supposing that a certain proportion of the others belonged to the late Neolithic Age. That some did is certain. See *Proc. Soc. Ant. Scot.*, xxxvi, 1902, pp. 159, 171, and pp. 408–9, *infra*.

CHAPTER IV

THE BRONZE AGE AND THE VOYAGE OF PYTHEAS

THOSE who have learned to realize the extreme slowness with which material culture was evolved in its earlier stages would be disposed to doubt whether the first metallic implements were made of bronze, and to ask whether, at all events in some part of the world, the Neolithic must not have merged into a Copper Age. It is easy to imagine that the accidental melting of a piece of copper ore may have suggested the possibility of fashioning the metal into tools; and that inventive cutlers took impressions of stone axes in clay, and found that they could make from them copper axes which were not liable to break: [1] but one can hardly believe that simultaneously the discovery should have been made that the softness and bluntness of copper could be remedied by mixing with it a small proportion of tin.

A Copper Age preceded the Bronze Age in certain countries, but has not been proved to have existed in Britain.

[1] M. Salomon Reinach (*L'Anthr.*, xvi, 1905, p. 659) characteristically remarks in regard to the discovery of metals that 'On l'explique ordinairement par une succession de hasards heureux, en oubliant que l'humanité primitive, n'ayant aucune idée de l'utilisation industrielle des métaux, ne pouvait en arriver là du premier coup . . . Aujourd'hui toute la métallurgie primitive me semble un chapitre de l'histoire des religions . . . On a soumis ces métaux [gold and tin] à l'action du feu, au cours d'opérations magiques; ainsi naquit l'idée de traiter de même les minerais de cuivre . . . et d'en dégager le métal brillant qui ressemble à l'or . . . L'alchimie primitive, absolument étrangère à toute application industrielle, chercha à manier des substances divines par l'action du feu, à opérer . . . des *hiérogamies* analogues à celle qui conduisit les agriculteurs à la découverte de la griffe. L'alliage du bronze fut un des résultats de leurs efforts.'

That smiths were sometimes regarded with superstitious awe by those who did not share their secrets (O. Schrader, *Prehist. Ant. of the Aryan Peoples*, pp. 165–8); that metallurgy was connected at various points with religion;—so much may be granted. But to say that 'primitive alchemy' (if it existed) had no industrial application is simply to make an unverifiable and improbable assertion. The discovery that ores could be smelted must have been accidental. Why should not the 'alchemist', however superstitious he may have been, have thereupon conceived the idea of turning gold to account for the manufacture of ornaments, or copper for that of axes?

It is indeed not inconceivable that bronze was the first metal which was ever manufactured; for near the surface copper ores often contain tin oxide; and it has been proved that by smelting such ores bronze can be produced.[1] But of course only experiment could have shown that tools made of this metal were better than copper. The Egyptians were acquainted with the use of copper long before they began to manufacture bronze;[2] and in many parts of the British Isles as well as of the Continent copper implements have been discovered which belonged to prehistoric times.[3] But such discoveries do not necessarily prove the existence of a Copper Age: they may often be accounted for by the supposition that tin, which is far less widely distributed than copper, was temporarily wanting. In many cases implements of copper and of bronze have been met with in intimate association; and sometimes copper implements of advanced type with primitive bronze.[4] When, on the other hand, copper implements are repeatedly found in deposits which are known to be older than the oldest bronze in the districts in which they occur, the conclusion is irresistible that they were used there before bronze was manufactured.[5] There was certainly a Copper Age in Mesopotamia, Egypt, and Cyprus; and probably also in Hungary, Northern Italy, Spain, and Ireland, with which, in ancient times, Spain was closely connected, and in which copper celts were unmistakably modelled upon those of stone: but for Britain the evidence is not sufficient.[6] We must

[1] *Proc. Soc. Ant.*, 2nd ser., xvi, 1895–7, p. 333.

[2] See *Man*, ii, 1902, No. 19, p. 29.

[3] *Journ. Anthr. Inst.*, xxxi, 1901, pp. 278–9; *Man*, iv, 1904, No. 5, pp. 13–4.

[4] *Rev. mens. de l'École d'anthr.*, iii, 1893, pp. 227–9; *Report of . . . the Brit. Association*, 1896, p. 911.

[5] See *L'Anthr.*, iv, 1893, p. 559; xvi, 1905, p. 198.

[6] *Congrès internat. d'anthr. et d'archéol. préhist.*, 1900 (1902), p. 340; *Vict. Hist. of . . . Hertford*, i, 232; C. H. Read, *Guide to the Ant. of the Bronze Age* (Brit. Museum), 1904, pp. 5–6, 27, 101, 111, 130. M. P. du Chatellier (*Rev. de l'École d'anthr.*, xiii, 1903, pp. 169–72) thinks that there was a copper age in Brittany, but admits that he cannot settle the question. Professor O. Montelius affirms (*Man*, v, 1905, No. 7, p. 13) that 'copper had been used there [in Britain] for a long time' before bronze; but British archaeologists do not bow to his authority. Pitt-Rivers (*Journ. Roy. United Service Inst.*,

assume then provisionally that in our island the metal which was first used for cutting-tools was bronze.

Bronze implements used for many centuries in Europe before the Iron Age.

Certain metallurgists, however, maintain that a Bronze Age, properly so called, may never have existed; and that iron may have been manufactured during and even before the period to which the bronze tools that are exhibited in museums belong. Iron was undoubtedly known to the Egyptians at a very remote date, perhaps as early as bronze.[1] Primitive methods of extracting iron from its ore, which are still practised in India and Africa, require far less skill than the manufacture of bronze: the metallurgists argue that since iron is rapidly oxidized by air and moisture, the iron tools which they assume to have been made in the so-called Bronze Age must have perished in the conditions to which most of the bronze tools that have been discovered were exposed; and they insist that iron tools have actually been found in association with objects of the early Bronze and even of the late Neolithic Age.[2]

xiii, 1870, p. 520) remarked that 'it is not surprising that on the first discovery of the advantages of [adding tin to copper] . . . all the old implements of copper, wherever procurable, should have been taken to the melting-pot for conversion into bronze, and we should thus be left with such scanty evidence of the existence of an age of copper'. Still we have sufficient evidence for Ireland, and not for Britain. Professor Gowland (*Journ. Anthr. Inst.*, xxxvi, 1906, p. 23) attributes the scarcity of copper celts in England to 'the occurrence of mixed copper and tin ores in Cornwall.'

[1] *Man*, iii, 1903, No. 8, pp. 147–9. Professor Montelius (*ib.*, v, 1905, No. 7, pp. 13–4) denies that iron was used by the Egyptians before the fifteenth century B.C., and insists that the lump of iron oxide which was found at Abydos in association with copper implements of the Sixth Dynasty, or about 3200 B.C., 'does not prove the *use* of iron, only the existence of that metal'. The professor doubtless wrote 'existence' by a slip for 'knowledge'. But, as Mr. H. R. Hall points out (*ib.*, No. 40, pp. 69–71), he ignores the discovery in the Great Pyramid of a piece of worked iron, which is now in the Third Egyptian Room of the British Museum (Case K 29, No. 2433), and to which a date about 300 years older 'is assigned on good *prima facie* grounds'; and Mr. Hall reasonably asks whether the discovery of the lump of iron oxide does not corroborate the other. It is unlikely that an unworked lump of iron would have been deposited along with copper tools; and we may fairly suppose that the lump is the remains of an iron tool.

[2] *Archaeologia*, lvi, 1899, pp. 302–3; L. Beck, *Die Gesch. des Eisens*, i, 1884, pp. 593–6. As far as I can see, all that is proved by the instances which Beck has collected is—what we know already—that stone implements continued in use after the Iron Age had begun. In regard to the discovery, mentioned

The inconsistency of these arguments is self-evident; and if their authors had known the rudiments of archaeology, they would never have published them.[1] Hundreds of iron weapons have been recovered from the Thames: a competent archaeologist has affirmed that there was not one which could not with certainty be attributed to some period later than the Bronze Age; and since numerous articles of stone and bronze have been found in the same bed, he reasonably concludes that if iron implements had been used in the Bronze Age, some few at least must have come to light.[2] Nor is there any reason to suppose that if iron tools had been laid in graves of the Bronze Age, they would necessarily have perished beyond recognition; for in the famous Tyrolese cemetery of Hallstatt, and in many other deposits that, like it, belonged to the transitional period when bronze and iron were simultaneously used, the iron objects, oxidized though they are, retain their distinctive forms.[3] Yet in the numerous British barrows of the Bronze Age, and in the hoards of the same period that have been unearthed in England, Scotland, and Wales, not a trace of iron has ever been found.[4] Nothing then can be more certain than that in Britain, as in the rest of Europe, the Iron Age was preceded by a long period during which the only metals used were copper and bronze.[5]

Where did the European bronze culture originate?

Every antiquary knows that bronze did not reach this country until long after it was first used in Southern Europe, and that it was common in Egypt many centuries before; but in what part of the world it was first manufactured

on p. 595, which Worsaee made in a stone chamber, may not graves of this kind have been built here and there after the Neolithic Age? See pp. 108, 109, n. 2, *supra*.

[1] See *Man*, v, 1905, No. 7, p. 13.

[2] *Journ. Anthr. Inst.*, xxx, 1900, p. 16.

[3] J. Evans, *Anc. Bronze Implements*, &c., p. 25. See also p. 95.

[4] *Ib.*, p. 23.

[5] See O. Schrader, *Prehist. Ant. of the Aryan Peoples*, pp. 194, 203–4, 242; *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 3–4; *Rev. mens. de l'École d'anthr.*, iii, 1893, pp. 105–6, 118, 120–2; Daremberg and Saglio, *Dict. des ant. grecques et rom.*, ii, 1075; and Pauly's *Real-Encyclopädie*, v, 1905, col. 2143. Classical scholars will remember that Lucretius (v, 1286), in his powerful description of prehistoric times, affirmed that bronze was used before iron.

remains an unsettled question.[1] The oldest piece of bronze that has yet been dated was found at Mêdûm in Egypt, and is supposed to have been cast about three thousand seven hundred years before the birth of Christ. But the metal may have been worked even earlier in other lands; for a bronze statuette and a bronze vase, which were made twenty-five centuries before our era, have been obtained from Mesopotamia; and the craft must have passed through many stages before such objects could have been produced. Yet it would be rash to infer that either the Babylonians or the Egyptians invented bronze; for neither in Egypt nor in Babylonia is there any tin. Some archaeologist who shall explore the virgin fields of the Far East may one day be able to prove that bronze was worked by the Chinese, in whose country both copper and tin abound, earlier than by any other people; but even so it will still remain doubtful whether the art was not independently discovered elsewhere. There is no evidence that the bronze culture of Mexico and Peru did not originate in America;[2] and although it was once believed that all the tribes of Europe ultimately derived their knowledge of the metal from Asia,[3] there are many who now maintain that it is impossible to detect in European deposits of the Bronze Age the slightest trace of Oriental origin.[4]

[1] See O. Schrader, *Prehist. Ant. of the Aryan Peoples*, pp. 192–3, *Reallexicon der indogermanischen Altertumskunde*, 1901, pp. 200–1; and *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 9–11. Dr. Schrader argues that as there are no special names for bronze in the languages of any of the ancient bronze-using peoples except the inhabitants of Mesopotamia, in whose tongue bronze, as distinct from copper (*urudu*), was designated by the word *zabar*, they must have been the inventors of bronze. See p. 494, *infra*.

[2] Professor Boyd Dawkins (*Early Man in Britain*, pp. 408–9) has compiled tables which show that the percentage of tin in British bronze implements varies between 5·09 and 18·31, and in French between 1·50 and 21·5. He concludes (*ib.*, p. 410) that 'the uniformity of the composition of the cutting implements of the Bronze Age implies that the art of compounding tin with copper was discovered in one place, from which the knowledge of it spread over . . . Europe and Asia, and the greater part of the Americas. Had it spread from separate centres, this uniformity would have been impossible.' The uniformity which subsists between 5·09 and 18·31, and between 1·50 and 21·5 is remarkable.

[3] See Sir John Evans's *Anc. Bronze Implements*, p. 420; *Rev. d'anthr.*, 3ᵉ sér., iii, 1888, pp. 209–10; and Lord Avebury's *Prehist. Times*, 1900, pp. 53–7.

[4] *L'Anthr.*, iv, 1893, pp. 548, 561–2, 566. M. Salomon Reinach (*ib.*, iii,

Origin and affinities of the bronze culture of Britain.

But whatever may have been the case in Southern lands, there is no doubt that the knowledge of bronze came to this country from abroad. The old theory that it was a result of Phoenician commerce with Britain has long been abandoned ;[1] and British bronze implements are so different from those of Norway and Sweden, Denmark, and Hungary that it cannot have been derived from any of those countries.[2] German influence was felt at a comparatively late period ;[3] but from first to last the British bronze culture was closely connected with that of Gaul, and through Gaul with that of Italy.[4]

Period of its commencement.

The period when bronze first appeared in Britain can only be approximately fixed. It is certain that in the south-eastern districts iron tools began to be used not later than the fourth century before the Christian era.[5] The final period of the British Bronze Age is marked by the discovery of bronze-founders' hoards, all of which contain tools or fragments of tools which are known as socketed celts, or other socketed instruments which were contemporary with them. These hoards are so numerous and so widely diffused, and the objects of which they are composed are so varied in form, that the time during which they were deposited cannot, in the opinion of experts, have been less than four or five hundred years. But before the first socketed celt was cast the bronze culture passed through earlier stages, during which the flat celts that resembled those of stone were being used, and then gradually giving way to improved forms, which in their turn were succeeded by later developments. The veteran archaeologist who has handled and examined almost every specimen of these numerous varieties has arrived at the conclusion that the British Bronze Age

1892, p. 280) has gone so far as to suggest that ' les origines mêmes de la métallurgie du bronze ' should be sought in Western Europe.

[1] See *Journ. Roy. United Service Inst.*, xiii, 1870, p. 539, and J. Evans, *Anc. Bronze Implements*, p. 419.

[2] *Ib.*, p. 476.

[3] *Ib.*, pp. 476–7. See p. 144, n. 5, *infra*.

[4] J. Evans, *Anc. Bronze Implements*, pp. 420, 477 ; *Association franç. pour l'avancement des sc.*, 2e partie, 1903, p. 931 ; *L'Anthr.*, xvi, 1905, p. 168.

[5] J. Evans, *Anc. Bronze Implements*, pp. 470–2. See pp. 231–4, *infra*.

must have begun at the latest between 1400 and 1200 B.C.;[1] and while no one would now contend for a later date, there are some who maintain that bronze was first used in Britain twenty centuries before the Christian era.[2]

Physical characters of the late neolithic and early bronze-using invaders of Britain.

After the Bronze Age set in, as before the close of the preceding period, bands of invaders, wholly different in physical type from the neolithic aborigines, landed successively through long ages upon our eastern and southern shores. They came from the Netherlands, from Denmark and its islands, perhaps also from Scandinavia and from Gaul. They must not, however, be identified either with the invaders who introduced the Celtic language into Gaul or with any Celtic-speaking people. There is no evidence, and it is in the last degree improbable, that any Celtic tribe had appeared in Gaul at the time when the alien immigrants began to settle in Britain, or that Celtic had then taken shape as a branch of the Indo-European language. Those immigrants have often been described as a tall, stalwart, round-headed race; but the evidence of sepulchral remains shows that they sprang from various stocks. Those of the type which is commonly regarded as specially characteristic of the Bronze Age were taller and much more powerfully built than the aborigines: their skulls were comparatively short and round; they had massive jaws, strongly marked features, enormously prominent brow ridges and retreating foreheads; and their countenances must have been stern, forbidding, and sometimes almost brutal. Similar skulls, which have much in common with the primitive Neanderthal type,[3] have been exhumed from neolithic tombs in Denmark and the Danish island of Falster. But the skeletons which have been found in some of the oldest Scottish cists belonged to men whose average height, although they were sturdy and thickset, was barely five feet three inches, and whose skulls, shorter and rounder than the others, as well as their milder features, proved that they were an offshoot of the so-called Alpine race of Central

[1] J. Evans, *Anc. Bronze Implements*, pp. 472–3.

[2] *Journ. Anthr. Inst.*, xxix, 1899, pp. 308–10; *Man*, v, 1905, No. 7, p. 13.

[3] See pp. 33–4, *supra*.

Europe, of which there were numerous representatives in Gaul. Again there were tall men with skulls of an intermediate type; while others, who combined harsh features and projecting brows with narrow heads, and whose stature was often great, would seem to have been the offspring of intermarriage between the older and the newer inhabitants. Not a single skeleton of the characteristic British round-barrow type is known to have been discovered on French soil: the round-headed inhabitants of Gaul were as conspicuously short as those of Britain were generally tall; nor, excluding the Britons of the Alpine stock, was there any physical resemblance between the two peoples. The British invaders of the Alpine stock, judging from the pottery which was found with their skeletons, came for the most part, as we shall afterwards see, not from Gaul but from the valley of the Rhine. Moreover, the round-headed people of Gaul settled there first early in the Neolithic Age, before a Celtic word was spoken; and although their descendants formed the substratum of the Gallic population who, in Caesar's time, called themselves Celts, that name was introduced by conquerors of a wholly different stock. Probably a Celtic invasion of Britain took place before the British Iron Age began: but the remains of such invaders are not recognizable in any British graves.[1]

Their social organization.

Each of the invading clans was doubtless ruled by a chief; for many of the burial mounds which they erected were intended for the great alone, and could only have been constructed by the organized labour of many hands.[2] They must have respected family ties; for women and even babies were interred with scrupulous care; and more than one barrow was reared for the reception of a single child.[3] Yet infants have so often been found buried along with women that one can only conclude that infanticide was as prevalent in ancient as in modern Britain.[4] Only the

[1] See pp. 424–44, *infra*.

[2] W. Greenwell, *Brit. Barrows*, pp. 111–2.

[3] *Ib.*, pp. 118–20.

[4] *Archaeologia*, xliii, 1871, p. 313; *Report of . . . the Brit. Association*, 1883 (1889), p. 316. Cf. O. Schrader, *Prehist. Ant. of the Aryan Peoples*, p. 389.

children were slain because their mothers could no longer nurse them, not because they desired to rid themselves of trouble.

Character and results of the invasions: the invaders poor in bronze weapons.

In Wiltshire and other parts of Southern Britain the old population would seem to have been largely dispossessed or subdued; but the skeletons found in the barrows of Derbyshire and Staffordshire, of Yorkshire and the other northern counties, indicate that there the immigrants mingled more or less peacefully with the people whom they came among.[1] Fighting no doubt took place everywhere; but the notion that bronze weapons gave the first invaders victory is disproved by the fact that in the earlier part of the era bronze was both costly and rare.[2] If chieftains had bronze, their clansmen were still armed with old-fashioned weapons; and until the new age was far advanced, the neolithic tribes, in so far as they were conquered, must have yielded to superior numbers, superior skill, or superior strength. Probably in certain districts they were never conquered, and never permitted the intruders to dwell among them. Among a vast number of stone implements that have been found lying on the moors west of Rochdale and Ashton-under-Lyne bronze was searched for in vain;[3] and one may provisionally infer that these hillmen were protected by the strength of their territory.

Evidence of finds as to the settlements of the invaders.

Bronze implements or other relics of the Bronze Age have been found in almost every county of England, Wales, and Scotland, and in some of the adjoining islands;[4] but their distribution appears to imply that, as might have been

[1] W. Greenwell, *Brit. Barrows*, pp. 127, 711–2; *Vict. Hist. of . . . Derby*, i, 180, 190, n. 1. See pp. 427–8, *infra*.

[2] See pp. 131–2, *infra*. Professor Boyd Dawkins (*Early Man in Britain*, p. 342) asserts that 'bronze weapons ensured victory [to the brachycephalic immigrants] over enemies armed with the old weapons of stone'. On page 344 he remarks that 'while the chiefs and the rich possessed bronze implements and weapons, the poorer classes would naturally continue to use those of stone', &c. How could bronze weapons have decided battles if only 'the chiefs and the rich' wielded them?

[3] *Vict. Hist. of . . . Lancs*, i, 212, 239.

[4] Since the publication of Sir John Evans's work, bronze weapons have been found in the Orkney and Shetland Islands (*Proc. Soc. Ant. Scot.*, xxi, 1887, pp. 340–2).

inferred from the geographical features, some districts were far more densely populated than others. The lands which the new comers selected were mainly those which were already occupied by the neolithic inhabitants. The relics are most abundant in those which are now most sparsely peopled, but which were then sought after because, even when the soil was poor, it was dry, well-watered, and comparatively open. The moors of Derbyshire, Yorkshire and other Northumbrian counties, Devonshire and Cornwall; the bracing uplands of East Anglia; the downs of Kent, Surrey, Sussex, Berkshire, Hampshire, Dorsetshire, and Wiltshire; and the wolds of Lincolnshire,—these were the tracts which the immigrants occupied in the greatest numbers. The Midlands, on the other hand, would seem to have attracted comparatively few: Durham, for some unexplained reason, was generally avoided;[1] while the northern and north-western tracts of Scotland were almost entirely neglected.[2] The Yorkshire Wolds afford an interesting example of the motives which determined the choice of abode. Their scanty vegetation could not have tempted a people who depended for their subsistence mainly upon their flocks and herds; yet the numerous barrows with which they are studded and the flint implements which have been picked up in thousands from their surface prove that they were as thickly peopled as any other part of Britain. The reason was that they were unencumbered by the forests which could only have been cleared by arduous labour; their climate was healthy; and, above all, they were so completely isolated by the wooded valley of the Derwent, the swamps of Holderness, the broad estuary of the Humber, and the morasses which then covered the plain of York, that their occupants were secure from all attack.[3]

[1] See p. 133, n. 1, *infra*.

[2] See the Topographical Index in Sir J. Evans's *Anc. Bronze Implements*; *Vict. Hist. of . . . Northampton*, i, 142; *Vict. Hist. of . . . Nottingham*, i, 289; *Journ. Anthr. Inst.*, xxxii, 1902, p. 386; and *Archaeol. Journal*, lxiv, 1904, pp. 310–2. It must not, however, be supposed that mere statistics of finds are necessarily valid evidence. See *Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 496–7.

[3] W. Greenwell, *Brit. Barrows*, pp. 118, 133–5.

In certain parts of England the routes by which invaders advanced may be traced by the sites at which bronze implements have been found. In Worcestershire, for example, these spots have been mapped along the line of the Avon from Warwickshire to the Severn, and again in the valley of the latter river, where it was apparently crossed by ancient trackways. The implements in these two counties belong to comparatively late periods.[1]

The settlements must often have been desperately resisted, more and more as time passed and unoccupied lands became rare. But it would be a mistake to assume that the struggle was always between aboriginal communities and round-headed invaders. There must have been much intermingling between the old population and the new : gradually the use of bronze weapons must have spread to neolithic clans or to those who could obtain them by barter or theft ; and by the time when the Bronze Age was far advanced tribes of mingled stock must often have presented a united front to enemies from over sea. Even when the invaders had slowly made their way from the Channel to the far north, and from the German Ocean to the Irish Sea, hunger or the lust of booty would often lead to intertribal raids. Gradually weapons were improved ; and we shall presently endeavour to trace their evolution. Even to the very end of the period, however, not only the rank and file but the wealthiest chief, who had a complete set of bronze implements and weapons, and who could afford to decorate the handle of his blade with ivory, amber, or gold, to wear gold buttons on his clothing, sometimes even to adorn his charger with a gold peytrel, shot arrows tipped with flint. Flint arrow-heads, leaf-shaped and barbed, have been found by thousands in deposits of the Bronze Age, but in this island never one of bronze. Even when daggers had given place to swords and bronze spears were common, battle-axes were made not of bronze but of stone.[2]

[1] *Vict. Hist. of . . . Worcester*, i, 183–4. Cf. J. Evans, *Anc. Bronze Implements*, pp. 81, 88, 129, 368.

[2] W. Greenwell, *Brit. Barrows*, pp. 159–60 ; J. Evans, *Anc. Bronze Implements*, p. 318, *Anc. Stone Implements*, 1897, p. 195 ; *Archaeologia*, xliii, 1871,

Stone implements used long after the introduction of bronze.

Stone implements indeed, such as were in use in the Neolithic Age, have been found so often in the graves of chieftains associated with those of bronze that we may be sure that, at least in the earlier part of the Bronze Age, even the wealthier classes could not afford to discard the older material; while among the needy population of the Yorkshire Wolds many barrows contained no implements except those of flint or bone.[1] Bronze saws have very rarely been found in this country, although they were common enough in Southern Europe;[2] and since all our bronze gouges are comparatively late,[3] it may be inferred that during the earlier Bronze Age these tools were everywhere still made of flint. In the west of Scotland, at all events, metal tools were apparently unknown until long after the first round-headed people landed, and probably until long after bronze had begun to be used in Southern Britain.[4] We may indeed be sure that the Stone Age continued for centuries later in remote parts of the country; and perhaps in certain islands bronze may have remained unknown.

Hill-forts.

When a clan had succeeded in establishing itself, it had to provide for its protection against cattle-lifters and slave-hunters; and gradually and by immense labour great

pp. 409–12; lii, 1890, pp. 60–1; liv, 1895, p. 105; *Archaeol. Cambr.*, 6th ser., ii, 1902, pp. 60–1. Flint arrow-heads are also found with burials of the Bronze Age in France (J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 171, n. 1); and Sir J. Evans (*Anc. Bronze Implements*, p. 318) says that 'many of the bronze arrow-heads found on the Continent appear to belong to the Early Iron Age'. He admits, however, that some very small spear-heads, so called, 'may possibly have served to point arrows'. Bronze battle-axes are unknown in Northern France as well as in Britain (*ib.*, pp. 161–2).

[1] *Ib.*, pp. 19–20, 41, 51, 165, 189–90, 224–5, 256, 480, 487; *Anc. Stone Implements*, 1897, p. 143; *Trans. Ethn. Soc.*, N. S., iii, 1865, p. 313; *Archaeologia*, xliii, 1871, pp. 412–3, 435–6, 438; W. Greenwell, *Brit. Barrows*, pp. 38, 43, 360; A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 11, 17; *Trans. Devon Association*, xxxiv, 1902, p. 128; *Vict. Hist. of . . . Lancs*, i, 218.

[2] No British bronze saws are mentioned in Sir J. Evans's work, which was published in 1880; but see *Proc. Soc. Ant.*, 2nd ser., xi, 1885–7, p. 12; *The Naturalist*, 1903, pp. 206–7; and J. R. Mortimer, *Forty Years' Researches*, p. 182.

[3] J. Evans, *Anc. Bronze Implements*, p. 173.

[4] *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 78.

strongholds were constructed on suitable sites. Comparatively rare in the south-east, they are conspicuous on nearly all the hilly districts of England, Wales, and Scotland;[1] but it is in the western and south-western counties that they most abound. Devonshire and the adjacent parts of Somersetshire contain not less than eighty; and almost every spur on Salisbury Plain is fortified.[2] The multiplicity of these camps bears witness not only to density of population and constant warfare, but also to the utter disunion which existed at the time when they were constructed. Supposing that the majority of the forts in Dorsetshire, for instance, were built in the Late Celtic Period, we should have to conclude that the Durotriges, who then inhabited that district, were merely a loose aggregate of scores of clans, ever ready to prey upon one another; for if the forts had been destined only to repel the attacks of some other tribe, they would hardly have been so numerous and so widely scattered. It is true that the Gallic Morini in Caesar's time had not become welded into one state, and that the Kentish clans were under four petty kings; but in the period when the older earthworks were thrown up it would seem that far less progress had been made towards union. But even supposing that most of the prehistoric forts were later than the Bronze Age, their purpose accorded with the methods of primitive warfare. A chain of modern fortresses impedes an invader because, while they remain uncaptured, he cannot pass between them without exposing his line of communication. But in ancient times, when one tribe attacked another, it had no communications to guard: the invaders carried their food with them, and when it was spent trusted for support to the enemy's country.[3] If a tribe had desired merely to protect its frontier, it would not have erected hill-forts but a continuous entrenchment.

[1] See B. C. A. Windle, *Remains of the Prehist. Age*, &c., pp. 232–47. Durham, strange to say, has hardly any, though they abound in Northumberland (W. Greenwell, *Brit. Barrows*, pp. 378, 440; *Journ. Brit. Archaeol. Association*, N. S., xi, 1905, p. 168). In highly cultivated districts many have of course been destroyed.

[2] *Archaeol. Journal*, xxix, 1872, p. 160, n. 1; xxxii, 1875, p. 292.

[3] See A. Pitt Rivers, *Excavations in Cranborne Chase*, iii, 5, 7–8.

Amongst those which were occupied in the Bronze Age or before may be mentioned Badbury Rings in Dorsetshire;[1] the stone fort on Whit-Tor in Dartmoor[2] and another in the Rhonddha valley in Glamorganshire;[3] Small Down camp near Evercreech in Somersetshire;[4] the fort of Carn Brea in Cornwall;[5] the series of entrenchments which mark the spurs of the hills that command the valley of the Esk from Guisborough to Whitby;[6] those which line the western border of Worcestershire;[7] Oldbury, some three miles east of Sevenoaks;[8] Hollingbury on the Sussex Downs;[9] Lutcombe Castle on the Berkshire Downs, overlooking the Vale of White Horse;[10] and the greatest of all—the Maiden Castle, whose stupendous ramparts are the pride of Dorchester.[11] But it is probable that the greater number may ultimately be referred to the Age of Bronze.[12]

[1] J. Evans, *Anc. Bronze Implements*, p. 250.

[2] *Trans. Devon. Association*, xxxi, 1899, pp. 146–55; *Trans. Hon. Soc. Cymmrodorion*, 1898–9 (1900), p. 19.

[3] *Archaeol. Cambr.*, 6th ser., ii, 1902, pp. 252–60.

[4] *Proc. Somerset. Archaeol. and Nat. Hist. Soc.*, l, 1905, part ii, pp. 32–49.

[5] *42nd Annual Report Roy. Inst. Cornwall*, 1860, pp. 17–43; *Journ. Roy. Inst. Cornwall*, xiii, 1895 (1896), pp. 98–9; *Trans. Hon. Soc. Cymmrodorion*, 1898–9 (1900), p. 19; *Trans. Roy. Irish Acad.*, xxxi, 1896–1902, p. 633.

[6] *Ib.*, pp. 618–9, 623; *Journ. Roy. Soc. Ant. Ireland*, xxxv, 1905, pp. 244–5.

[7] *Vict. Hist. of . . . Worcester*, i, 182.

[8] G. Payne, *Collectanea Cantiana*, 1893, pp. 176–7.

[9] *Vict. Hist. of . . . Sussex*, i, 471.

[10] *Vict. Hist. of . . . Berks*, i, 261.

[11] *Journ. Brit. Archaeol. Association*, xxviii, 1872, pp. 40, 42; *Man*, iv, 1904, No. 105, pp. 161–2. Mr. Cunnington (*Proc. Dorset. Nat. Hist. and Ant. Field Club*, xxiv, 1903, pp. xxxiv–xxxviii) has wasted much labour in endeavouring to prove that the Maiden Castle was constructed by the Romans. In 1882 and following years he excavated in the eastern division of the fort and found remains of a Roman building, which proves merely that the fort was occupied in Roman times.

[12] I am glad to find that I have the support of Mr. C. H. Read (*Guide to the Ant. of the Bronze Age*, p. 78) and Mr. Reginald Smith (*Guide to the Ant. of the Early Iron Age*, 1905, p. 122), who unhesitatingly refer the Dorsetshire hill-forts in general to 'the Bronze, and possibly, in some cases, the Neolithic period'. Mr. H. St. G. Gray strains Pitt-Rivers's doctrine when he argues (*Index to Excavations in Cranborne Chase*, 1905, p. xix; *Proc. Somerset. Archaeol. and Nat. Hist. Soc.*, 3rd ser., ix, 1903 [1904], p. 28) that, without excavation, it is idle to express any opinion as to the age of a camp. He says that 'Caesar's Camp' at Folkestone 'was always considered to be pre-Roman before Lane-Fox excavated it and proved it to be of Norman construction'

The form, construction, and materials of British forts are naturally diverse. In Cornwall, Devonshire, Wales, and other places they were of course built largely or wholly of stone, the masonry being always uncemented: elsewhere they were true earthworks. Leaving out of sight the question of their date, they may be grouped in three classes.[1] The first comprises those that were erected on promontories or other heights which on one or more sides were fortified by precipice, river, or sea. Such was the fort of Carl's Wark in Derbyshire, which, on three sides, rises almost sheer above the swamps of Hathersage Moor. On the west, where the ground slopes towards the plain, a huge earthen rampart, faced with dry masonry, afforded secure protection; and the slopes below the eastern and southern sides are strewn with great stones which must have fallen from the walls above.[2] The 'cliff-castles' on the coasts of Kirkcudbright and of Wales and on the headlands between the Land's End and Cape Cornwall belong to the same

This remark is incorrect (see *Excavations in Cranborne Chase*, iii, p. xi), and, even if it were true, would only prove that less was known about the principles of construction of prehistoric British camps in former days than now. I cannot conceive how anybody could on *a priori* grounds suppose 'Caesar's Camp' to be pre-Roman, even if he had only seen the plan on the 25-inch O. S. map. Of course I freely admit that, without excavation, it would be generally (though not always) idle to express any opinion as to the particular prehistoric epoch to which a fort belonged.

Dr. Joseph Anderson (*Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 139, with which cf. D. Christison, *Early Fortifications in Scotland*, 1898, pp. 350–3, 380–1) observes that no one Scottish fort 'can be assigned with certainty to the Bronze Age'; but it is nevertheless morally certain that many did then exist.

Mr. R. Burnard (*Vict. Hist. of . . . Devon*, i, 366) remarks that it is unsafe to infer from the fact that Cranbrook Castle, in the valley of the Teign, contained pottery of Bronze Age type, that it belonged to that period; for such pottery was also used in the Early Iron Age. Certainly it was; and so also were bronze implements (see pp. 266–7, *infra*); but, as a rule, the discovery of Bronze Age pottery, or bronze implements, unaccompanied by objects of the Early Iron Age, is enough to raise a presumption, which in most instances would be correct, that the site was occupied in the Bronze Age.

[1] *Scheme for recording Ancient Defensive Earthworks and Fortified Enclosures*, 1903, pp. 2–3, 6 (published by the Congress of Archaeological Societies). My classification differs slightly in form, but not in substance, from that adopted by the Congress.

[2] *Journ. Derbyshire Archaeol. and Nat. Hist. Soc.*, xxv, 1903, pp. 175–80.

group.[1] In the second class the entrenchments, traced upon commanding sites, which, however, were nowhere so steep as to dispense with artificial aid, followed the tactical line of defence which the nature of the hill indicated. Most of the heights on which they stand are covered with soil so thin that they never could have been thickly wooded, and if trees had encumbered their sides they would have been cut down; for the object of the engineers was to leave no 'dead ground' on which an assailant could conceal himself. If he felt strong enough to lead his clansmen to the assault, he knew that they could not avoid being exposed from the moment when they penetrated within the range of a bow or a sling. General Pitt-Rivers, who did so much to illuminate the study of prehistoric fortifications, was never weary of calling attention to the skill with which they had been designed. Once only, when he was exploring the camp at Seaford, he thought that he could detect evidence of neglect. As he stood upon the rampart he noticed that an advancing force would be able to conceal itself for a while. Presently, however, it flashed across his mind that time had done its work upon rampart and ditch; and soon excavation proved that the latter had lost by silting seven feet of its original depth. The general saw with delight that the designer had been as vigilant as any of his contemporaries. The rampart in ancient times must have been at least five feet higher; and then the garrison who manned it would have been able instantly to detect the first enemy who ventured within range. 'How carefully,' he wrote, 'the defenders economized their interior space, drawing their rampart just far enough down the hill to obtain a command of view, but not one yard further.'[2]

In certain cases, however, the hill was so extensive that if the tactical line of defence had been slavishly followed, the defenders would have been too few. Then the chief

[1] *Archaeologia*, xliv, 1873, p. 424; xlix, 1885, p. 181; *Journ. Roy. Soc. Ant. Ireland*, xxxv, 1905, p. 244; *Archaeol. Cambr.*, 3rd ser., xi, 1865, pp. 77–81. Many of the 'cliff-castles' probably do not belong to the Bronze Age (see *Vict. Hist. of... Cornwall*, i, 451–2, 458–9).

[2] *Journ. Anthr. Inst.*, vi, 1877, pp. 288–9. See also *Archaeologia*, xlvi, 1881, p. 458.

engineer modified the accepted principle. Selecting a spot at which he might safely abandon the natural line, he made his sappers build a cross rampart at right angles to it straight across the hill-top until it joined the works on the further side. An example of this device may be seen in the camp of Puttenham in Surrey.[1]

Among the more famous strongholds of the second class are Cissbury on the South Downs, which, as we have seen, was almost certainly erected in the Neolithic Age,[2] Badbury Rings, and the Maiden Castle. This noble fortress must surely have deserved its modern name. No British force could ever have taken it: no other country can show its match. Three lines of ramparts defend the northern and four the southern side: gaining the summit of the road from Weymouth, you see them outlined against the sky; and as you mount the hill-side, they rise, one behind another, like veritable cliffs. Worn by the rains of five-and-twenty centuries or more, they still stand sixty feet[3] above their fosses; and their entrances, on the east and the west, are guarded by overlapping works so intricate that if a column had succeeded in forcing its way across the abatis, it would have found itself helplessly winding in and out as through a labyrinth, pounded on either flank and enfiladed by stones and arrows discharged at point-blank range.

The strongholds of the third class were erected on lower hills or on high ground little elevated above the surrounding country, and therefore depended less for their protection upon natural features.[4] Those that have been explored belong to the Late Celtic Period.[5] It may be doubted, however, whether such forts were generally later than those whose sites were more commanding; for the inhabitants

[1] *Journ. Anthr. Inst.*, vi, 1877, pp. 288–9. The same feature exists in the camp at Seaford.

[2] See p. 98, *supra*.

[3] See *Archaeol. Journal*, xxii, 1865, p. 354.

[4] E.g. Ambresbury Banks in Essex, Yarnbury on Salisbury Plain, and Hunsbury near Northampton. See R. C. Hoare, *Anc. Wilts*, i, 1812, pp. 89–90; *Trans. Epping Forest . . . Field Club*, ii, 1882, pp. 55–68; and *Journ. Brit. Archaeol. Association*, N. S., vii, 1901, p. 23.

[5] See pp. 259–60, *infra*.

of every district could only choose the best positions which they could find.[1] Cherbury camp indeed, about four miles south-east of Fyfield in Berkshire, was built on a low-land plain.

Some of the Gallic forts which Caesar saw, and of our own, were in his time inhabited by large industrial communities; but although many of the British strongholds which belonged to the Bronze Age contain the foundations of huts and broken pottery,[2] it is doubtful whether they had more than a few occupants except in time of war.[3]

Every explorer who has tried to imagine the conditions of life in ancient British forts has noticed that many of them have no apparent source from which water can be obtained. It has indeed been suggested that where there was neither a spring nor running water within reach the garrison had recourse to dew-ponds, which are still used for watering cattle on the Hampshire downs.[4] But even these reservoirs were generally lacking. Pitt-Rivers, however, argued that in the chalk districts many sites which are now remote from water may have possessed springs. At the village of Woodcuts in Cranborne Chase, after cleaning out a Roman well, one hundred and eighty-eight feet deep, he found no water, but the iron-work of a bucket.[5] But even where there was no spring it is easy to understand how the garrison supplied themselves. None of these camps was ever subjected to a prolonged siege. No army can undertake such an opera-

[1] Mr. I. Chalkley Gould (*Trans. Essex Archaeol. Soc.*, N. S., viii, 1903, p. 139, with which cf. *Journ. Derby. Archaeol. and Nat. Hist. Soc.*, xxiv, 1902, p. 29) says that 'the early constructors fixed on the highest points, and . . . adopted a system of tortuous and involved entrances'; and that in later times engineers 'no longer depended on involved tortuous entrances'. There is some truth in this; but forts which were at all events occupied in the Early Iron Age stood 'on the highest points', and the entrances of Cissbury (*Archaeologia*, xlv, 1880, p. 338, pl. xxvi), which was probably erected in the Neolithic Age (pp. 97–8, *supra*), and of many other hill-forts were not tortuous.

[2] See, for instance, *Trans. Devon. Association*, xxxi, 1899, p. 151, xxxiii, 1901, pp. 129–38; and *Archaeol. Cambr.*, 6th ser., ii, 1902, pp. 252–60.

[3] See *Vict. Hist. of . . . Derby*, i, 364, and pp. 257–8, *infra*.

[4] *Proc. Geologists' Association*, 1887–8 (1889), pp. 376–7; *Papers Hants Field Club*, iii, 1896, part ii, p. 175; W. Johnson and W. Wright, *Neol. Man in N.-E. Surrey*, 1903, p. 47; *Cornhill Mag.*, May, 1906, p. 612.

[5] *Excavations in Cranborne Chase*, iv, 19–20.

tion unless it can ensure a continuous supply of food; and to do this requires forethought and organization of which barbarous clans are incapable. Again and again the Gauls with whom Caesar contended, whose civilization was far more advanced than that of the Britons of the Bronze Age, were obliged to abandon movements that might otherwise have succeeded, simply because their commissariat had been neglected.[1] When ancient Britons were obliged to take refuge in their stronghold, they knew that the danger would pass if they could hold out for a little while. Women and children who failed to reach the entrenchment in time were doubtless slain or enslaved. But otherwise the worst that was to be dreaded was the loss of crops or stock and the destruction of dwellings. We may suppose that while the cattle were being driven into the fort the women carried up in vessels of skin or earthenware as much water as would suffice for a few days. Such was the practice of the Maoris at a recent time.[2]

Primitive metallurgy.

In spite of war industrial arts were making progress, which was stimulated by war itself. Copper was abundant in Cornwall, Cardiganshire and Anglesey, and near Llandudno: tin was to be had near the surface in Cornwall,[3] and perhaps first attracted attention where it was associated with gold; native smiths began to copy the tools which were brought from abroad; and insular forms were gradually evolved. Among the immigrants there must have been some who were acquainted with metallurgy; and just as the modern coach-builder finds himself obliged to manufacture motor-cars, so, we may be sure, the more enterprising cutlers who had hitherto made stone implements

[1] See my *Caesar's Conquest of Gaul*, 1903, pp. 51, 160–1.

[2] See *Archaeologia*, xlii, 1869, p. 51; A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 238; and *Journ. Derby. Archaeol. and Nat. Hist. Soc.*, xxiii, 1901, pp. 112–3.

[3] Prof. F. J. Haverfield (*Eng. Hist. Rev.*, xix, 1904, p. 746) says that 'the scanty archaeological evidence hardly seems to justify . . . Cornish mining so early as B.C. 800'. But it does prove that bronze implements were made in Britain in the earlier part of the Bronze Age,—considerably earlier than 800 B.C.; and the tin must have been obtained either from Cornwall, or from Dartmoor, or from both. There is no evidence of prehistoric mining in Dartmoor, and there is in Cornwall (pp. 502–3, n. 8, *infra*).

gradually learned to produce tools of copper or bronze. The metals were of course not at first procured by mining. Copper would be obtained from boulders or from lumps of ore on hill-sides, and tin from the gravel beds of streams. The methods, which have been recorded by modern observers, of primitive communities are probably much the same as those of the Britons of the Bronze Age. The original furnaces differed hardly at all from the fires at which food was cooked. The fire was kindled within a fire-place of large stones, underneath which was a pit. The wind, rushing through the crevices of the stones, created a draught, which may have been forced by some rude bellows. After the embers and the slag had been raked away the molten metal in the pit was watched until it was on the point of becoming solid, when the copper cakes were snatched out and broken into the lumps of which specimens have been found in bronze-founders' hoards. For the smelting of tin a method may have been adopted which was still practised in Germany in the Middle Ages. A trench was filled with brushwood, above which logs were piled; and as soon as the fuel was aglow the ore was pitched on to the fire until a sufficient amount had accumulated. Then the embers were raked away, and the molten tin ladled out.[1] It is worthy of remark that all the Scottish bronze implements which had been analysed up to the year 1880 contained lead;[2] and one may perhaps infer that the tin which was exported from Cornwall to Scotland was not pure.

Bronze implements:— celts.

Many bronze implements were reproductions, more or less modified, of neolithic models. Stone celts, knives, daggers, spear-heads, awls, chisels, gouges, sickles, and saws have their successors in bronze. Gradually, however, new forms were developed or invented. Bronze was of course at first reserved for weapons; and knives or knife-daggers probably preceded all others, because the metal was originally too scarce and expensive to be used for those

[1] See Prof. Gowland's interesting paper in *Archaeologia*, lvi, 1892, pp. 267–322, especially 268, 284–5, 287, 296.

[2] J. Evans, *Anc. Bronze Implements*, p. 425. I do not know what the results of later analyses have been. See also *Archaeologia*, liv, 1895, p. 97, and Addenda, p. 739.

which required a large expenditure of material.[1] Flat axes, resembling more or less closely the polished neolithic celts, were, however, manufactured early in the Bronze Age. After some time the sides of the narrow part of the celt, above the cutting edge, were hammered upwards,—probably in order to steady the blade against a lateral strain; and thus by insensible gradations the flat was transformed into the flanged celt; while a projection, commonly called a stop-ridge, was cast on the narrow part of the blade with the object of preventing it from being forced too far into its wooden haft. As the flanges became more marked, they were first confined to the upper part of the tool, and afterwards developed into wings which were hammered inwards so as to form a kind of rudimentary socket.[2] Celts of this form are called palstaves,—a word of Icelandic origin, which denotes a spade. In palstaves of another kind the part between the wings and above the stop-ridge was cast thinner than the rest, so that a groove appeared into which the haft could be securely fitted; and a loop was often added at one side to enable the attachment to be secured by bands of twine.[3] The final improvement was to cast the blade with a socket for the reception of the handle: but palstaves remained in use down to the very end of the Bronze Age;[4] while in some socketed celts the wings survive as mere ornaments upon the sides.[5] Like palstaves nearly all socketed celts are looped on one side, and a few on both.[6] Naturally the socket was not limited to celts, but applied also to knives,[7] chisels,[8] gouges,[9] and other tools. Socketed knives, however, are very rare in Scotland; and on the Continent, except in Northern France, they are almost unknown.[10] On the other hand the patterns

[1] J. Evans, *Anc. Bronze Implements*, pp. 39, 188, 204–5, 222, 487.
[2] *Ib.*, pp. 48, 69–70, 74, 76, 153.
[3] *Ib.*, pp. 70–3, 160.
[4] *Ib.*, p. 468.
[5] *Ib.*, pp. 107–8.
[6] *Ib.*, pp. 160, 162–3.
[7] *Archaeologia*, liv, 1895, p. 98.
[8] J. Evans, *Anc. Bronze Implements*, p. 167. Cf. *Archaeol. Journal*, xxvi, 1869, pp. 346–50.
[9] J. Evans, *Anc. Bronze Implements*, pp. 173, 175–6.
[10] *Ib.*, pp. 207–8.

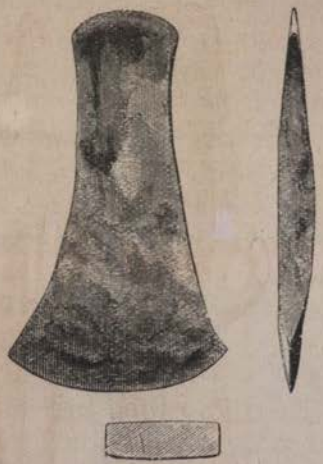


FIG. 18. ½

FIG. 19. ½

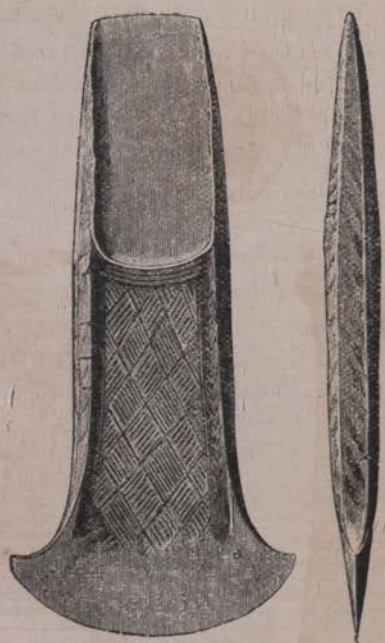
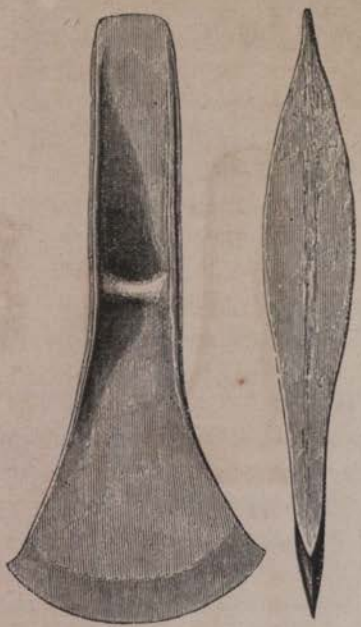


FIG. 20. ½



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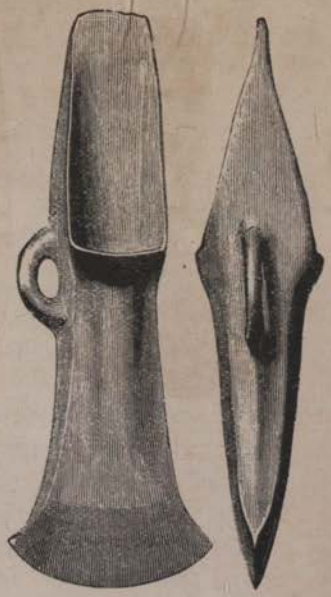

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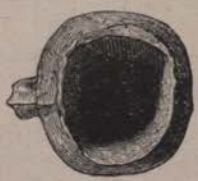

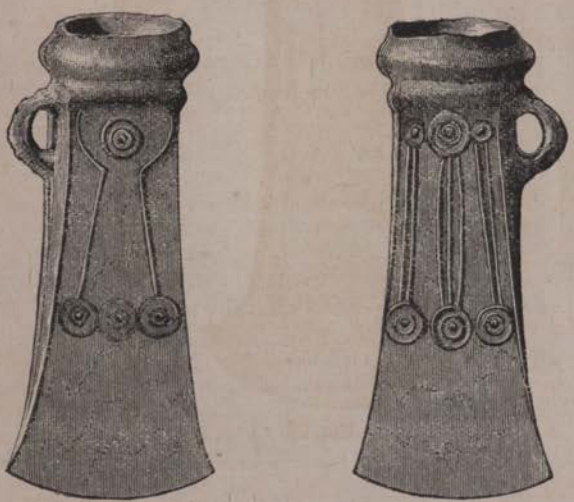

FIG. 23. ½

of our socketed chisels and gouges appear to have been derived from some foreign source.[1]

The earliest British celts were copied not from stone models but from foreign ones of bronze;[2] and our winged celts and palstaves resemble certain French specimens so closely that they too were probably modelled in the first instance upon the latter.[3] The socket also was invented by some ingenious foreign cutler;[4] for palstaves with the wings bent over are rare in this country, whereas socketed celts with ornamental wings are common.[5] Socketed celts were apparently never widely diffused in Northern Britain; and of course even in the south they did not altogether displace palstaves.[6] Even after they began to be manufactured here the output was supplemented by importation from Gaul: a certain type, the blades of which, instead of expanding, are long and narrow, and the sockets almost square, occurs frequently in North-Western France and our southern counties, but very seldom in the north.[7]

Bronze celts in general, like those of stone, were doubtless used for various purposes—as hoes, hatchets, and possibly battle-axes—and some, which are very narrow or very small, as chisels.[8] Palstaves were sometimes used, as their name would suggest, in the construction of earthworks.[9]

Sickles. Sickles probably originated in Southern Europe. The few early specimens that have been found here have their closest analogies in France and Denmark; but, for some

[1] J. Evans, *Anc. Bronze Implements*, p. 480.

[2] *Ib.*, p. 479; *Papers Hants Field Club*, iii, 1895, pp. 56–7.

[3] J. Evans, *Anc. Bronze Implements*, p. 106.

[4] Sir J. Evans (*ib.*, p. 483) observes that 'our socketed celts appear to have had the cradle of their family in Western Germany'. See also *ib.*, pp. 107–8.

[5] *Ib.*, pp. 84, 108, 483. [6] *Ib.*, pp. 108, 135. [7] *Ib.*, pp. 114–5.

[8] *Journ. Roy. United Service Inst.*, xiii, 1870, p. 531; J. Evans, *Anc. Bronze Implements*, pp. 75, 133.

[9] Pitt-Rivers (*Excavations in Cranborne Chase*, iv, 106–7), referring to *Ancient Bronze Implements*, p. 72, remarks that a palstave found on the bottom of the Angle Ditch on Handley Down was probably used in excavating the ditch, the sides of the lower part of which are 'scored all along by vertical grooves', some of which 'coincide with the width of the flat side of the palstave'.

unknown reason, socketed sickles are almost peculiar to the British Isles.[1]

Fig. 24. ½

A hoard was found early in the eighteenth century on Arreton Down, near Newport in the Isle of Wight, which helped to illustrate the evolution of bronze weapons. Daggers, which differed from knives principally in size, though they began to be manufactured later, were originally hafted with rivets; but afterwards they were cast with tangs or shanks, which were let into the handle, and fastened by a single rivet.[2] The Arreton Down hoard contained nine tanged blades, which closely resemble daggers but may have been spear-heads. Many similar blades have been found since, but hardly any outside the British Isles.[3] The Arreton Down hoard.

From daggers were derived a class of weapons very rare in this country, called halberds, which in Scandinavia and Northern Germany have been found mounted as battle-axes. Heavier and broader than their prototypes, they were often made of nearly pure copper, which rendered them less brittle and more suitable for dealing heavy blows.[4] Halberds.

Swords, shields, and, with certain exceptions, spears and javelins were not manufactured until the latest period of the Bronze Age. Swords and spear-heads required great skill in casting: shields were so thin that they could not be cast at all, but were wrought by the hammer.[5] Even at the close of the Bronze Age they were probably unobtainable except by the rich, while the rank and file doubtless Shields, swords, spears.

[1] J. Evans, *Anc. Bronze Implements*, pp. 194, 197–200, 202; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 202; *Proc. Soc. Ant.*, 2nd ser., xv, 1893–5, pp. 358–60.

[2] J. Evans, *Anc. Bronze Implements*, pp. 204, 222–4.

[3] *Ib.*, pp. 257–60, 473, 480; *Archaeologia*, xxxvi, 1855, pp. 326–31.

[4] J. Evans, *Anc. Bronze Implements*, pp. 261–2, 265, 269–70.

[5] *Ib.*, pp. 248–9, 256, 273, 342–3, 354–5.

still made shift with bucklers of wicker-work, wood or leather. The shields of the Bronze Age were invariably circular. Nearly all were ornamented over their whole surface with concentric rings, of which one example has as many as thirty, separated by circles of small studs;

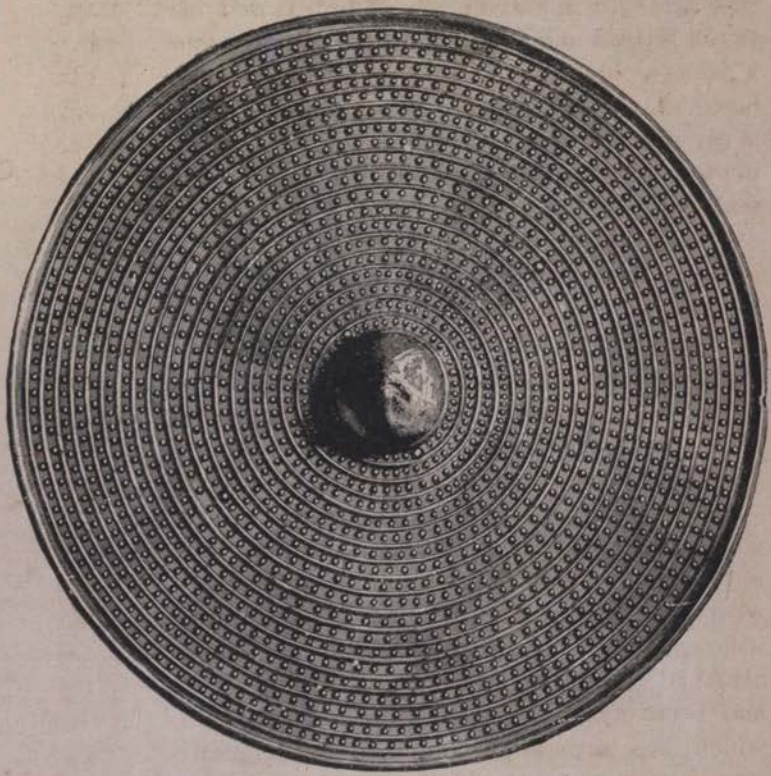

FIG. 25. ⅛

and this ornamentation is peculiarly British. One curious shield, found in the Fen country, is adorned with serpentine lines, which may have been intended to represent snakes.[1]

British bronze swords, like those of the Continent, from which they were copied, are commonly of a type which is called leaf-shaped, the blade tapering gently inwards from

[1] J. Evans, *Anc. Bronze Implements*, pp. 343–8, 354–5, 481; *Archaeologia*, xxvii, 1838, pp. 298–300, liv, 1895, p. 112; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 227.

the hilt, then gradually expanding until, at about one-third of the distance, measured from the point, it attains its greatest width. They, as well as certain rapier-shaped swords, were intended for stabbing, not striking. Their length was generally about two feet, but varied between sixteen and thirty inches. Their sheaths were as a rule made of wood or leather, which, however, were often tipped with bronze; and many of these tips or chapes have been found in the Thames and elsewhere without the scabbards, which had perished.[1]

The spears of the earlier Bronze Age were identical with neolithic flint weapons. Probably the earliest bronze spear-heads were some of the larger blades that have been found in Wiltshire barrows, which are commonly described as knives or daggers.[2] Others were derived from the tanged blades of the Arreton Down type, if, indeed, the latter were not themselves spear-heads. A curious and unique specimen, which was found in the Thames at Taplow, and is now in the British Museum, is ornamented with gold studs on the bottom of the blade, which are merely survivals of the rivets that attached to its haft the dagger from which it had been evolved.[3] Spear-heads of this kind, which are invariably provided either with a pair of holes in the blade or a pair of loops below it, intended to secure its attachment to the shaft,[4] are extremely rare on the Continent,

FIG. 26. ¼

[1] *Ib.*, pp. 171, 173; J. Evans, *Anc. Bronze Implements*, pp. 250–1, 278–81, 297, 301–2, 308, 481; *Archaeologia*, liv, 1895, p. 112.

[2] *Ib.*, xliii, 1871, pp. 455–6; J Evans, *Anc. Bronze Implements*, p. 310.

[3] *Proc. Soc. Ant.*, 2nd ser., xix, No. ii, 1902, pp. 287–9.

[4] 'The openings' [in one specimen], says Sir J. Evans (*Anc. Bronze Implements*, p. 332), 'are about 17 inches from the point. An Irish friend has suggested that they were for the reception

and appear to have been invented in Ireland, whence they spread in the course of trade to Britain.[1] Another form of spear-head, which originated in the British Isles and has never been found elsewhere, was barbed, and seems to have been used for hunting rather than in war.[2] The commonest, however, is the continental leaf-shaped type, some specimens of which have analogies in Gaul and the Swiss lake-dwellings.[3] The smaller weapons of the spear-head class were doubtless javelins.[4]

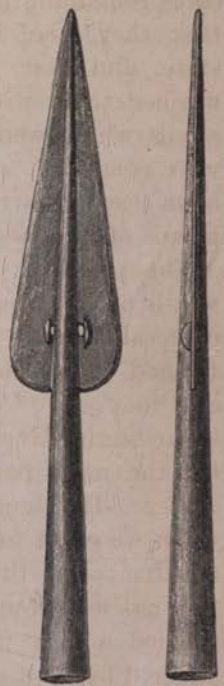

FIG. 27. ½

Moulds.

Many of the moulds in which weapons and implements were cast have been preserved. Open moulds sufficed for flat axes; but the more difficult operations of casting palstaves and socketed celts required that the moulds should be made in halves. All the open ones that remain were of stone; many others, however, were doubtless formed of more perishable materials, such as clay or compact sand. Bronze moulds were also used; but the only specimens which have been found were for palstaves, socketed celts, and gouges. There is a bronze mould in the British Museum that was itself cast in a mould of clay, formed round a model palstave, and attached to it by string, which was of course reproduced in the metal. Leaden celts have once or twice been met with, which of course would have been useless as cutting tools; and it is probable that they were intended simply for making moulds of clay or sand. Bronze moulds were costly, and would soon wear out. It has been suggested therefore that, just as a printer uses in his press not his original wood-block but an electrotype

of poison, but after the blade had penetrated seventeen inches into the human body such an use of poison would probably be superfluous.'

[1] J. Evans, *Anc Bronze Implements*, pp. 341-2; *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 68-9, 81.

[2] J. Evans, *Anc. Bronze Implements*, pp. 337-8, 341-2.

[3] *Ib.*, p. 315.

[4] *Ib.*, p. 311.

copy, so the bronze-founder generally reserved his bronze moulds for making leaden models from which any number of clay moulds could be formed.[1] Sockets were produced by means of clay cores, which were inserted in the moulds. Socketed celts have so often been found in hoards with the cores remaining in them that we may reasonably conclude that they were bartered by the bronze-founders in this state, and that, as in the Neolithic Age, the purchasers finished them with their own hands.[2] The hammers and anvils which were used in the final stage of manufacture were commonly stone, though a few light bronze hammers have been unearthed; and the decoration was applied by means of punches.[3]

Decoration of weapons.

The patterns with which weapons were decorated are worth noticing even by those to whom archaeology for its own sake makes no appeal. Daggers and flat or slightly flanged celts were incised with rectilinear figures and chevrons only:[4] winged celts, palstaves, socketed celts, and spear-heads have similar designs in a few instances,[5] but for the most part they are ornamented with concentric circles. The significance of these facts will become apparent when we come to deal with certain chronological questions relating to the Bronze Age.[6]

Hoards.

What we know of the metal-work of this period has been learned mainly from buried hoards which were never recovered by their owners, and of which more than a hundred have been unearthed in Great Britain from Cornwall to

[1] J. Evans, *Anc. Bronze Implements*, pp. 107, 427–8, 430, 438, 440–1, 445; *Proc. Soc. Ant.*, 2nd ser., xvi, 1895–7, pp. 328–30; xx, 1904–5, p. 259; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 487–505; *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 72–3.

[2] J. Evans, *Anc. Bronze Implements*, pp. 186, 451–2. See p. 71, *supra*.

[3] *Ib.*, pp. 67–8, 177, 179, 181, 451–3.

[4] Daggers with chevron ornament are very rare in the British Isles except in Ireland (*Archaeol. Cambr.*, 6th ser., ii, 1902, p. 221). One or two English specimens are noticed by Sir John Evans (*Anc. Bronze Implements*, pp. 232, 238).

[5] *Ib.*, pp. 108, 320, 330. Mr. Romilly Allen is mistaken when he says (*Archaeol. Cambr.*, 6th ser., ii, 1902, p. 220) that winged, looped, and socketed celts never have chevron ornament. Several instances are given in *Anc. Bronze Implements* (pp. 74, 84, 90, 126, 128, 132).

[6] See pp. 181–4, *infra*.

Sutherland.[1] These hoards were of three kinds.[2] Some, consisting entirely of newly-made articles, belong to traders. Others, which comprise damaged or broken goods, and include moulds and often cakes of copper, represent the stock-in-trade of bronze-founders, who tramped over the country-side, and were ready to cast implements or ornaments of the latest fashion and to melt and recast old ones for anybody who could give them what they wanted in exchange. The tools in these collections were for the most part broken intentionally to make them more portable and ready for the crucible.[3] Other hoards again, which frequently comprise ornaments, alone or associated with implements, were the property of persons who were not in the trade. Hoards were of course buried when robbers were about or when some marauding clan appeared. By far the greater number belong to the latest period of the Bronze Age,[4] which shows that in earlier times the craft had not been specialized, or that people who could afford to buy bronze implements were so few that no travelling dealer could make a fair profit. Those who then possessed bronze tools must have made them for themselves unless there happened to be a skilled craftsman near who could earn a living by working for his neighbours.

The great improvement of tools and weapons would lead us to look for traces of corresponding progress in every department of material culture.

Pasturage. Pasturage of course continued to be the mainstay of the mass of the population; and although there were probably

[1] *Anc. Bronze Implements*, pp. 457–70. For details of hoards which have been found since the publication of Sir John Evans's book, see *Archaeol. Cambr.*, 5th ser., i, 1884, pp. 225–7; *Proc. Soc. Ant.*, 2nd ser., xi, 1885–7, pp. 12, 42–51; xv, 1893–5, p. 138; xvi, 1895–7, pp. 96–8, 327–30; xviii, 1900, pp. 285–7; *Archaeologia*, xlviii, 1888, pp. 106–14; *Proc. Soc. Ant. Scot.*, xxvi, 1892, pp. 182–8; xxxv, 1901, pp. 266–75; *Papers Hants Field Club*, iii, 1895, pp. 53–66; and *Vict. Hist. of . . . Surrey*, i, 241.

[2] *Anc. Bronze Implements*, pp. 457–8; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 504.

[3] Some hoards of damaged and broken implements, unaccompanied by copper cakes, may have been formed for barter with a bronze-founder (*Vict. Hist. of . . . Surrey*, i, 240).

[4] See p. 126, *supra*.

few households which did not subsist partly upon the chase, the remains of funeral feasts in barrows and the refuse heaps of dwellings show that game was eaten much less than the flesh of domestic animals. It has been said that sheep were not introduced into Britain before the Roman conquest; but excavation has proved that they were bred by the bronze-using inhabitants of Dorsetshire.[1] Besides the small cattle that were common in the Neolithic Age large oxen were reared, at all events on Cranborne Chase and the Yorkshire Wolds; and, as in the Neolithic Age and doubtless for the same reason, animals were commonly slaughtered before they had reached maturity.[2] Although bronze fish-hooks, almost identical in form with our own hooks of steel, abounded in the Swiss lake-dwellings, and were present in more than one of the hoards that have been unearthed in France, only a single specimen has yet come to light in the British Isles: but it need not be inferred that the Britons had no taste for fish; for they probably caught trout and salmon with nets or spears.[3]

The growth of population was indeed making it difficult for men to provide for their families; and they were constrained to toil harder in order to avoid starvation. Under this pressure agriculture began to flourish; and wheat was grown at least as far north as Yorkshire.[4] Armed with bronze axes, the husbandmen were better able to clear forests and to bring new land under cultivation; and at harvest time, when they reaped their reward, then, we may be sure, the clansmen gathered, and sacrificed to their god, and held high festival.[5] Their labours are attested not only by numerous stone mullers and by the sickles that have been already mentioned, one of which was found even in Aberdeenshire, but also, as we have already seen,

Agriculture.

[1] Cf. *Archaeologia*, xliii, 1871, p. 536, with Greenwell's *Brit. Barrows*, p. 740, and Pitt-Rivers's *Excavations in Cranborne Chase*, iv, 132.

[2] W. Greenwell, *Brit. Barrows*, pp. 110–1; *Archaeologia*, liv, 1895, pp. 110–1; A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 19.

[3] J. Evans, *Anc. Bronze Implements*, pp. 192, 236.

[4] J. R. Mortimer, *Forty Years' Researches*, &c., pp. 111–2. See also *Brit. Barrows*, p. 114; and *Journ. Anthr. Inst.*, xxxiv, 1904, pp. 392, 396.

[5] See W. Robertson Smith, *The Religion of the Semites*, 1901, pp. 260–1.

by the teeth of the skeletons in the barrows.[1] Oxen were probably used in ploughing.[2] Horses, which were very small, were domesticated, and in certain parts of the country eaten,[3] but they were not common; and, although the rock-carvings of Scandinavia and the bridle-bits and wooden wheels that have been found on the sites of Swiss lake-dwellings show that in the Bronze Age men had learned to ride and drive,[4] similar evidence is wanting in Britain. Looped bronze plates, however, have been found in a hoard at Abergele, which are supposed to have been a jingling ornament, attached to harness; and some small bells, found at Dowris in Ireland, resemble those which occasionally form a part of modern horse-trappings.[5] Oxen indeed, if not horses, must have been required for hauling timber even in neolithic times when clearings had to be made; and the wagons which conveyed tin to the coast when Pytheas visited Cornwall[6] had probably been in use long before his time.

Signs of amelioration in the conditions of life.

Certain facts seem to indicate that the conditions of life in the Bronze Age were becoming more favourable to longevity, and in particular that women were better off than before. Famines indeed must still have occurred; for of course there were bad harvests from time to time, and cattle then, as now, were liable to disease, and doubtless often perished in hard winters. But the disparity in stature between men and women was far less than it had been in the Neolithic Age;[7] and Thurnam estimated the average

[1] See p. 90, *supra*.

[2] Oxen drawing a plough are depicted on rock-carvings in Scandinavia (*Congrès internat. d'anthr. et d'archéol. préhist.*, i, 1874 [1876], pp. 454 [fig. 1], 473 [fig. 31]).

[3] W. Greenwell, *Brit. Barrows*, pp. 220, 262; *Archaeologia*, liv, 1895, p. 110; A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 127–8.

[4] O. Montelius, *Civilisation of Sweden in Heathen Times*, 1888, pp. 71–6; *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 141.

[5] J. Evans, *Anc. Bronze Implements*, pp. 399–400, 404. Harness rings have been found, according to Dr. J. Anderson (*Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 168), in Scottish hoards. Professor W. Ridgeway (*The Thoroughbred Horse*, p. 92) argues that 'the use of the horse by man in the British Isles cannot be placed before the end' of the Bronze Age.

[6] See p. 221, *infra*.

[7] W. Greenwell, *Brit. Barrows*, pp. 656–7.

age of the people of the round barrows whose skeletons he had examined at fifty-five, eight years more than that of the aboriginals.[1] It has been affirmed that even the primitive Aryans often put old people to death ;[2] but skeletons have been exhumed in Britain which showed signs of extreme age.[3]

Dwellings.

One might be inclined to suppose that this amelioration was partly due to improved housing ; but such evidence as exists tends to show that the habitations of the Bronze Age, although, owing to improved tools, they may have been better built, were designed on much the same lines as those of the preceding epoch. Pit-dwellings, like those which have been already described,[4] were still constructed in districts where stone was not obtainable. Very few, as we have seen, can be even approximately dated ; but some which have been excavated at Hitcham in Buckinghamshire and in the fort of Eggardun on the Dorsetshire downs contained pottery which made it safe to assign them to the Bronze Age.[5] It may be that some of the Scottish subterranean dwellings which are known as weems belong to the same period, for a bronze sword was found in one at Monzie in Perthshire ;[6] and perhaps a few of the so-called Picts' houses and of the beehive huts in Cornwall and North Britain, which will be described hereafter, were built before iron was there used.

Lake-dwellings.

It is, as we have seen,[7] very doubtful whether any of the lake-dwellings of Britain were older than the Bronze Age ; and it cannot be positively affirmed that any were as old. One at Barton Mere in Suffolk, if it really was a lake-dwelling,[8] probably belonged to that time, although the only implement found in it was a spear-head ;[9] but the evidence

[1] *Nature*, Nov. 22, 1894, p. 92.

[2] O. Schrader, *Prehist. Ant. of the Aryan Peoples*, p. 379.

[3] W. Greenwell, *Brit. Barrows*, pp. 139, 591-2, 599. [4] See pp. 85-6, *supra*.

[5] See *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 52-3 ; and p. 467, *infra*.

[6] D. Wilson, *Prehist. Annals of Scotland*, 2nd ed., i, 1863, p. 107.

[7] See p. 87, *supra*.

[8] See B. C. A. Windle, *Remains of the Prehist. Age*, &c., p. 279.

[9] *Anc. Bronze Implements*, p. 486. Bronze spear-heads were associated with objects of the Early Iron Age in a hoard found on Hagbourne Hill in Berkshire, which belonged to a period of transition. See p. 267, *infra*.

for the date assigned to the well-known settlement at Holderness is considerably stronger. It has been argued that since both stone and bronze implements were found there, the site must have been occupied before the Iron Age, because, although in a time of transition the old material may persist by the side of the new, implements of two earlier periods would hardly survive into a third.[1]

Hut-circles.

There is, however, one class of dwellings numerous examples of which have been proved to have existed in the Bronze Age, if not before. The best-known groups of hut-circles are those of Anglesey, Dartmoor, Cornwall, and Northumberland. Sportsmen who have shot snipe in Anglesey must have noticed low mounds dotting the rough wastes which are common in the island. Buried beneath these hillocks lie the foundations of huts which were built in prehistoric times. Most of them are clustered in tiny hamlets of five or six; but at Ty Mawr on the southern slopes of the Holyhead Mountain, sheltered from the cold winds by a precipitous cliff and fortified against attack from below, was a considerable village, comprising more than fifty huts. On a clear day the villagers could discern the Wicklow Mountains; and the triple head of Snowdon, haunted, as they surely believed, by some divinity, closed their southward view. The lower walls of the huts, which alone remain, are about three feet thick, and enclose spaces of from fifteen to twenty feet in diameter, partitioned in one instance by upright stones. The entrance, defined by two pillars, invariably faces the south-west. Stones, blackened by fire and doubtless used for cooking, were found within, and also mullers for grinding corn, and the broken shells of the limpets and periwinkles on which the occupants partly lived. Some of the huts, however, appear to have been simply workshops. They were littered with broken quartz from a neighbouring copper lode: the fire-

[1] *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 143. The argument is no doubt generally sound, and no find, as far as I know, refutes it; but I do not think that it is absolutely conclusive. Stone implements were undoubtedly used in the Early Iron Age; and bronze and iron implements have been found together.

places, of which each contained two, one having a chimney in the thickness of the wall, were strewn with slag; and mortars and mullers abounded, which had been used not for grinding corn but for breaking stone.[1] Possibly the huts may have been roofed with converging stones, laid one above another in the beehive fashion; but some in Northumberland and Devonshire contain central cavities, like those of neolithic pit-dwellings, in which poles for supporting a roof of boughs thatched with turf were apparently fixed.[2] Hut-circles everywhere present the same general features; but of course there are numerous varieties of size and construction. Nearly all the huts were round; but a few in East Cornwall are oval;[3] and while most of the hamlets were enclosed by walls, some apparently did not need protection,[4] or were situated near a fort in which the villagers could take refuge. Grimspound on Dartmoor, the typical example of a fortified village, was apparently the stronghold of the people whose huts were scattered on the slopes hard by; and the dwellings which it enclosed may have been occupied in time of peace only by caretakers.[5] Some hamlets were encircled by non-defensive walls, which appear to be the remains of cattle-pens; while in others each pen was connected with its own hut, the walls forming a complex whole.[6] Many huts contain cooking-holes, lined with stones, in some of which traces of charcoal are found:[7] others had cooking-stones but no holes:[8] occasionally the kitchen was in the open air outside the dwelling;[9] and in a circle on Whit-Tor, where no provision for cooking was discernible, there seemed to be evidence that the hut had been simply the workshop of

[1] *Archaeol. Journal*, xxiv, 1867, pp. 229–35; xxvi, 1869, pp. 301–5, 317; xxvii, 1870, pp. 158–9. The workshops may have been used in the Bronze Age; but one, in which iron slag was found, contained Roman coins. Huts similar to those of Ty Mawr have been explored in Brittany (*ib.*, p. 148).

[2] *Archaeologia*, xlv, 1880, pp. 356–8. See p. 86, *supra*.

[3] W. C. Lukis, *Prehist. Stone Monuments of the Brit. Isles,—Cornwall*, pp. 18–9.

[4] W. Greenwell, *Brit. Barrows*, p. 402; *Reliquary*, viii, 1902, p. 92

[5] *Vict. Hist. of . . . Devon*, i, 350, 352.

[6] *Ib.*, p. 349; *Trans. Devon. Association*, xxxiv, 1902, p. 161.

[7] *Ib.*, p. 163. [8] *Ib.*, pp. 161–3.

[9] *Vict. Hist. of . . . Devon*, i, 354.

a flint implement maker.[1] Many of the dwellings on Dartmoor apparently consisted of only one room; while others, like the single specimen on Ty Mawr, contained partitions.[2] Some huts were paved, while others had no visible means of excluding damp.[3] The large size of many of the Dartmoor circles has led antiquaries to believe that they could not have carried roofs sufficiently strong to withstand the snows and storms of winter, and were only occupied in the summer by herdsmen;[4] but in most parts of England huts must have been inhabited throughout the winter, whose roofs were constructed of nothing more substantial than woodwork overlaid with sods or bracken. It is remarkable that not a single bronze implement, weapon, or ornament has ever been found in a hut-circle on Dartmoor, although sufficient pottery of the Bronze Age type remained to attest their age.[5] Probably, like the people who dwelt on the Yorkshire Wolds, the inhabitants were poor and backward; for the extreme scarcity of spindle-whorls and the abundance of the flint scrapers used for leather-dressing that lay scattered in their abodes seem to show that they were commonly clad in skins.[6]

Inhabited camps.

On the borders of Wiltshire and Dorsetshire, and doubtless also in other parts of Britain, small communities erected earthworks for permanent occupation, which differed in size, situation, and mode of construction from the great hill-forts, but were nevertheless adapted to some extent for defence. A considerable number of small entrenchments, approximately square in outline, are scattered over the downs in these two counties; and three of them—Martin Down Camp, South Lodge Camp, and Handley Hill Camp—have been thoroughly excavated. The results left it doubtful whether the last-named had not been con-

[1] *Trans. Devon. Association*, xxxi, 1899, p. 148.

[2] *Ib.*, xxxiv, 1902, p. 160. Cf. *Archaeologia*, xlv, 1880, pp. 362–3.

[3] *Trans. Devon. Association*, xxxiv, 1902, p. 160.

[4] *Reliquary*, N. S., viii, 1902, p. 91.

[5] *Trans. Devon. Association*, xxxiv, 1902, p. 163. Bronze has been found, though rarely, in graves on Dartmoor (*Ib.*, p. 130; *Reliquary*, N. S., vii, 1901, p. 95).

[6] *Ib.*, p. 92.

structed in Roman times;[1] but the other two belonged unmistakably to the Bronze Age. Martin Down Camp covered about two acres; and South Lodge Camp only three-quarters of an acre. The ramparts, which were very low, were probably strengthened by stockades. Both camps were situated not on the summits of hills but in sheltered nooks, and were probably used as enclosures for cattle; but an abundance of broken pottery, animal bones, and burnt cooking flints proved that they had also been inhabited by man.[2]

But the evidence for describing the domestic life of our Bronze Age is insignificant in comparison with that which is afforded by the Swiss lake-dwellings. The most remarkable British habitation of that time, indeed almost the only one which can rival those of Switzerland in the richness of its remains, is not a hut, not even an artificial shelter of the poorest kind. In 1859 some quarrymen were removing limestone from a ravine formed by the Stanhope Burn, a tributary of the Wear, when they discovered the now far-famed Heathery Burn Cave. Antiquaries hurried to the spot; and when a layer of stalagmite had been removed relics began to be found. During thirteen years exploration went on; and finally, besides the bones of the family who had occupied the cave, those of the animals on which they had fed, and the shells of mussels, cockles, and limpets, a vast number of tools, weapons, utensils, and ornaments were collected, which belonged to the closing period of the Bronze Age. A pair of bronze tongs, unique in Britain, The Heathery Burn Cave.

[1] A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 46–8.

[2] *Ib.*, pp. 14, 15–6, 20 (preface), 185–90, and pl. 306. See also *Journ. Anthr. Inst.*, xxxiv, 1904, pp. 387–97. Mr. J. R. Mortimer (*Forty Years' Researches*, pp. 365, 369) describes an 'extensive labyrinth of entrenchments', which 'traversed the high grounds of the [Yorkshire] Wolds in every direction, forming a network . . . connecting hill to hill and valley to valley'. He states (p. 379) that several round barrows have been mutilated by these entrenchments 'in a manner which shows that the latter are the more recent' [but does not show that they are later than the Bronze Age], but that they are 'at least for the most part pre-Roman, being in several instances crossed by what are believed to be portions of . . . Roman roads'; and he concludes, disagreeing with Pitt-Rivers (see p. 441, n. 2, *infra*), that they were intended to protect cattle against robbers. In regard to the entrenchments near Flamborough Head which Pitt Rivers excavated, I prefer his guidance.

and one-half of a mould for casting socketed celts showed that they had been independent of bronze founders; and their outfit comprised two swords, seven spear-heads, nineteen socketed axes, two chisels, three gouges, two socketed knives, a tanged knife, a razor, two implements of deer's horn, three bone knives, a stone spindle-whorl and some flint flakes, fifteen bronze and four bone pins, a bronze cauldron, a gold bracelet, numerous penannular bronze bracelets, including one which was so small that it must have been worn by a little girl, eight large bronze bangles evidently intended to be worn on the upper arm, six bronze disks, whetstones, buttons, and other articles too numerous to mention. Indeed the only bronze objects of any importance which are not represented in the collection are daggers, hammers, sickles, and shields.[1] The cauldron, which is shaped like a truncated cone with the broad end uppermost, belongs to a class of vessels which were not made before the close of the Bronze Age, and are exceedingly rare in England, but not uncommon in Scotland and Ireland. It closely resembles one which was dredged up from the bed of the Thames near Battersea, and which may be seen in the British Museum; and perhaps it may have come in the course of trade from Etruria, where the type originated.[2]

[1] W. Greenwell, *Brit. Barrows*, pp. 107–8; J. Evans, *Anc. Bronze Implements*, pp. 179, 185; *Archaeologia*, liv, 1895, pp. 87–114. Canon Greenwell (*ib.*, p. 103) conjectures that the disks, which have analogues in France, Switzerland, and Italy, may have been worn as ornaments upon the breast; but their use is uncertain. Cf. *Anc. Bronze Implements*, pp. 401–3.

[2] *Ib.*, pp. 408–14; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 205; *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 28–30, 84. Cauldrons with ring-handles and rounded bottoms have been found in Ireland and in various parts of Scotland, but, according to Dr. J. Anderson (*Proc. Soc. Ant. Scot.*, xix, 1885, pp. 313–5; xxxix, 1905, pp. 14–20) and Mr. C. H. Read (*Guide*, &c., p. 30), not on the Continent. Dr. R. Munro, however (*Lake Dwellings of Europe*, 1890, p. 290), affirms that they have been found in the famous settlement of La Tène. They belonged indeed to the very latest period of the Bronze Age, if not to the Early Iron Age (*Anc. Bronze Implements*, p. 410). Only one cauldron of the flat-bottomed type has been found in Scotland (*Proc. Soc. Ant. Scot.*, xxii, 1888, p. 36). The British cauldrons of this kind differ in details from continental examples, especially in their handles, which are rings, and may be of native manufacture. Anyhow, the Heathery Burn Cave cauldron had been mended with a degree of skill which shows that British workmen knew how to rivet plates together.

It had been used for cooking, and was associated with numerous fragments of earthenware. The domestic pottery of the Bronze Age, like the sepulchral vessels, was made by hand,[1] and, unlike them, was fitted to endure rough usage; but while the collection obtained from the cave and nearly all the other examples that have been found are unornamented, the table ware of Dartmoor hardly differs from that which came from the barrows of the same district and is as elaborately decorated.[2] It is also remarkable that many kinds of household utensils—bowls and jars, pans and pannikins, cooking pots, pots for boiling water or meal, pipkins, cups, and strainers—have been discovered in barrows. Some, which were entire, had apparently been deposited instead of regular sepulchral vessels; but many were in fragments, and may have been used in funeral feasts.[3]

The exploration of the Heathery Burn Cave not only illustrates the life of the Bronze Age; it also shows that even in districts far remote from the Continent the use of bronze was not confined to a conquering people but spread to the descendants of the older population. The skeletons in the cave were wholly different from the types which are associated with the round barrows, and closely resembled

[1] Professor Boyd Dawkins (*Early Man in Britain*, pp. 360–2) remarks that if a well-known amber cup, which was found at Hove, was of British workmanship, it proves that the use of the lathe was known in Britain in the Bronze Age. Sir John Evans (*Anc. Stone Implements*, 1897, pp. 445–50), like the professor, thinks that the cup may have been imported; but he points out that cups made of shale, which were certainly turned in a lathe, and were most probably of British manufacture, have been found in round barrows on Broad Down near Honiton. Sir R. C. Hoare (*Anc. Wilts*, i, 122–3) found in the trunk of a tree inside a bowl barrow along with a skeleton an urn which he described as 'different both in shape and colour to any we have ever found in the British sepulchres', and which appeared to him to have been turned in a lathe. Still the statement in the text is, generally speaking, true both of the British Isles and of Northern and Western Europe.

[2] *Trans. Devon. Association*, xxxiv, 1902, p. 125. See also p. 467, *infra*.

[3] *Archaeologia*, xliii, 1871, pp. 338–43, and pl. xxix; liv, 1895, p. 110; W. Greenwell, *Brit. Barrows*, pp. 11, 106–8; *Trans. Devon. Association*, xxxiv, 1902, p. 125; *Proc. Somerset. Archaeol. and Nat. Hist. Soc.*, l, 1905, part ii, p. 42; J. R. Mortimer, *Forty Years' Researches*, pp. lxvii, 9, 82.

those which have been recovered from the beds of rivers in England and Ireland.[1]

But what is most remarkable is the contrast between the wealth of these cave-dwellers and the discomfort in which they lived. Here was a family well armed, equipped with the best tools of the time, owning flocks and herds, possessing land which they cultivated, and rich enough to load their women with ornaments, yet content to live in a dark damp cavern traversed by a stream, which one night rose in flood and drowned them in their sleep. It has been suggested that they had huts in the neighbourhood, and only resorted to the cave on extraordinary occasions.[2] What could have induced them to live in it even for a day is difficult to conceive; but that they inhabited it, if not permanently, at least for long periods, is proved by the abundance of pottery as well as by the heaps of refuse which represented the remains of a long succession of meals.[3]

Dress.

The spindle-whorls of stone, bone, and baked clay which have been found in this cave, in barrows,[4] hut-circles, and elsewhere, and hardly differ from those which, a few years ago, were commonly used in Scottish villages and in many parts of the Continent,[5] are not the only relics that bear witness to the development of dress during the Bronze Age. The deer-horn implements which belonged to the cave-dwellers and exactly resemble others that were obtained from the sites of Swiss lake-dwellings, were probably used in weaving.[6] Bone tweezers from barrows in Wiltshire and Dorsetshire and bronze tweezers from Anglesey were perhaps designed for drawing thread through holes in leather: but they may also have been used for extracting superfluous hairs;[7] and the numerous razors[8] that have come to light,

[1] See pp. 395–7, *infra*.

[2] *Archaeologia*, liv, 1895, pp. 112–4.

[3] *Ib.*, pp. 94, 108.

[4] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 438–9; *Anc. Bronze Implements*, p. 383.

[5] Sir A. Mitchell, *The Past in the Present*, pp. 5–6, 12.

[6] *Archaeologia*, liv, 1895, pp. 108–9.

[7] *Ib.*, xliii, 1871, p. 440; J. Evans, *Anc. Bronze Implements*, pp. 191–2; A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 162–3.

[8] 'So called', says Pitt-Rivers (*ib.*, i, 66), 'because some of them are found with an edge as sharp as a penknife'. The thought of shaving with a bronze

some of which have no parallel in any foreign country, show that Britons, even in the furthest north, shaved their beards many centuries before Caesar noticed the custom.[1] Leathern garments, as we have seen, were largely worn :[2] indeed the remains of a stitched leathern dress have been recovered from a barrow in Northumberland;[3] but more interesting are pieces of the woollen and linen clothes in which the dead were sometimes buried.[4] Nor was the apparel of the Bronze Age devoid of ornament, or fastened merely with thorns, like that of the Germans of a far later period whom Tacitus[5] described. Pins of bone or bronze, some certainly worn with dresses, others perhaps in the hair, were not uncommon; and we have seen how large a store was possessed by a single family.[6] Even the indigent people of the Yorkshire Wolds wore buttons not only of stone, bone, and wood, but of jet, some of which were beautifully ornamented with the pattern of a Maltese cross.[7] During the earlier part of the Bronze Age buttons were pierced on the under side with V-shaped holes, which enabled them to be sewn on to the dress—a device which, on the Continent, was inherited from the Stone Age; and,

Pins and buttons.

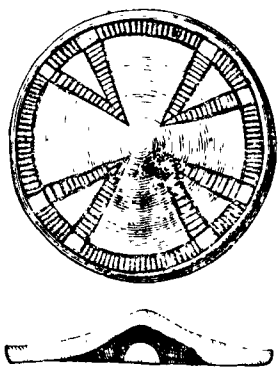

FIG. 28.

razor is not pleasant; but the negroes of Tanganyika still use razors of this metal (*L'Anthr.*, xiv, 1903, pp. 667-75; xv, 1904, p. 116); and everybody knows that the Flamen Dialis might only shave with a bronze knife. See J. G. Frazer, *The Golden Bough*, i, 1900, p. 242.

[1] *Archaeologia*, xliii, 1871, p. 451; liv, 1895, p. 99; J. Evans, *Anc. Bronze Implements*, pp. 217-21, 480.

[2] See p. 156, *supra*. Cf. W. Greenwell, *Brit. Barrows*, pp. 118, 142.

[3] *Ib.*, p. 411. [4] See p. 189, *infra*. [5] *Germania*, 17.

[6] *Archaeol. Journal*, ix, 1852, p. 8; *Archaeologia*, xliii, 1871, pp. 432-4; liv, 1895, pp. 101, 107; W. Greenwell, *Brit. Barrows*, pp. 15, 31, 33; J. Evans, *Anc. Bronze Implements*, pp. 366-73; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 168.

[7] W. Greenwell, *Brit. Barrows*, pp. 32-3, 54-6; *Archaeologia*, xliii, 1871, pp. 510-2, 519-22; J. R. Mortimer, *Forty Years' Researches*, p. xli.

as far as can be judged from the skeletons with which they are associated, they were used only by men. At a later time the perforation was apparently superseded by a raised loop, which is found on buttons of bronze.[1] In Wiltshire and Norfolk chiefs actually adorned their tunics with buttons of gold.[2] Ivory buttons and ivory pins have been unearthed in Wiltshire; and amber buttons were among the ornaments not only of that rich district but of Norfolk and even of Yorkshire and Dorsetshire.[3] Nor were these costly materials used only for personal adornment. A bronze dagger with an ivory handle has been obtained from a barrow near Bere Regis in Dorsetshire : [4] an archer's wrist-guard or bracer of bone, found at Kellythorpe in the East Riding, was decorated with bronze studs, plated with gold : [5] a barrow on Hammeldon Down in Devonshire has yielded a dagger hilt of red amber inlaid with pins of gold ; [6] and from a barrow near Normanton in South Wiltshire Hoare obtained a dagger with a wooden handle exquisitely inlaid in a chevron pattern with thousands of golden rivets, each smaller than the smallest pin. 'It could not,' he wrote, 'be surpassed (if, indeed, equalled) by the most able workman of modern times.' [7] With such a weapon hanging at his side and his dress glittering with gold or amber studs, a British chieftain must have made a splendid show. But some were not content with such display. Early in the last century a cairn was opened at Mold in Flintshire, which was said by the peasants of the country-

Weapons mounted with gold or amber.

[1] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 452–5 ; *Anc. Bronze Implements*, pp. 400–1 ; *Proc. Soc. Ant. Scot.*, xxxvi, 1902, pp. 473–4, 477, 480–5. One button with a V-shaped perforation, found in a barrow at Winterbourne Stoke, Wiltshire, was associated with glass beads (*ib.*, p. 474), which (p. 183, *infra*) appear to have belonged to a comparatively advanced period of the Bronze Age. Cf. *Proc. Soc. Ant. Scot.*, xxxvi, 1902, p. 481.

[2] R. C. Hoare, *Anc. Wilts*, i. 99, pl. x ; *Archaeologia*, xliii, 1871, pp. 524–5.

[3] *Ib.*, pp. 490–1, 502–3 ; J. R. Mortimer, *Forty Years' Researches*, pp. li, 92.

[4] *Archaeologia*, xliii, 1871, p. 492.

[5] W. Greenwell, *Brit. Barrows*, p. 55, n. 1.

[6] *Trans. Devon. Association*, v, 1872, pp. 554–5 and pl. ii.

[7] *Anc. Wilts*, i, 202, pl. xxvii, 2. See also *Archaeologia*, xliii, 1871, p. 459, and *Anc. Bronze Implements*, pp. 51, 232.

side to be haunted by a ghost in golden armour. Three hundred loads of stones were carted away; and then appeared a skeleton, accompanied by three hundred amber beads that had once formed a necklace, and a golden peytrel, mounted on a copper plate, with which the owner had decorated his horse's breast.[1] This interment indeed belonged to the very latest period of the Bronze Age; but much earlier was the barrow of Upton Lovel in South Wiltshire, which contained along with personal ornaments of gold an amber necklace of a thousand beads that had been worn not by a woman but by a man.[2] Ornaments.

But although necklets and bracelets and other ornaments were commonly worn by knights and Druids in Gaul, their use in this country seems to have been generally restricted to women; and, whatever the reason may have been, the women of Britain, then as now, wore less jewellery than those of foreign countries.[3] Still, many specimens, most of which belonged to late periods, are to be seen in the museums which illustrate the culture of the Bronze Age; but for the most part they were either imported or fashioned after foreign designs.[4] Bronze ornaments are comparatively rare[5] although, as we have seen, the family who lived in the Heathery Burn Cave possessed many, and their armlets are absolutely unique.[6] In Scotland as well as in the wealthier parts of England women displayed gold torques of various patterns, some plain, others penannular, which resembled large bangles, others again funicular, of twisted ribbon-like form, or wrought with a pattern like the thread of a screw;[7] while gold bracelets in equal variety

[1] *Archaeologia*, xxvi, 1836, pp. 422–31; *Archaeol. Journal*, lviii, 1901, p. 324; *Proc. Soc. Ant.*, 2nd ser., xviii, 1899–1901, pp. 223–4.

[2] *Archaeologia*, xliii, 1871, p. 501. Cf. p. 502, note c.

[3] See *Anc. Bronze Implements*, pp. 395, 481, 487.

[4] *Ib.*, p. 481.

[5] *Ib.*, p. 395.

[6] *Archaeologia*, liv, 1895, p. 102. Sir John Evans (*Anc. Bronze Implements*, p. 402) does not accept them as armlets; but cf. *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 85.

[7] J. Evans, *Anc. Bronze Implements*, pp. 76, 90, 96, 375–9. Cf. W. Greenwell, *Brit. Barrows*, pp. 55, n. 1, 436. Bronze torques of all these patterns have also been collected. Funicular torques are unknown in Scotland.

Fig. 29. ½

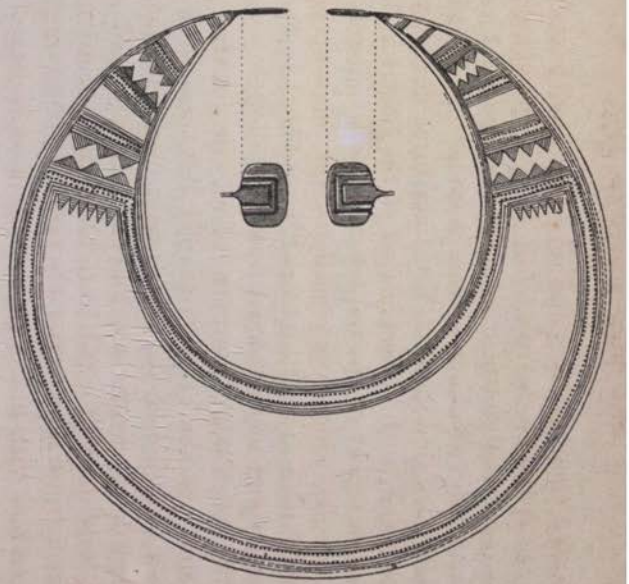

Fig. 30. ⅓

clasped their wrists; and an ivory armlet has been found in a Wiltshire barrow.[1] In 1863 a ploughman, guiding his team at Mountfield in Sussex, turned up a hoard of gold ornaments weighing eleven pounds.[2] A hoard buried in Elginshire contained no less than three dozen gold armlets, belonging to the latest period of the Scottish Bronze Age; and an armlet of twisted wires, made to encircle the arm in four coils, which was considered the finest specimen of the goldsmith's art of this period ever found in Scotland, was cut up and melted down by an Edinburgh jeweller.[3] The most interesting, however, of all the Scottish gold ornaments are the crescent-shaped lunettes, worn round the neck, which were of Irish origin, and of which only four English specimens are known.[4] They would seem to be of early date; for two were found in association with a flat celt.[5] Rings were extremely rare;[6] and ear-rings have only been met with in Derbyshire, the East Riding of Yorkshire, and the north of Scotland.[7] A pair which was found in a grave in Morayshire can only be described as hideous. They were made of gold, in shape like an open shell or pod, five inches and a half long, and suspended at right angles

[1] J. Evans, *Anc. Bronze Implements*, pp. 283, 381–7; *Archaeologia*, xliii, 1871, pp. 489–90, 528; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 94–5, 168, 217; A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 6.

[2] *Proc. Soc. Ant.*, 2nd ser., ii, 1861–4, pp. 247–8; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 224, n. 1; Mr. G. Clinch (*Vict. Hist. of . . . Sussex*, i, 320) thinks that the Mountfield hoard probably belonged to the Late Celtic period.

[3] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 217, 220–1.

[4] *Archaeol. Journal*, xxiv, 1867, pp. 197, 201; J. Evans, *Anc. Bronze Implements*, p. 42; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 222; *Rev. celt.*, xxi, 1900, pp. 166–73; *Vict. Hist. of . . . Cornwall*, i, 356. Cf. *L'Anthr.*, xvii, 1906, p. 135.

[5] See *Proc. Soc. Ant. Scot.*, xxxv, 1901, p. 263.

[6] *Archaeologia*, xliii, 1871, p. 470; J. Evans, *Anc. Bronze Implements*, pp. 390–1.

[7] *Ib.*, pp. 391–3; *Archaeologia*, xliii, 1871, pp. 471, 531; W. Greenwell, *Brit. Barrows*, pp. 117, 223, 324; J. R. Mortimer, *Forty Years' Researches*, pp. xlvi, 218. A pair of ornaments, which may have been ear-rings, have been found in a barrow in Wiltshire. Cf. *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 94, with *Anc. Bronze Implements*, p. 393. Ear-rings of the Bronze Age are equally rare in France (*Proc. Soc. Ant. Scot.*, xxxv, 1901, pp. 267, 273).

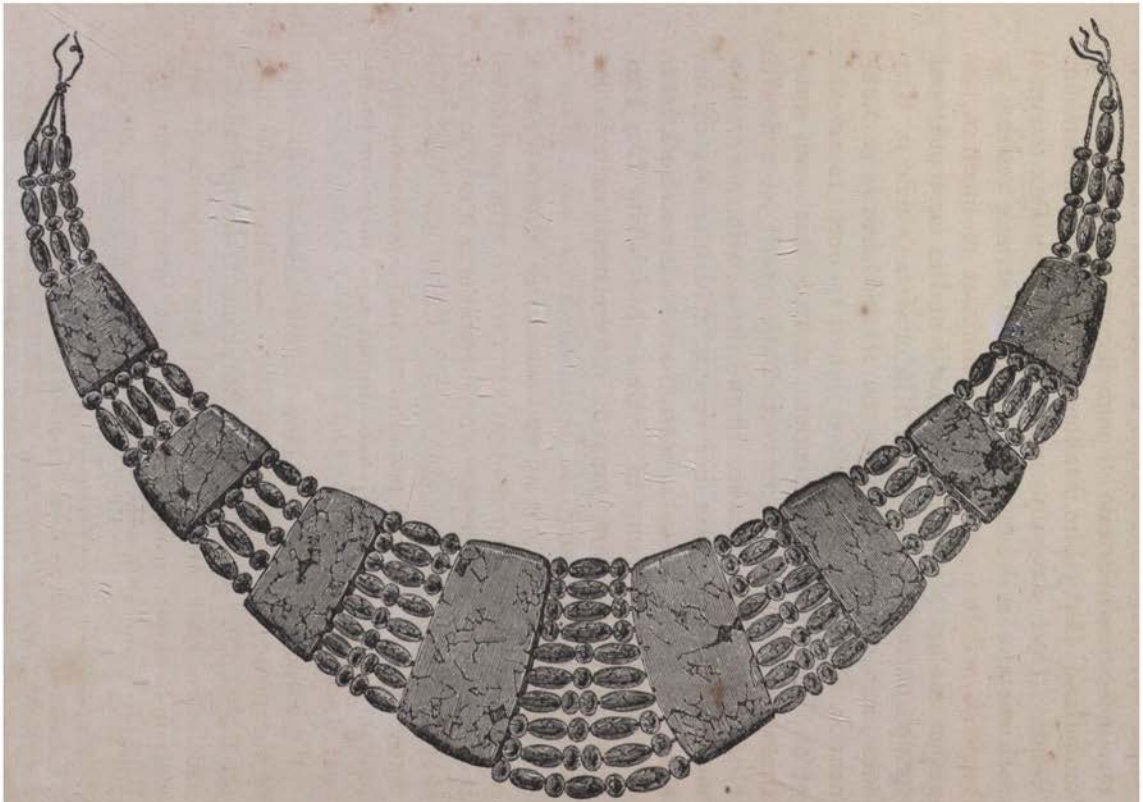

FIG. 31. $\frac{1}{2}$

to the hook.[1] Perhaps the most beautiful and characteristic ornaments of the Bronze Age were the jet necklaces, which were very common in Scotland and comparatively rare in Southern Britain, though they were worn in Northumberland, Derbyshire, Staffordshire, and on the Yorkshire Wolds. They generally consisted of flat plates, adorned with chevron or lozenge patterns, and strung together by bugle-shaped beads.[2] A similar necklace of quadrangular amber tablets, connected by beads of the same material, formed part of the treasures of a chieftain's wife in Wiltshire, and was deposited in one of the barrows at Lake, near Stonehenge.[3] Amber was indeed the most fashionable of all ornaments in this region, where it was worn sometimes alone, sometimes in combination with jet and with blue or green glass beads. In full dress, with one of these necklaces hanging over her bosom, gold bracelets on her arms, a pair of gold disks, bearing devices like a Greek cross, on her dress, and pins of bronze, which shone like gold, in her hair, a Wiltshire dame must have surpassed even her husband in splendour.[4]

Those who could not afford such costly ornaments were not always obliged to content themselves with perforated boars' teeth or bone beads; for, incredible as it may appear, sham jewellery was in vogue even in the Bronze Age. Not many years ago three penannular rings, picked up by a ploughman near Forfar, were found to consist of bronze coated with gold leaf.[5]

Distribution of wealth: sources of gold, ivory, and amber.

While these things help us to realize the circumstances of the people who wore them, they also throw light upon the distribution of wealth, and supplement the information which we have already obtained from implements and weapons about internal trade and foreign commerce. Possibly some difference of burial customs may account

[1] J. Evans, *Anc. Bronze Implements*, pp. 392–3

[2] *Archaeologia*, xliii, 1871, pp. 514–5, 522; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 51–2; *Proc. Soc. Ant. Scot.*, xxxv, 1901, pp. 270–1; *Archaeol. Cambr.*, 6th ser., ii, 1902, p. 209.

[3] See p. 469, *infra*.

[4] *Archaeologia*, xliii, 1871, pp. 494–5, 500, 504, 526–7.

[5] *Proc. Soc. Ant. Scot.*, xxvi, 1892, p. 183.

for the comparative abundance of gold ornaments in Scotland and the almost entire absence of trinkets of any kind in Cornwall; but the evidence is generally accepted which seems to point to the conclusion that the inhabitants of Wiltshire—especially of Salisbury Plain—were richer than those of any other part of Southern Britain. The most expensive ornaments—amber, gold, ivory, and glass—have been found there in considerable numbers; and all of them must have been imported, directly or indirectly, in some cases from abroad. The glass beads, which, strictly speaking, were made of vitreous paste, perhaps came from the Mediterranean; and a blue one of real glass with yellow spirals, taken from a Ross-shire barrow, had its counterparts in the cemetery of Hallstatt.[1] Where the ivory was procured is doubtful: objects of this material, apparently made from the fossilized tusks of a mammoth, lay among the relics in the Paviland Cave in Glamorganshire;[2] but most of the mammoth tusks in this country are too decomposed to be susceptible of manufacture.[3] Gold has been obtained from most of the alluvial gravels in the West of England that have been worked for tin;[4] but many of the English and perhaps all the Scottish gold ornaments were made of gold that had been won in Ireland, which has been justly called the El Dorado of the ancient world. Many gold ornaments in Denmark are of Irish origin; and the leading archaeologist of Scandinavia affirms that the metal-workers of his own country and of France imported Irish gold.[5] Amber has been washed ashore at Deal and on other parts of the

[1] J. Evans, *Anc. Bronze Implements*, pp. 394, 485; *L'Anthr.*, xvi, 1905, pp. 173-5. Mr. L. McLellan (*Proc. Soc. Ant. Scot.*, xl, 1906, pp. 396-402), unlike Mr. Abercromby (*Journ. Anthr. Inst.*, xxxix, 1905, p. 262), argues that the paste beads were made in Britain.

[2] *Archaeologia*, xliii, 1871, pp. 492-4.

[3] J. Evans, *Anc. Bronze Implements*, p. 485.

[4] *Journ. Roy. Inst. Cornwall*, xvi, 1904, p. 103.

[5] *Journ. Roy. Soc. Ant., Ireland*, 5th ser., v, 1895, p. 23; *Report of . . . the Brit. Association*, 1896, pp. 912-4; *L'Anthr.*, vii, 1896, pp. 688-9; *Rev. celt.*, xxi, 1900, pp. 166-75; *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 29, 145-6; *Man*, v, 1905, No. 7, p. 13. For evidences of intercourse between Scotland and Ireland see *Proc. Soc. Ant. Scot.*, vi, 1867, pp. 350-1; W. Greenwell, *Brit. Barrows*, p. 62; and R. Munro, *Prehist. Scotland*, p. 290.

east coast; and the necklaces of Wiltshire may perhaps have been generally of British material as well as of British workmanship:[1] but those of Ireland were probably made from amber that had come from Scandinavia,[2] and may have been taken in exchange for gold. In the time of Augustus amber was one of the British imports;[3] and, although at least one necklace found its way even to Orkney,[4] its rarity in Scotland and in the northern counties of England suggests that it was imported even in the Bronze Age.[5] Indeed, since amber was so much commoner in Wiltshire than elsewhere, it would seem probable that it came generally from abroad.[6]

Why was Wiltshire exceptionally rich in ornaments?

But why was it so abundant in Wiltshire? Why are gold, amber, and ivory rare even in the other southern counties, and wholly absent in Derbyshire, where round barrows are so numerous?[7] Why was the wealth of Wiltshire, so far as it can be estimated from the evidence of the graves, almost entirely concentrated in the south, and especially in the district round Stonehenge?[8] The modern population of South Wiltshire is very scanty: Salisbury Plain is barren; and the only soil at all fertile is in the valleys of the Wiley and the Avon.[9] One would

[1] *Archaeologia*, xliii, 1871, pp. 508–9; J. Evans, *Anc. Bronze Implements*, p. 484; *Journ. Roy. Soc. Ant. Ireland*, 5th ser., vi, 1896, p. 37, n. 1.

[2] This conjecture, I find, has the support of Mr. Coffey (*ib.*, p. 39).

[3] See p. 357, *infra*.

[4] *Proc. Soc. Ant. Scot.*, iii, 1860, pp. 183, 195.

[5] *Ib.*, xxvi, 1892, pp. 186–7; W. Greenwell, *Brit. Barrows*, p. 55.

[6] *Journ. Anthr. Inst.*, xxxv, 1905, p. 261.

[7] W. Greenwell, *Brit. Barrows*, pp. 54–6; *Archaeologia*, xliii, 1871, pp. 492, 494–5, 507, 530; lii, 1890, pp. 58–9. A glass bead has been found in one Derbyshire barrow (*Proc. Soc. Ant.*, 2nd ser., xv, 1893–5, p. 425). Gold has only once been found in a barrow on the Yorkshire Wolds (*Archaeologia*, lii, 58–9); and of 379 interments only 10, of which 2 were Late Celtic, were found there accompanied by ornaments (*Brit. Barrows*, pp. 51–2); whereas in Wiltshire 64 were found out of 354 (*Archaeologia*, xliii, 488).

[8] The absence of gold and amber which distinguishes the group of barrows round the great stone circle at Avebury from those associated with Stonehenge (*Crania Britannica*, ii, pl. 11, p. 5, n. 11) is remarkable. Perhaps it may be due partly to the greater antiquity of the Avebury barrows.

[9] In regard to the poverty of the people who, probably in the Early Iron Age, used the stronghold of Winkelbury Hill, 13 miles WSW. of Salisbury, see A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 236.

have expected to find that the wealthiest part of Britain was the south-east; and that in the prehistoric period, as in the time of Caesar, the richest of all was Kent. Yet Kent has yielded very few glass beads or gold ornaments of the Bronze Age, and not one of amber or ivory. Doubtless there were once many barrows in the south-eastern counties which have been rifled or ploughed down; but jewellery was not deposited only in barrows; and so many bronze tools and weapons have been found in this region that the scarcity of barrows will not account for the rarity of ornaments. No explanation, so far as I know, has ever been offered; and I offer one with diffidence. First, it is not certain, and indeed improbable, that more than a small proportion of the riches that have been unearthed from the sepulchres of South Wiltshire belonged to families who had lived in the neighbourhood. The prodigious abundance of barrows around Stonehenge can only be explained by supposing that the bodies of chieftains, of their wives and children, were brought from distant parts to be buried there, as to a hallowed spot. Secondly, it is conceivable that the clans which, early in the Bronze Age, settled in South Wiltshire were numerically stronger, better organized, or better armed than their neighbours, and that much of their wealth may have been obtained by plunder.

British trade and the spiral.

Another indication of ancient British trade appears in the geographical distribution of the spiral. This form of decoration, which was common in Egyptian and Aegean art, travelled along the route of the amber trade by the Danube valley and Hungary to Scandinavia, and ultimately reached the British Isles, where, however, it occurs only on stone balls,[1] the stones of cists, and megalithic monuments, of which the most conspicuous example is New Grange in

[1] Dr. Joseph Anderson assigns these balls, which have been found only in Scotland, to the Iron Age; but Mr. George Coffey (*Journ. Roy. Soc. Ant. Ireland*, 5th ser., vi, 1896, p. 42) thinks that 'the general character of these spirals appears to be distinctly Bronze Age, not Late Celtic'. It has been pointed out (*Proc. Soc. Ant.*, 2nd ser., xvi, 1895–7, pp. 408–9) that Mr. Coffey's theory is 'strengthened by the fact that stone balls of this class have been found associated with cist burials [of the Bronze Age] . . . near Ballater, and . . . [in] Elginshire'.

the county Meath. The spiral is not found on objects of the Bronze Age in Spain, nor in France except on the dolmen of Gavr' Inis in Brittany and in a grave in the department of the Aube: in the British Isles it is confined to Scotland, Cumberland, Lancashire, and Northumberland, the north of Ireland,[1] and Merionethshire (which may have owed its solitary specimen to Irish influence); and, moreover, in the British Isles and Scandinavia spirals are connected by the same device.[2] Scandinavia therefore was undoubtedly the source from which the spiral reached Britain.[3]

Yet while the reader who has been accustomed to suppose Compara-

[1] Meath, Louth, Fermanagh, Tyrone, and Donegal.

[2] See Mr. G. Coffey's articles in *Journ. Roy. Soc. Ant. Ireland*, 5th ser., iv. 1894, pp. 349-79; v, 1895, pp. 16-29, 195-211; vi, 1896, pp. 34-69, and especially 40-2, 65; *Proc. Soc. Ant. Scot.*, xxi, 1887, p. 144; xxiii, 1889, p. 133; xxix, 1895, pp. 191-4; xxxiii, 1899, pp. 363-4, 368; *L'Anthr.*, vii, 1896, pp. 688-9; xvii, 1906, p. 332, fig. 6; *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., ii, 1902, pp. 381-2; Romilly Allen, *Celtic Art in Pagan and Christian Times*, 1904, pp. 50-3; C. H. Read, *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 96; and *Vict. Hist. of . . . Lancs*, i, 240. Mr. Read observes (*op. cit.*, p. 103) that some of the Scandinavian bronzes have been shown by analysis to have been imported from Britain.

[3] M. Salomon Reinach (*L'Anthr.*, iv, 1893, pp. 688-90), if I do not misunderstand him, although he admits that the British Isles in the Bronze Age were connected by trade with Scandinavia, thinks it probable that the spiral reached the former by way of Spain. He observes that among Scandinavian rock-sculptures there are no spirals; that certain designs—for example the boat—are common to sepulchral monuments in Brittany and in Ireland; that there are striking points of resemblance between the bronze culture of Ireland and Spain; and that designs which have been found in the East Riding of Yorkshire represent Aegean types which also appear in the departments of the Marne and the Gard (see p. 200, *infra*). These arguments seem unavailing against those stated in the text, and especially against the almost complete absence of the spiral from Spain, Gaul, and Southern England. If the figure does not appear on Scandinavian rocks, it abounds on Scandinavian weapons and ornaments; and on rocks boats are frequently represented (O. Montelius, *Civilisation of Sweden in Heathen Times*, 1888, pp. 46, 73-6). Nor is M. Reinach's reasoning sound when he goes on to argue that we have no right to trace the Bronze Age spiral of Western Europe to an Egyptian source because the same design has been found engraved on mammoths' tusks in the Pyrenaean cave of Espélungues at Arudy (*L'Anthr.*, xv, 1904, p. 146, fig. 24; xvi, 1905, p. 5, fig. 4). As Dr. Arthur Evans remarks (*Report of . . . the Brit. Association*, 1896, p. 913), 'the earliest cultural strata of Europe, from the Neolithic period onwards, betray an entire absence of the recurring spiral motive. When we find it later propagating itself as a definite ornamental system in a regular chronological succession throughout an otherwise inter-related European zone, we have every right to trace it to a common source.

tive backwardness of culture in Britain.

that the Britons even of Caesar's time were mere savages may be astonished to learn that already in the Bronze Age there was commercial intercourse between Britain and the Continent, he must beware of assuming that his forefathers were on a level with the inhabitants of Central and Southern Europe. Our country has long been the geographical centre of the civilized world: in ancient times it was outside the pale. Regular trade did not exist except with Northern Gaul and, probably towards the end of the age, with Massilia and Phoenician Spain:[1] such articles of commerce as found their way to Britain from Central Europe were flotsam and jetsam. Long after swords had come into use abroad the Briton's chief weapon was still a stout dagger: bronze was used here for centuries after iron had been adopted in more fortunate lands; and the glass beads of which the women of Wiltshire were so proud would have been scorned by foreign ladies who compared them with their own.[2] Moreover, even in bronze our workmanship never reached the pitch of excellence which the artificers of the north, in their prolonged Bronze Age, were able to attain. Just as the neolithic cutlers of Britain were inferior to those of Denmark, so there is nothing in our museums which can vie with the astonishing splendour of the decorated palstaves and shields, the trumpets and vessels of the Scandinavian region.

The information obtainable from graves.

But we shall be better able to understand the relations that existed between our country and the Continent in the Bronze Age when we have studied the graves, the objects other than weapons, implements, and ornaments that have been found within them, and the rude stone monuments with which they were often associated.

[1] See pp. 499–500 and 511–4, *infra*.

[2] See *Journ. Anthr. Inst.*, xxx, 1900, p. 94, and the Hon. John Abercromby's article in the same periodical, xxxv, 1905, pp. 256–64, especially 262. Mr. George Coffey (*Journ. Roy. Soc. Ant. Ireland*, 5th ser., vi, 1896, p. 40), remarking that Britain and Gaul were on a lower plane of civilization in the Bronze Age than Scandinavia, argues that one cause may have been that 'the sea-way south of the Elbe was possibly closed to Scandinavian enterprise in the Bronze Age'. But the North Sea was not; and apparently there was nothing to prevent Scandinavian traders from landing on our eastern coasts if they had thought it worth while.

Round barrows, cairns, and sepulchral circles.

We have seen that round barrows were already being erected before the Bronze Age began, and that they were used not only by the round-headed invaders but also by the older population.[1] After the close of the Neolithic Age no more long barrows were constructed,[2] although some of those which existed were still used even under the Roman occupation;[3] nor were the dead buried, except perhaps in certain Cornish cairns,[4] in chambers which were intended to be opened from time to time. Thenceforward the graves were cists, commonly made of four stones set on edge, which were closed by a fifth once for all after the corpse or burnt bones had been laid within them;[5] or, where no stones could be obtained, holes scooped in the chalk,[6] and sometimes even hollowed trunks of trees or real coffins.[7] Occasionally, however, the body, burnt or unburnt, was laid upon the ground without anything to protect it from the superincumbent mass.[8] When a tumulus was erected, whether it was an earthen barrow or a cairn, its form was usually round and occasionally oval. The change involved degeneration.[9] Galleries were no longer required. The chambered cairns of the north gave way to structureless heaps of stone: the chambered long barrows of England

[1] See p. 119, *supra*. Prof. B. C. A. Windle (*Remains of the Prehist. Age*, pp. 153–73) gives a fairly complete list. It would be superfluous to print references for barrows belonging to counties not mentioned in my text; for full lists are being given in the *Victoria County History*.

[2] Long barrows may possibly have been erected in remote districts after bronze had been introduced into Southern Britain. See *Archaeol. Cambr.*, 5th ser., viii, 1891, pp. 33–7.

[3] A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 58–9. Cf. *Archaeologia*, xlii, 1869, p. 196.

[4] See p. 108, *supra*.

[5] W. Greenwell, *Brit. Barrows*, pp. 13, 451; *Trans. Devon. Association*, xxxiv, 1902, p. 111.

[6] *Archaeologia*, xliii, 1871, pp. 309, 314, 326; W. Greenwell, *Brit. Barrows*, pp. 12–3.

[7] *Ib.*, pp. 31–2; R. C. Hoare, *Anc. Wilts*, i, 52, 122–5; J. Hutchins, *Hist. and Ant. of Dorset*, 3rd ed., i, 1861, p. 100; *Archaeologia*, xliii, 1871, pp. 314–5; A. Pitt-Rivers, *Excavations in Cranborne Chase*, i, 4; *Proc. Soc. Ant. Scot.*, xxxix, 1905, pp. 179–81; J. R. Mortimer, *Forty Years' Researches*, p. xxvii.

[8] J. R. Mortimer, *Forty Years' Researches*, p. xxv.

[9] The same degeneration took place in Gaul (A. Bertrand, *Archéol. celt. et gaul.*, 1889, p. 104; *Rev. de l'École d'anthr.*, xv, 1905, pp. 213–4).

with their portals, entrance-passages, and graceful exterior curves were succeeded by mere mounds.[1]

What would first impress an ordinary wayfarer is the vast number of the round barrows compared with the rarity of those of the older form. The mounds clustered in the immediate neighbourhood of Stonehenge many times outnumber all the long barrows in Britain. Three hundred still exist in an area of twelve square miles; and from one spot hard by the great stones Stukeley counted a hundred and twenty-eight.[2] Again, while the long barrows almost always stand on conspicuous hills, round barrows are sometimes placed on low ground.[3] In certain maritime districts, for instance Cornwall and Brittany, it has been noticed that the monuments of the dead are most thickly strewn in the extreme west, as if the builders had desired that the spirits of those who had gone before them might look upon the setting sun.[4]

The material, it need hardly be said, varied according to the resources of the district. In Scotland, Wales, and Cornwall cairns are almost universal, some being of gigantic size. In 1876 a cairn in Fifeshire was opened; and after more than a thousand cartloads of stones had been removed, a solitary cist appeared, containing one interment.[5] Sometimes, however, mounds of various kinds coexist in the same region: thus in Devonshire we find round barrows, cairns, and small central cairns covered by round barrows.[6] In other counties again barrows made of earth, of chalk, and of earth and chalk mixed may be seen close together.[7] Curiously enough many barrows on the Yorkshire Wolds

[1] Mr. J. R. Mortimer (*Forty Years' Researches*, p. xxv) believes that on the Yorkshire Wolds barrows were occasionally erected over the dwellings in which the dead had lived; but the evidence which he adduces, except in one instance (pp. 182–3), appears to me weak. See pp. 155, 328–9, 336–7.

[2] *Stonehenge*, 1740, p. 45.

[3] *Archaeologia*, lii, 1890, p. 63.

[4] *Ib.*, xlix, 1885, p. 183.

[5] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 3–4; *Trans. Glasgow Archaeol. Soc.*, N. S., iii, part ii, 1899, p. 499.

[6] *Trans. Devon. Association*, xxxvii, 1902, p. 106.

[7] *Archaeologia*, xliii, 1871, p. 309; lii, 1890, p. 63; *Proc. Soc. Ant.*, 2nd ser., viii, 1879–81, p. 289.

were constructed of clay which had been fetched from distant places.[1]

Round barrows range in diameter from twenty to one hundred and fifty feet; and while some are even now twenty-four feet high, others barely rise above the level of the surrounding ground.[2] Those of the oldest form, which, however, continued to be erected contemporaneously with others of later types, have some resemblance to a shallow inverted bowl. More than three-fourths of the Wiltshire barrows belong to this variety, which is also prevalent in Yorkshire and almost invariable in Derbyshire, Dorsetshire, Somersetshire and the Orkney Islands.[3] These mounds are occasionally surrounded by shallow ditches, in which cases they represent a transition to the form which is called bell-shaped.[4] Barrows of the latter kind, which stand on a flat area surrounded by a ditch, but not by a bank, and are larger, steeper, and more conical than those of the primitive form, are far more numerous in Wiltshire, and especially round Stonehenge, than elsewhere; although a few exist in other parts of Southern England, and some of the so-called bowl barrows in the East Riding can hardly be distinguished from them.[5] Latest of all were the disk-shaped barrows,—small mounds standing alone, in pairs, or in groups of three, within a circle defined by a ditch, which is fenced on its outer side by a bank. Occasionally the enclosure contains no mound at all, but only a grave dug out of the chalk; on the other hand, in one instance the whole area within the ditch is covered by a low mound. Disk barrows are commonest near Stonehenge, and outside

[1] J. R. Mortimer, *Forty Years' Researches*, p. xxi.

[2] *Archaeologia*, xliii, 1871, pp. 291-2; W. Greenwell, *Brit. Barrows*, pp. 4-5.

[3] *Ib.*, p. 3; *Archaeologia*, xliii, 1871, pp. 290-1, 301-4; A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 7-8, 64.

[4] *Archaeologia*, xliii, 1871, p. 291. It has often been assumed that barrows had no ditches because none were visible; but Pitt-Rivers (*Excavations in Cranborne Chase*, i, 4) showed that several barrows on his estate were surrounded by ditches 'of which no trace was seen before excavating'. See also *ib.*, ii, 7-8.

[5] *Archaeologia*, xliii, 1871, pp. 302-4; W. Greenwell, *Brit. Barrows*, pp. 3-4. Composite bowl barrows and bell barrows are described in *Archaeologia*, xliii, 297-300.

Wiltshire they are hardly to be found except in the adjoining corner of Dorsetshire, on the Cotswold Hills, in Sussex, and, though rarely, in Derbyshire. As they contained ornaments more frequently than the other kinds, it has been supposed that they were specially devoted to the interment of women ;[1] but we may accept the explanation that, like the barrows, the ornaments for the most part were comparatively late.

The significance of the ditches and banks has puzzled many antiquaries. There are barrows close to one another, some of which are surrounded by ditches, while others have none ; while in districts in which stone is abundant there are barrows enclosed by or enclosing small circles of stones, and others which have neither one nor the other.[2] Perhaps the barrows enclosed by circles are comparatively late, and the stones may sometimes have been intended, wholly or

[1] *Archaeologia*, xliii, 1871, pp. 293–5, 303–4 ; *Folk-Lore*, vi, 1895, pp. 14–5. Pitt-Rivers (*Excavations in Cranborne Chase*, iv, 145) suggests that the common form of disk barrows 'may have arisen through a failure to carry out the original intention'. 'The first idea,' he continues, 'of the mourners . . . may probably have been to erect a large monument . . . and the ditch in such a case would contain a large area. In the course of a few days, however, the grief may have abated, and laziness supervened, in which case the arrested tumulus would assume the form described. The habit of all primitive peoples . . . of lashing themselves up into a frenzy on the occasion of a death, and general excitability upon any uncommon occurrence, followed by a speedy relapse, favours this hypothesis. When, however, a vallum is seen to follow the line of the ditch, this cause cannot be assigned to the particular structure. It may, however, be a form that has become persistent and conventionalized through the cause already mentioned.' This ingenious theory seems to imply that the motive of laziness only began to operate when disk barrows came into fashion.

The few disk barrows of Derbyshire have no apparent ditches (*Vict. Hist. of . . . Derby*, i, 169). That county also contains barrows constructed differently and of different materials from those which undoubtedly belong to the Bronze Age : their date is uncertain, but may be Romano-British (*ib.*, pp. 186–9 ; *Proc. Soc. Ant.*, 2nd ser., xv, 1893–5, p. 427).

Mr. G. F. Tregelles (*Vict. Hist. of . . . Cornwall*, i, 358) thinks that in Cornwall the distinctions between conical, bowl, bell, flat, and ring barrows 'may be little more than differences in height'.

[2] *Journal Brit. Archaeol. Association*, xviii, 1862, p. 39 ; *Proc. Soc. Ant.*, 2nd ser., viii, 1879–81, pp. 289, 291–2 ; x, 1884–5, pp. 305–6 ; W. C. Lukis, *Prehist. Stone Monuments of the Brit. Isles,—Cornwall*, p. 6 ; *Archaeologia*, lii, 1890, p. 63 ; *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., i, 1901, pp. 295–9.

in part, to give form and symmetry to the mound; for in Derbyshire, where the barrows of the Bronze Age are really cairns, a structural improvement was made by building up the whole mound of concentric rings of stones.[1] Again in Wiltshire ditches and banks are invariably complete;[2] whereas on the Yorkshire Wolds banks and ditches or circles of stones are generally incomplete; and this characteristic, which belonged, as we have seen, to certain long barrows,[3] is repeated not only in megalithic circles in the British Isles and in India, but also in rings which are carved on rocks and on the covering stones of cists.[4] It has been suggested that the banks and stone circles were intended to bar the exit of the dreaded spirits of the dead;[5] but if this was the purpose of the builders, why did they leave the barrier imperfect? It is possible that their motive was not superstitious but utilitarian: the break may simply have been a causeway intended to give access to the barrow.[6]

Round barrows and cairns, like long barrows, are commonly supposed to have been erected only as memorials of chiefs, their relatives, and perhaps their honoured retainers;[7] for,

[1] *Vict. Hist. of . . . Derby*, i. 169.

[2] The ditches of the Cranborne Chase barrows in Dorsetshire, just outside the frontier of Wiltshire, are sometimes incomplete (A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 138).

[3] See p. 104, *supra*.

[4] W. Greenwell, *Brit. Barrows*, pp. 6–8; *Proc. Soc. Ant.*, 2nd ser., xi, 1885–7, p. 434. Canon Greenwell (*Proc. Soc. Ant. Scot.*, vi, 1867, p. 339), speaking of a cairn near Crinan in Argyllshire, surrounded by 'a double circle of stones', which 'stood from 3 feet to 5 feet apart, except for a space . . . where, in both circles, four stones were found placed close together', says, 'This is not an unusual feature in circles which enclose burials; in fact it is, in one shape or another, almost universal. . . . The object seems to be to make the circle incomplete . . . When the circle is made of stones placed close together, or is formed of earth, then one or more openings occur in it.' On the other hand, Mr. W. C. Borlase (*Archaeologia*, xlix, 1885, p. 183) found that in Cornwall the stone rings enclosing barrows were almost always continuous.

[5] W. Greenwell, *Brit. Barrows*, p. 8.

[6] A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 138. Mr. J. R. Mortimer (*Forty Years' Researches*, p. xxii), referring to the incomplete stone rings or trenches which are found *within* barrows, suggests that they were intended 'to mark off . . . the sacred spot in which the ceremony and interment were afterwards to be conducted, and that the break in the circle had no other significance than to serve as a place of ingress and egress'.

[7] *Ib.*, p. lxxi; W. Greenwell, *Brit. Barrows*, p. 112.

it is said, no humble family would have had the needful command of labour: but considering that in Wiltshire, where there are more long barrows than in all the rest of Britain, round barrows are thirty-four times as numerous,[1] it is difficult to accept this opinion. Many of the round barrows are small; and it is surely probable that the poorer clansmen sometimes voluntarily gave their services to provide respected members of their own class with a distinctive monument. Barrows and cairns, however, are not the only sepulchres in which interments of the Bronze Age have been discovered. A cave at Gop, near Rhyl, which had been used as a dwelling, contained a sepulchral vault;[2] and Rains Cave in the same county was used alternately as a dwelling and a cemetery.[3] Many graves also exist over which no mound was erected.[4] Thus on Handley Down in Dorsetshire no less than fifty-two interments of cremated bones were found in holes dug out of the chalk on the western side of a barrow. They were evidently later than the funeral in the barrow itself, and were doubtless the remains of the descendants or connexions of the chief who had been buried there.[5] In Scotland numerous cemeteries, most of which were on knolls or sandhills, were unmarked by any external sign;[6] and at Elton, near Beverley, in the East Riding, more than seventy bodies were interred without a barrow.[7] It has been supposed that such graves belonged to the poor and lowly; and doubtless where they occur in large numbers and are almost or entirely devoid of accompanying relics the assumption is justified.[8] In certain cases also, where one or two large barrows are associated with groups of tiny mounds, the latter were devoted to the

[1] *Archaeologia*, xlii, 1869, pp. 169–71.

[2] *Archaeol. Journal*, lviii, 1901, pp. 328–31.

[3] *Journ. Derby. Archaeol. and Nat. Hist. Soc.*, xi, 1889, pp. 39–44; xiv, 1892, pp. 244–7, 250; xv, 1893, pp. 161–2.

[4] In certain cases, however, the mound may have been so worn down by denudation as to escape notice.

[5] A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 148.

[6] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 28, 37–8; *Proc. Soc. Ant. Scot.*, xx, 1886, pp. 114, 240–1; xxix, 1895, pp. 46–8; xxxv, 1901, pp. 258–66; xxxix, 1905, pp. 189, 528–32.

[7] *Anthr. Rev.*, v, 1867, p. 255. [8] W. Greenwell, *Brit. Barrows*, pp. 112–3.

humbler members of the tribe. Two of the Scottish cemeteries, however, contained gold armlets, others beautifully ornamented bronze blades;[1] and three of the only four graves in which Pitt-Rivers found the sepulchral vessels which are known as drinking-cups lacked any memorial.[2] These may have been the graves of men of rank; and so may the simple stone cists, in which relics have been found that would seem to have belonged to persons of some wealth;[3] for while every cist that has been observed in Devonshire either is or was once covered by a mound,[4] there are many in Northumberland, as in Scotland, which were left without any monument.[5]

Perhaps the most curious of all the burial grounds of the Bronze Age is one which has been lately explored at Bleasdale in Lancashire, and which may be compared with the wooden chamber in the neolithic Wor Barrow on Cranborne Chase.[6] Here, on a moorland knoll surrounded by an amphitheatre of hills, is a circle made not of stones but of wooden logs closely planted in a trench, and containing a smaller circle, which consists of a bank with a ditch on its inner side. Within this ditch is a low mound, concealing another circle of logs, in the centre of which were found two sepulchral urns. The ditch is floored with poles, which may perhaps have been trodden by worshippers who walked in

[1] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 38, 48–50, 63–4.

[2] *Excavations in Cranborne Chase*, iv, 22–3 (preface). 'There is no knowing,' says Pitt-Rivers, 'how many of these graves without mounds or ditches may exist in the soil; as they show no mark on the surface, they can only be found accidentally'.

[3] W. Greenwell, *Brit. Barrows*, p. 113.

[4] *Trans. Devon. Association*, xxxiv, 1902, p. 111.

[5] *Brit. Barrows*, p. 402. For other instances of moundless graves see *Anthr. Rev.*, iii, 1865 (*Journ. Anthr. Soc.*, p. lxvii); *Journ. Brit. Archaeol. Association*, xlv, 1889, pp. 112–22; *Proc. Soc. Ant.*, 2nd ser., xvi, 1895–7, p. 335; W. C. Borlase, *Dolmens of Ireland*, iii, 1013; *Archaeol. Cambr.*, 6th ser., ii, 1902, p. 28; *Wilts Archaeol. and Nat. Hist. Mag.*, xxxiii, 1904, pp. 410–1; *Archaeol. Aeliana*, 3rd ser., ii, 1906, p. 132; and *Vict. Hist. of . . . Lancs*, i, 245. Mr. J. R. Mortimer, *Forty Years' Researches*, p. lxxii, suggests that 'the great number of small cairns which even yet exist on the uncultivated moors of Yorkshire' may have 'belonged to the masses'.

[6] See pp. 105–6, *supra*.

ceremonial procession around the grave; for the bottom of a ditch surrounding a barrow near Blandford, which was opened towards the end of the last century, was worn into a smooth track by human feet.[1]

Hardly less remarkable is a circle near Port Erin in the Isle of Man, formed of eighteen cists, in six separate sets, each composed of three arranged in the shape of the letter T, two being placed end to end along the circumference, while the third extended outwards at right angles.[2]

In Britain, as in other countries, cenotaphs were erected in honour of the dead whose remains could not be found. Barrows have been opened within which, after the most careful scrutiny, not the faintest indication could be detected of any burial, although in one there was an empty urn and in another a small stone pavement, enclosed by a miniature stone circle and resting upon burnt earth, which suggested that an ideal cremation had been performed.[3] It seems possible that Silbury Hill was a monument of this sort. This stupendous earthwork, which commands the Bath road, six miles west of Marlborough, is one hundred and thirty-five feet high and covers about five acres. The cost of its erection at the present day would be not less than twenty thousand pounds.[4] In 1777 a shaft was sunk from the top to the bottom; and in 1849 a tunnel was driven from the side to the centre. No trace of burial was found:[5]

[1] *Trans. Lancs. and Cheshire Ant. Soc.*, xviii, 1900 (1901), pp. 114–24.

[2] These cists are assigned by the excavators, Dr. W. A. Herdman and Mr. P. M. C. Kermode (*Proc. Liverpool Biol. Soc.*, viii, 1894, pp. 159–72), to the Neolithic Age; but the evidence which they adduce is purely negative. Perhaps the people who built them only had stone tools; but the fact that the interments were in cists and accompanied by cinerary urns proves that they were made after bronze had come into use.

[3] A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 4; *Archaeologia*, lii, 1890, pp. 24–7, 41, 60; W. C. Borlase, *Dolmens of Ireland*, ii, 634; Général Pothier, *Les tumulus du plateau de Ger*, 1900, pp. 28–9. See also *Journ. Anthr. Inst.*, xv, 1886, pp. 95–7.

[4] J. H. F. Brabner, *Gazetteer of England and Wales*, vi, 31.

[5] W. Greenwell, *Brit. Barrows*, p. 2, n. 2. Mr. J. R. Mortimer (*Forty Years' Researches*, p. xviii, n. ||) remarks that 'it could hardly be expected that these two small openings would be more likely to find the primary grave ... than two rat holes would be likely to come upon the ashes of a mouse placed under a mound ten feet in diameter'.

but even primary interments were not always made at the centre of a barrow; and the labour of proving, if it could be proved, that Silbury Hill was not erected over a grave would be out of all proportion with the result. At all events its purpose was connected with sepulchral usage. Recent excavations in the meadow west and north of the hill are believed to have shown that it was originally surrounded by a trench, which was filled with water; and a local antiquary has suggested that the mound was an artificial stronghold![1] But what clan would have undertaken this herculean labour in a district where every hill was suitable for defence, and of what use would the mound have been for such a purpose?

Chronology of the barrows.

The chronology of the barrows is somewhat perplexing. There is hardly a single absolutely certain instance in which a socketed celt, a sword, or a socketed spear-head has been found in a barrow, associated with an interment;[2] and most antiquaries infer that the round barrows generally belong to the earliest period of the Bronze Age.[3] It would follow that during not less than four or five centuries the

[1] *Wilts Archaeol. and Nat. Hist. Mag.*, xxiii, 1887, pp. 245–52. At the foot of Garrow by Hill, on the Yorkshire Wolds, there is another gigantic mound, 50 feet high and 250 feet in diameter, which has not been opened (J. R. Mortimer, *Forty Years' Researches*, p. xx).

[2] J. Evans, *Anc. Bronze Implements*, pp. 134, 273–4, 277, 342; W. Greenwell, *Brit. Barrows*, pp. 44, 48–9; *Archaeologia*, xliii, 1871, pp. 442–6. See also *Journ. Brit. Archaeol. Association*, x, 1855, p. 8. Dr. Thurnam (*Archaeologia*, xliii, 447 and note b) quotes one instance of the discovery in a round barrow of a socketed celt, which, notwithstanding the doubts expressed by Sir John Evans (*Anc. Bronze Implements*, p. 134), appears to have been contemporaneous with the interment; and another is mentioned in *Anc. Bronze Implements*, p. 114. On the other hand, Pitt-Rivers, referring to the spear-head mentioned in *Archaeologia*, xliii, 447, says, 'I am informed by Mr. William Cunnington . . . that . . . it was found by his grandfather . . . immediately under the turf near Stonehenge, and not in a barrow'. The reference given by Pitt-Rivers (*Excavations in Cranborne Chase*, iv, 20) to *Archaeologia* is incorrect.

[3] See J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 169–70. Canon Greenwell (*Brit. Barrows*, pp. 48–50) asserts that knife-daggers and flat axes—the only bronze implements found in Yorkshire barrows—never accompany swords and spear-heads in hoards, and argues that this proves the early date of round barrows in general. If the assertion were true, the fact would prove the early date of those Yorkshire barrows in which daggers and flat axes were found; but the question is whether many other barrows

practice of raising mounds over graves was discontinued, and one could only wonder how it came to be revived at the beginning of the Iron Age. It has indeed been argued that the absence of swords is no proof that they were not used when barrows were being erected, but merely shows that it was not customary to bury costly weapons which were not habitually worn.[1] It seems difficult, however, to explain why a distinction should have been drawn between swords and socketed celts, on the one hand, and knives, daggers, and awls, which were often buried, on the other.[2] Some may accept the suggestion that in the later period of the Bronze Age, when cremation had presumably become general, the practice of burying weapons or ornaments had ceased;[3] but in the Early Iron Age it was not uncommon.[4] It would seem, moreover, that in one or two instances socketed weapons were laid with the dead;[5] and Dr. Arthur

do not belong to later periods of the Bronze Age. Moreover, flat axes have been found twice in hoards,—one with palstaves (J. Evans, *Anc. Bronze Implements*, p. 464), another with swords (*ib.*, p. 466).

[1] *Archaeologia*, xliii, 1871, p. 463.

[2] The Hon. John Abercromby (*Journ. Anthr. Inst.*, xxxv, 1905, p. 262) affirms that 'the few bronze swords, spear-heads, etc., of the Bronze Age and Hallstatt Period, that first filtered in driblets into this country, and were then reproduced with variations by native smiths, were too precious to be laid by for ever in a grave, even at the end of the Bronze Age in Britain'. Is not this begging the question? If small bronze weapons were 'laid by for ever' in graves in the earlier period, when bronze was scarce, why should not large ones have been laid by when it was common? And if gold ornaments were not too costly to be sacrificed, why should bronze swords have been deemed so precious?

[3] *Anc. Bronze Implements*, pp. 473–4.

[4] See p. 288, *infra*, and Addenda.

[5] See p. 181, n. 2, *supra*; *Archaeologia*, xliii, 1871, pp. 447–8; and *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 93. Sir John Evans (*Anc. Bronze Implements*, p. 310), referring to the tanged weapons of Arreton Down type and to certain blades found in the Wiltshire barrows, which (see pp. 145, 147, *supra*) he thinks 'may have been the heads of spears rather than the blades of daggers', remarks that 'at the period to which they belong the art of making cores must have been known, as the ferrule found at Arreton Down will testify'. This is significant; but on p. 473 he refers the 'tanged spear-heads or daggers' to the second period of the Bronze Age,—earlier than that of palstaves and socketed celts.

Mr. C. H. Read (*Guide to the Ant. of the Bronze Age*, p. 24) observes that cinerary urns 'possibly represent the period during which swords and spear-heads of bronze were manufactured . . . by our population'. It is true that

Evans, pointing out that an amber necklace, found in one of the barrows near Stonehenge, is identical in form and arrangement with the amber necklaces of Hallstatt, boldly affirms that the disk-shaped barrows of Wiltshire belong to the end of the Bronze Age.[1] Be this, however, as it may, it is morally certain that some of the glass beads which abounded in the graves of South Wiltshire were contemporary with socketed weapons ; and a competent antiquary, who has diligently examined their associations, concludes that they belonged to the eighth and seventh centuries before the Christian era.[2] Moreover, an earthenware vessel of the kind which are called incense-cups, found in a barrow at Bulford, near Amesbury, was ornamented with concentric circles ;[3] and, as we have seen,[4] this form of decoration, which is common on the covering stones of cists in Scotland and in the north of England,[5] is also characteristic of socketed celts and unknown on implements of earlier date. The number of celts which have been found in barrows is so small that it would be premature to lay stress upon the fact that only one belonged to the socketed type ;[6] and there

certain urns of overhanging rim type were contemporary with socketed weapons (*Journ. Anthr. Inst.*, xxxv, 1905, pp. 262–4) ; but the oldest urns were much earlier. Cf. *Proc. Soc. Ant.*, 2nd ser., xviii, 1899–1901, pp. 251–3.

[1] *Archaeol. Rev.*, ii, 1889, p. 323. Cf. *Folk-Lore*, vi, 1895, pp. 15–7. Dr. Evans remarks in this article that ' the characteristic form presented by a spiral ring of bronze found in one urn leads one indeed to believe that these flat disk-barrows of Standlake [in Oxfordshire] belong to a time when iron was coming into use '.

[2] *Journ. Anthr. Inst.*, xxxv, 1905, pp. 256–64.

[3] *Archaeologia*, xliii, 1871, pp. 366, 369.

[4] See p. 149, *supra*.

[5] See pp. 205–6, *infra*.

[6] Flat bronze celts were found by Canon Greenwell in two only of the multitudes of barrows which he has explored not only in the northern counties but also in Wiltshire and Berkshire (*Archaeologia*, lii, 1890, p. 3) ; while Mr. J. R. Mortimer (*Forty Years' Researches*, p. xlvi) never found one with any of the 893 interments which he examined on the Yorkshire Wolds. The canon opened four Late Celtic barrows in the parish of Cowlam, of which he says (*Brit. Barrows*, p. 212), ' Had the bodies occurred without the necklace, fibula, or armlets, I should not have hesitated the least about classing these four barrows with the other barrows in the immediate vicinity, which were of the time of stone, or more probably of bronze.' Is it unreasonable to conclude that a few other barrows which contain no relics of the late Bronze Age may nevertheless belong to that time ?

may have been some reason, of which we are ignorant, for the absence of spear-heads and swords. In Gaul, at all events, relics belonging to every phase of the bronze culture have been exhumed from burial mounds.[1]

Cremation and inhumation.

In the Bronze Age, as in the period of the long barrows, both cremation and inhumation were practised in Britain. In Cleveland and on the coast between Scarborough and Whitby cremation was almost invariable : [2] in Northumberland nearly twice as common as inhumation.[3] In Derbyshire,[4] on the other hand, inhumation interments are slightly commoner than those by cremation ; and on the Yorkshire Wolds more than three times as numerous.[5] In Wiltshire and Dorsetshire inhumation is as rare as cremation on the Wolds ; and in Gloucestershire, Devonshire, Cornwall, Merioneth, Carnarvon, and Denbigh cremation is practically universal.[6] In Devonshire interments by inhumation have been found, but never in barrows.[7] In Scotland the numbers are about equal.[8]

Archaeologists generally hold that cremation was not practised in the Bronze Age until a comparatively late date, —probably not before 1000 B.C. ; and this view seems at

[1] *L'Anthr.*, xvii, 1906, pp. 321–42. Cf. W. Greenwell, *Brit. Barrows*, p. 44, n. 2, and *Rev. de l'École d'anthr.*, xv, 1905, pp. 213, 215.

[2] W. Greenwell, *Brit. Barrows*, pp. 21, 333.

[3] *Ib.*, p. 21, n. 1.

[4] *Ib.* See p. 185, n. 3, *infra*.

[5] W. Greenwell, *Brit. Barrows*, pp. 19–20 ; J. R. Mortimer, *Forty Years' Researches*, p. xxxiv. Canon Greenwell remarks, however (pp. 28–30), that charcoal was almost always found in contact with unburnt bodies ; and he was doubtful whether it was merely the ashes of the fire at which the funeral feast had been cooked, or might be regarded as a sign that the corpses had been passed through fire, just as in baptism aspersion was substituted for immersion. But this would of course imply that cremation on the Wolds was earlier than inhumation. Cf. J. R. Mortimer, *Forty Years' Researches*, p. lxxvii.

[6] W. Greenwell, *Brit. Barrows*, pp. 21 n. 1, 445 ; *Archaeologia*, xliii, 1871, p. 310 ; xliv, 1873, p. 426 ; lii, 1890, pp. 37–8, 43 ; *Report of . . . the Brit. Association*, 1888 (1889), p. 315 ; *Journ. Anthr. Inst.*, xxxii, 1902, p. 386. In regard to the few interments by inhumation that have been found in Cornwall see *Vict. Hist. of . . . Cornwall*, i, 362–3, 366.

[7] *Trans. Devon. Association*, xxxiv, 1902, p. 119.

[8] *Archaeol. Scotica*, ii, 1822, pp. 76–102 ; iii, 1831, pp. 40–50 ; *Nature*, Jan. 13, 1898, p. 236.

first sight to be supported by the facts that it was unknown in Scandinavia in the earlier period;[1] that cinerary urns were not the earliest of the sepulchral vessels; and that drinking-cups, which were in use before any of the others, although they continued to be used after cinerary urns had been introduced,[2] are generally found with unburnt skeletons, and have never been found with the cremation interments in Cleveland.[3] On the other hand, in Brittany in the centuries which immediately followed the introduction of metallurgy cremation was almost invariable;[4] burnt bones, as we shall presently see, were often buried without urns; and since cremation was not uncommon in the Neolithic Age, the custom probably persisted into the Bronze Age independently of its introduction by immigrants who possessed weapons of bronze. Indeed, unless cremation existed from the very beginning of the Round Barrow period, it seems impossible to account for the fact that in the sepulchres of certain districts not a single instance of inhumation has ever been observed. Before the inhabitants of Bute emerged from their Stone Age they practised both cremation and inhumation; and there is no evidence that the latter was earlier than the former.[5] Not infrequently both in Scotland and

[1] O. Montelius, *Sur la chronologie de l'âge du bronze*, 1885, p. 3.

[2] Mr. Abercromby in *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 364, denies that any drinking-cups were contemporary with cinerary urns; but in *Journ. Anthr. Inst.*, xxxii, 1902, p. 385, he affirms that some were.

[3] *Archaeologia*, xliii, 1871, p. 390; A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 6; *Journ. Anthr. Inst.*, xxxii, 1902, pp. 375, 381; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 364. Five more drinking-cups have lately been found with burnt bones in two cists in Dilston Park, Northumberland (*Archaeol. Aeliana*, 3rd ser., ii, 1906, pp. 142–6, 148).

Mr. John Ward (*Vict. Hist. of . . . Derby*, i, 177–8) shows from an examination of the sepulchral pottery of Derbyshire (cf. pp. 191–6, *infra*) that in those districts in which interments of both kinds are found cremation was, generally speaking, later than inhumation. This conclusion is supported by the fact that in Wiltshire, where cremation on the whole greatly predominates, it occurs only about as often as inhumation in bowl and bell barrows (*Archaeologia*, xliii, 1871, p. 293).

[4] *L'Anthr.*, xvii, 1906, p. 326.

[5] *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 78. Canon Greenwell (*Ib.*, vi, 1867, p. 343, n. 2), speaking of a cairn near Crinan in Argyllshire, which he explored, remarks that 'in this part of Scotland at all events the earliest interments in the large megalithic chambers are of burnt bodies'.

in many parts of England skeletons and burnt bones reposed under the same cairn, in the same barrow, within the same stone circle, even in the same cist; and in some cases they were buried at the same time.[1] A cairn has been opened at Greenhill in Fifeshire, in which four different modes of sepulture had been practised: cremated remains had been laid in the earth, and beneath a stone slab; an unburnt body had been buried in a cist, and another lowered into a pit.[2] In some barrows one unburnt body has been found accompanied by several deposits of burnt bones; and it has been inferred that, even after cremation had become general, the bodies of chieftains were very rarely burned, although those of their wives and retainers were.[3] It is possible that this distinction may sometimes and in some places have been maintained; but obviously it was very unusual. For otherwise we should be compelled to suppose that in Cleveland and in those western districts in which cremation was universal no chiefs were buried in barrows at all, although it is universally admitted that it was in their honour that barrows were erected. And if the presence of an unburnt body surrounded by urns is a sign that wife and dependents were sacrificed in honour of the dead chief, what conclusion is to be based upon the association of nine skeletons with a single cremated interment?[4] On the

[1] *Proc. Soc. Ant. Scot.*, vii, 1870, pp. 268–70; xx, 1886, p. 252; xxix, 1895, pp. 191–4; *Archaeologia*, xxx, 1844, p. 335; xliii, 1871, pp. 450–1; lii, 1890, pp. 25, 64; J. B. Davis and J. Thurnam, *Crania Britannica*, ii, pl. 1, p. 1; W. Greenwell, *Brit. Barrows*, pp. 20–1; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 90; A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 2, 29; iii, 17; *Journ. Brit. Archaeol. Association*, N. S., vi, 1900, p. 10. Secondary interments by inhumation sometimes succeeded primary interments by cremation (*Excavations in Cranborne Chase*, iv, 173; *Proc. Dorset Nat. Hist. and Ant. Field Club*, xvi, 1895, p. 50).

[2] *Proc. Soc. Ant. Scot.*, xxxvi, 1902, pp. 650–1.

[3] *Archaeologia*, xliii, 1871, pp. 312, 450–1; *Journ. Brit. Archaeol. Association*, N. S., vi, 1900, p. 10; *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 16. The two latter authorities only suggest that in cases like that mentioned in the text the unburnt body was that of the chief.

[4] *Archaeologia*, xv, 1806, pp. 340–1. The nine skeletons may have belonged to a secondary interment, but Cunnington inferred from the careless manner in which they had been buried that they were 'slaves or dependents of the great personage below'. Mr. J. R. Mortimer (*Forty Years' Researches*,

Yorkshire Wolds the question as to which method should be adopted had nothing to do either with rank or sex or age ; [1] and one may reasonably suppose that it was often settled simply by individual preference. Moreover, the expense of cremation was far greater than that of inhumation ; [2] and it is not improbable that long after the former had become prevalent among the wealthy the poor were generally obliged to content themselves with the latter.

Inhumation was accompanied by many varieties of usage. Most of the Wiltshire barrows contained only one interment, though in a few—evidently family tombs—there were two or even more.[3] Those of the Yorkshire Wolds, on the other hand, generally contained several, two or three having sometimes been laid in one grave ; and where one only was found the barrow was of the conical kind which is common in Wiltshire.[4] In the Scottish cists also, single burial is the rule, though occasionally husband and wife were interred together, and sometimes a father with his child.[5] The same variety has been noticed in connexion with cremation : a group of eight barrows in Lincolnshire contained one urn each, while inside a barrow in Dorsetshire was found a cairn which covered nearly fifty interments.[6] When a mound was erected, the primary interment was generally made in the centre.[7] The body was almost always laid in the crouched position. In Wiltshire this custom was absolutely, and on the Yorkshire Wolds almost, universal : the same posture indeed was commonly adopted

p. xxxii), remarking that 'in several instances where the body of the chief burial was reduced to ashes the attendants [?] were inhumed', argues that in some cases cremation, in others inhumation, was considered the more honourable mode of sepulture. Perhaps.

[1] W. Greenwell, *Brit. Barrows*, pp. 20–1. Cf. J. R. Mortimer, *Forty Years' Researches*, pp. xxxii, 60, 318.

[2] *Proc. Soc. Ant.*, 2nd ser., xvi, 1895–7, p. 304.

[3] *Archaeologia*, xliii, 1871, pp. 313–4.

[4] *Ib.*, lii, 58–9 ; J. R. Mortimer, *Forty Years' Researches*, p. xxxviii. Cf. *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 90.

[5] *Nature*, Jan. 13, 1898, p. 237 ; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 17, 74–5.

[6] *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 48.

[7] *Archaeologia*, xliii, 1871, p. 309 ; W. Greenwell, *Brit. Barrows*, p. 12.

there even when the body was cremated.[1] In Dorsetshire, on the other hand, the extended position appears to have been occasionally met with.[2] When secondary interments have been found, they were generally on the surface of the barrow or just outside it, and were covered with fresh material.[3] There is a barrow on Lord's Down in Dorsetshire, formed of alternate layers of mould and chalk, which represent no less than five successive interments, each of which was covered by a new tumulus.[4] Almost invariably on the Wolds secondary interments were made on the southern or eastern side of the mound, doubtless in order that the dead might face the sun; and this fancy underlies the prejudice, which still exists, against burying on the northern side of a churchyard.[5] Probably the same purpose is discernible in the orientation of the skeletons. Generally in Wiltshire they were laid with their heads towards the north so that they looked southwards;[6] and although in Yorkshire and elsewhere the head has been found directed to almost every

[1] W. Greenwell, *Brit. Barrows*, pp. 22–3; *Archaeologia*, xliii, 1871, pp. 315–8. Cf. *Journ. Brit. Archaeol. Association*, N. S., vi, 1900, pp. 8–9.

[2] C. Warne, *Celtic Tumuli of Dorset*, 1866 ('Tumuli opened at Various Periods', pp. 10–1, 72, 76). The instance mentioned on p. 72 is, in my opinion, doubtful, and no certain pre-Roman instances are recorded in the sections entitled 'Personal Researches' and 'Communications from Personal Friends'. Prof. Ridgeway (*Early Age of Greece*, i, 1901, p. 502), referring to Greenwell's *Brit. Barrows*, p. 22, states that 'in Dorsetshire . . . the extended position seems to be the prevalent one', a remark which illustrates the danger of relying on second-hand evidence.

In a few cases in Derbyshire and elsewhere in which the body has been found sitting the posture was perhaps due to some accident in filling up the grave (*Archaeologia*, xliii, 1871, pp. 318–20; *Journ. Brit. Archaeol. Association*, xxxviii, 1882, pp. 109–10). Two skeletons, however, were found sitting, back to back, in a barrow in Denbighshire (*Crania Britannica*, ii, pl. 23, p. 1).

[3] W. Greenwell, *Brit. Barrows*, p. 12. In a round barrow on the south of the road between Rochester and Gravesend, and about midway between Chalk Church and the Crown Inn, five skeletons were found in the trench near the bottom (*Archaeol. Cant.*, xxiv, 1900, pp. 86–90).

[4] C. Warne, *Celtic Tumuli of Dorset*, pp. 46–9. Cf. pp. 36–7. In Derbyshire 'secondary interments are found in any position, central or otherwise' (*Vict. Hist. of . . . Derby*, i, 176).

[5] W. Greenwell, *Brit. Barrows*, pp. 12–3.

[6] *Archaeologia*, xliii, 1871, pp. 321–3. Cunnington, however, remarks (*ib.* xv, 1806, p. 343) that 'on the top of barrows we find the skeletons in every direction'.

point of the compass, yet, as a general rule, it was so laid as to face the sun: thus when it pointed westward or to the north or south of west, the body was commonly laid upon its right side; when to the east or the adjacent points, upon the left.[1]

It is probable that bodies were generally interred either in the clothes which had been worn in life or in a winding-sheet; for at Kelleythorpe in the East Riding a linen cloth was underlying a skeleton: bones have been found in divers parts of Britain with fragments of woollen or leathern fabrics clinging to them; and buttons in their natural positions on the breast-bone.[2] In one instance Hoare found a skeleton in a disk-barrow near Amesbury, lying on the ground, without cist, grave, or coffin, beneath a heap of stones, and quaintly suggested that the dead man had suffered the doom of Achan.[3] Occasionally, however, corpses were not buried entire; but, as in the Stone Age, the bones were disjointed and interred separately.[4]

When the dead were cremated the customs which governed the disposal of primary and secondary interments remained the same: indeed in the Lord's Down barrow the latter comprised both skeletons and burnt bones. The mound was sometimes raised over the funeral pile; but more commonly the ashes were brought to the place of interment.[5]

[1] W. Greenwell, *Brit. Barrows*, pp. 25–6; *Archaeologia*, lii, 1890, pp. 25, 38, 64; *Wilts Archaeol. and Nat. Hist. Mag.*, xxxiii, 1904, pp. 412–3; J. R. Mortimer, *Forty Years' Researches*, p. xxxvii; *Vict. Hist. of . . . Derby*, i, 173. All the kistvaens of Dartmoor lie at one end between north and west, at the other between south and east of the corrected compass (*Trans. Devon. Association*, xxxiii, 1901, pp. 121–2; xxxiv, 1902, p. 164); and the cairns near the Land's End have 'an aspect ranging from south-east to south-west' (*Archaeologia*, xlix, 1885, p. 182). Cf. *Rev. arch.*, 4e sér., v, 1905, p. 307.

[2] *Archaeologia*, xxxiv, 1862, p. 255; xliii, 1871, pp. 314–5; W. Greenwell, *Brit. Barrows*, pp. 31–2; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 74–5; *Proc. Soc. Ant. Scot.*, xxxix, 1905, p. 552; J. R. Mortimer, *Forty Years' Researches*, p. xli.

[3] *Anc. Wilts*, i, 124.

[4] J. R. Mortimer, *Forty Years' Researches*, pp. xxxiii, 15–6, 63, 66, 77; A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 66.

[5] *Ib.*, ii, 1–2; E. T. Stevens, *Flint Chips*, 1870, p. 410; *Archaeologia*, xliii, 1871, pp. 324-5; W. Greenwell, *Brit. Barrows*, p. 14; *Trans. Devon. Association*, xxxiv, 1902, p. 108.

Although they were often enclosed in urns, this custom was by no means universal. In the disk-shaped barrows of Wiltshire, in which cremation was almost invariable, urns were very rare: the remains had generally been wrapped in a skin or a linen cloth.[1] In Dorsetshire, on the other hand, except in the north-eastern corner, the customs of which closely resembled those of Wiltshire, urns were used three times out of four;[2] while some barrows have been opened which contained both urns enclosing burnt bones and burnt bones without any urn.[3] Occasionally an urn has been found which, instead of containing the bones, was surrounded by them.[4] Sometimes the urn was placed upright; but much oftener, at least in Wiltshire, it was inverted;[5] and occasionally one urn was inverted as a cover over another.[6] In more than one instance a custom described by Homer had found its way to Britain: the urn which contained the ashes of Patroclus was wrapped in a cloth;[7] and in a barrow in Cambridgeshire, as well as in six of those which Hoare opened, the same ritual was observed.[8] In several Scottish graves tiny urns, containing the remains of infants, were placed inside vessels of ordinary size;[9] and it is remarkable that in a few instances empty cinerary urns have been found in association with unburnt bodies.[10] Why urns were some-

[1] *Archaeologia*, xliii, 1871, pp. 294, 310, 325–6.

[2] *Ib.*, p. 326. Cf. W. Greenwell, *Brit. Barrows*, p. 445; *Nature*, Jan. 13, 1898, p. 236; *Journ. Brit. Archaeol. Association*, N. S., vi, 1900, pp. 8–9; and *Trans. Devon. Association*, xxxiv, 1902, pp. 108–9.

[3] *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., i, 1901, pp. 295–9. Cf. *Proc. Soc. Ant. Scot.*, xxxix, 1905, p. 547.

[4] *Ib.*, xxxvi, 1902, p. 644.

[5] *Archaeologia*, xliii, 1871, p. 326. On the other hand the urns which Canon Greenwell found in Yorkshire were usually placed upright (*Brit. Barrows*, p. 14).

[6] *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 49.

[7] *Iliad*, xxiii, 254. Cf. xxiv, 795–6.

[8] *Archaeologia*, xliii, 1871, p. 326.

[9] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 43–7. An urn only $\frac{7}{8}$ inch high, which of course could not have been used for containing ashes, has been found in a cairn of the Bronze Age in Fifeshire (*Proc. Soc. Ant. Scot.*, xxxvi, 1902, p. 641).

[10] J. Anderson, *op. cit.*, pp. 51–2, 74–5; W. Greenwell, *Brit. Barrows*, pp. 62, 72–3, 139, 277, 291, 297.

times broken into fragments before they were placed in the grave it would be vain to guess.[1]

Sepulchral pottery.

The urns and drinking-cups which have been so often mentioned were not the only kinds of sepulchral pottery. Besides them were bowls which have been called food-vessels and incense-cups. The custom of placing vessels in graves was not, however, universal: both in Wiltshire and in Yorkshire the majority of interments were without them.[2] All four kinds are worth studying, not only as illustrative of funeral customs, but also because they throw light upon the origin of the round-headed invaders and upon the intercourse which subsisted in the Bronze Age between Britain and other lands.[3]

Like the domestic pottery of the same period and of the modern inhabitants of the Hebrides, they were generally made by women: the markings, produced by the impression of finger-tips and finger-nails, with which they were often ornamented, were the work of small hands.[4] The potter's wheel, which, more than two thousand years before the Christian era, was used in Hissarlik, the town on whose site Troy was afterwards built, was as yet hardly known in Britain,[5] and the British pottery of the Bronze Age was baked at open fires.[6]

Although they all comprise numerous varieties, the four groups are so distinct that an observant eye, after an hour spent in a well-stored museum, or even after studying the illustrations alone, would be able, in almost every instance, to assign this or that specimen to its proper class. Drinking-cups are generally about seven or eight inches high, and fall under three principal types. That which is apparently the earliest and, in Southern Britain, by far the commonest, is

[1] A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 29. Canon Greenwell (*Archaeologia*, lii, 1890, pp. 63–4) has described in an interesting paragraph 'the infinite variety, within certain limits, which is found in connection with the burials of the Bronze Age'.

[2] *Archaeologia*, xliii, 1871, p. 331; *Brit. Barrows*, p. 74; J. R. Mortimer, *Forty Years' Researches*, pp. lv, lix.

[3] See pp. 442–3, *infra*.

[4] Sir A. Mitchell, *The Past in the Present*, 1880, p. 28; *Archaeologia*, xliii, 1871, p. 334.

[5] See p. 159, n. 1, *supra*.

[6] W. Greenwell, *Brit. Barrows*, p. 63. See, however, *Archaeologia*, xlix, 1885, p. 184.

globular in its lower part, and rises from the waist into a high brim with straight sides. In cups of the second class an oval body passes into a brim which curves outward. The third kind, almost all the examples of which belong to Northumberland and Scotland, and which, from its accompaniments, would seem to have been the latest, is also somewhat oval in the lower part, and has a very low and more or less straight brim. A few high-brimmed cups have handles, and are not unlike modern tankards. Drinking-cups in general are the handsomest and the most skilfully baked of all the British sepulchral vessels; but in course of time their forms gradually deteriorated, for each generation had inferior models to copy.[1]

FIG. 32. ½

[1] *Archaeologia*, xliii, 1871, pp. 391, 396; *Journ. Anthr. Inst.*, xxxii, 1902, pp. 373–97; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 323–410; xxxix, 1905, pp. 326–44; J. R. Mortimer, *Forty Years' Researches*, pp. lxv–lxvii.

Mr. J. P. Gibson, in a paper on a recent find of drinking-cups in Dilston Park, Northumberland (*Archaeol. Aeliana*, 3rd ser., ii, 1906, pp. 126-49) says (pp. 146–7), 'There appears nothing in the Dilston Park discovery to confirm this suggested [chronological] arrangement. Mr. Abercromby tells me that evidence received since the paper [in *Proc. Soc. Ant.*, xxxviii, 1904] was published has convinced him that "the whole question requires a fresh investigation" . . . The Dilston Park find . . . furnishes two instances in which in the same cist . . . vessels are found varying widely both in form and decoration. It also proves the great difficulty of attempting to fix any relative dates of Bronze Age beakers by a comparison either of their shape or ornament.'

Food-vessels, which range between three and eight inches in height, are very diversified in form, and, unlike drinking-cups, vary greatly in quality. They commonly resemble a large cup or bowl with a narrow bottom, and sometimes they are slightly contracted towards the mouth. Many of them have knobs round the neck, which are sometimes perforated, so that they might have been suspended by a cord; and those which have no perforations are doubtless mere survivals.[1]

FIG. 33. ½

FIG. 34. ½

Cinerary urns, which were certainly introduced later than food-vessels or drinking-cups, are as a rule much larger, although one or two have been found which were as small as the smallest incense-cup. Many of them are more than two feet high. The

Mr. Abercromby, however, in his third paper (*ib.*, xxxix, 1905), adheres to his chronological arrangement. See also *ib.*, xl, 1906, pp. 32–3, 371.

Handles are occasionally found not only on drinking-cups, but also on the other kinds of sepulchral pottery.

[1] *Archaeologia*, xliii, 1871, pp. 378–83; W. Greenwell, *Brit. Barrows*, pp. 83–93; J. R. Mortimer, *Forty Years' Researches*, pp. lxii–lxv.

commonest form resembles a double truncated cone with the base in the centre, the upper being much the smaller of the two; but some urns are cylindrical, barrel-shaped, or even like flower-pots; while a few, which are peculiar to central Dorsetshire, are nearly globular, and, except for the scantiness of their ornament, not unlike certain drinking-cups.[1]

Incense-cups are the smallest, perhaps the latest of all sepulchral vessels, and the most various in form. Some contract from the centre towards the top and the bottom; others expand, others again contract from the bottom to the top. A few resemble saucers in shape; and many are perforated with oval, lozenge-shaped, or vertical holes, one example having as many as twenty-seven.[2]

FIG. 35. ½

Drinking-cups have been found on the Continent not only in Germany, Gelderland, and Denmark, from which countries, it should seem, they were introduced into Britain, but also in Spain, Portugal, Brittany, and the Channel Islands.[3] On the Continent they all belong to the Neolithic

[1] *Archaeologia*, xliii, 1871, pp. 343–57; xlix, 1885, p. 195; W. Greenwell, *Brit. Barrows*, pp. 66–74; J. R. Mortimer, *Forty Years' Researches*, pp. lviii–lix; *Proc. Soc. Ant. Scot.*, xxxix, 1905, pp. 415–6.

[2] W. Greenwell, *Brit. Barrows*, pp. 74–83; *Archaeol. Journal*, xxiv, 1867, pp. 22–5; *Archaeologia*, xliii, 1871, pp. 357–77; J. R. Mortimer, *Forty Years' Researches*, pp. lix–lxii.

[3] W. Greenwell, *Brit. Barrows*, p. 61; *Archaeol. Cambr.*, 6th ser., ii, 1902, p. 197. The counties in which drinking-cups have been found are Kent, Sussex, Suffolk, Norfolk, Berkshire, Buckingham, Hampshire, Dorset, Devon, Somerset, Wiltshire, Oxford, Cambridge, Lincoln, Derby, Stafford, the East Riding of Yorkshire, Durham, Northumberland, Westmorland, Cumberland, Monmouth, Anglesey, Carnarvon, Denbigh, Glamorgan, Berwick, Roxburgh, Ayr, Argyll, Stirling, Lanark, Haddington, Edinburgh, Linlithgow, Kinross, Fife, Forfar, Perth, Aberdeen, Banff, Elgin, Nairn, Inverness, Ross,

Age; and this fact alone is sufficient to show that the people who brought them into Britain had no bronze implements.[1] Moreover, although they continued in use in this country during a considerable part of the Bronze Age, they have rarely been found with bronze.[2] Only two specimens have been obtained in Ireland,[3] an additional indication of the erroneousness of the theory which identifies the earliest round-headed invaders who introduced drinking-cups into Britain with the Goidelic Celts. Like food-vessels, drinking-cups were receptacles for solid food or perhaps some kind of porridge; for remains which have been proved by analysis to be animal or vegetable have been found in both.[4]

Sutherland, and the island of Mull (*Journ. Anthr. Inst.*, xxxii, 1902, p. 386 and map facing p. 396; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 329). Their rarity in South-Eastern Britain is doubtless due largely to the destruction of barrows in a highly cultivated region; while their absence from many of the Midland counties may be ascribed partly to the same cause and partly to the fact that the population of those parts in the Bronze Age was probably small. In Cornwall vessels of a peculiar kind appear to have served the same purposes as drinking-cups and food-vessels (*Archaeologia*, xlix, 1885, pp. 186–8). From the frequency with which drinking-cups occur in the east of Scotland it may perhaps be inferred that they were introduced into that country, at least in part, by immigrants from Scandinavia or Denmark.

A gold cup, which in form resembles certain drinking-cups and is ornamented on the bottom with concentric circles, has been found with a bronze dagger at Rillaton in Cornwall (*Archaeol. Journal*, xxiv, 1867, p. 189).

[1] See pp. 408–9, 442–3, *infra*.

[2] *Journ. Anthr. Inst.*, xxxii, 1902, pp. 376–85.

[3] *Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 346–7; W. Greenwell, *Brit. Barrows*, p. 94.

[4] *Ib.*, pp. 93–4, 101; *Archaeologia*, xliii, 1871, p. 386; lii, 1890, pp. 24–5. Cf. J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 76, and E. B. Tylor, *Prim. Culture*, ii, 1903, pp. 30–43. Mr. J. R. Mortimer (*Forty Years' Researches*, pp. lxvi–lxvii) says, 'If food was essential [to the dead], so would liquid be . . . and I do not know of an instance of the remains of animal matter having ever been found in any vase of the true drinking-cup type. That they served the purpose of holding liquid, there can be little doubt'. Mr. Mortimer is more logical than the people of the Bronze Age. His argument would lead to the conclusion that only food or only drink was considered necessary for the dead according as food-vessels or drinking-cups were placed with them. Very likely liquid was sometimes poured into drinking-cups: but for obvious reasons evidence is wanting; whereas evidence exists that they sometimes held food. By 'the true drinking-cup type' Mr. Mortimer apparently means the low-brimmed type which Thurnam called γ (see W. Greenwell, *Brit. Barrows*, p. 95, fig. 82), and which, as we have seen, is confined to Northern

Food-vessels are unknown outside the British Isles, and are frequent in Ireland,[1] while hardly a single specimen has been found in any of the numerous barrows of Wiltshire or Dorsetshire.[2] Like drinking-cups, they accompany skeletons far more frequently than burnt bones;[3] and they were obviously invented after drinking-cups had been some time in use, though, as it would seem, while incense-cups were still unknown.[4]

Incense-cups, like food-vessels, are common in Ireland as well as in Britain: a few have been found in the Channel Islands; but on the Continent they do not exist. They, too, are rare in Dorsetshire and the western counties,[5] although cremation was even more prevalent there than in Wiltshire, where they are numerous, and although they have hardly ever been found except with cremated remains.[6] It is remarkable that they were often deposited inside the urn and among the burnt bones.[7] The purpose for which they were designed has been a subject of much controversy. It is difficult to believe that they were really censers, for incense was probably not obtainable in Britain, though amber, which has occasionally been used as incense, may possibly have been burned in them. The numerous holes with which so many of them are pierced, and which would have stimulated combustion, might suggest that they were intended to carry the sacred fire from which the funeral pile was to be lighted; but as many specimens contain no holes it is impossible to acquiesce in this explanation.[8]

Britain; but he is alone in calling this type 'true' to the exclusion of the others.

[1] *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 44–5.

[2] *Ib.*, p. 45; *Archaeologia*, xliii, 1871, p. 378.

[3] *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 44.

[4] *Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 362–3.

[5] *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 46.

[6] *Ib.*; *Archaeologia*, xliii, 1871, p. 358; W. Greenwell, *Brit. Barrows*, p. 80. The orthodox view is that incense-cups have never been found with interments by inhumation; but see J. R. Mortimer, *Forty Years' Researches*, pp. liv–lv, lx, 256 (fig. 724), 259.

[7] *Archaeologia*, xliii, 1871, p. 357; W. Greenwell, *Brit. Barrows*, p. 80.

[8] The suggestions that they may have been lamps or even small urns intended to receive the ashes of infants have been refuted. See on the whole question

All these vessels were ornamented with the geometrical decoration characteristic of the Bronze Age, which consists for the most part of combinations of straight lines, arranged in almost infinite variety—chevrons, zigzags, lozenges, and the herring-bone pattern—as well as dots and what have been called oblong punch marks, and, in a few cases, crosses, curves, and even circles. The patterns were impressed upon the clay while it was still wet by a pointed implement of bone or wood, by cords, and occasionally, as we have seen, by finger-nails or finger-tips. Some of them may have been imitated from basket-work or from the plaited straw or grass with which the fragile vessels were protected; for Pitt-Rivers found on his estate a fragment of fine basket-work over which clay had been plastered on both sides. As a general rule drinking-cups and food-vessels are far more profusely ornamented than the other kinds, both being in many cases covered with decoration.[1] Except perhaps in the case of drinking-cups, it is doubtful whether any useful conclusion can be drawn from the patterns; for, although the oblong punch marks are apparently peculiar to the British Isles,[2] chevrons of divers kinds have been found in nearly every country of Europe, as well as Africa, Madagascar, Siberia, Ceylon, the Philippine Islands, and North Australia.[3]

Archaeologia, xliii, 1871, pp. 374–7; W. Greenwell, *Brit. Barrows*, pp. 81–3; *Nature*, Jan. 13 (with which cf. *Archaeologia*, xliii, 374–5); E. B. Tylor, *Prim. Culture*, ii, 1903, pp. 383–5; and *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 45–6.

[1] *Archaeologia*, xliii, 1871, pp. 388–400; W. Greenwell, *Brit. Barrows*, pp. 65–7, 71, 76–7, 92–102; *Journ. Roy. Soc. Ant. Ireland*, 5th ser., iv, 1894, pp. 378–9; A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 164, 169, 216–39; J. Romilly Allen, *Celtic Art in Pagan and Christian Times*, 1904, pp. 26–39; *Proc. Soc. Ant. Scot.*, xxxix, 1905, pp. 333, 536–7. Mr. J. R. Mortimer (*Forty Years' Researches*, p. lv) says that he has found vessels of all four kinds which were quite plain.

[2] A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 235–8.

[3] *Ib.*, pp. 218–34. 'General resemblance of ornamental patterns,' says Pitt-Rivers (*ib.*, p. 216), 'is not enough to prove that they were copied from one another . . . when, however, the points of resemblance are very minute, and the distribution limited and continuous, it may be fairly argued that the different kinds of earthworks in which they are found, in the same district, were of the same period'. Mr. Andrew Lang's remarks on decorative motives (*Custom and Myth*, 1885, pp. 286–9) contain much, but not the whole truth.

Indeed one form of chevron ornament—the so-called diaper pattern—appears not only on French neolithic pottery and on urns from a chambered cairn in Orkney, but also on a palaeolithic implement from Brassempouy;[1] and the rude hand-made bowls out of which the modern Hebrideans eat their porridge are still ornamented, as they were three thousand years ago, with straight lines made with a pointed stick or with impressions of a thumb-nail.[2] On the other hand, as chevron patterns characterized the Bronze Age throughout Europe, although they occurred both earlier and later, further research may ultimately show that they had a common origin.[3] The supposition that concentric

'The conviction becomes irresistible,' he writes, 'that all these objects, in shape, in purpose, in character of decoration, are the same, because the mind and the materials of men, in their early stages of civilisation especially, are the same everywhere. You might introduce old Greek bits of clay-work, figures or vases, into a Peruvian collection, or might foist Mexican objects among the clay treasures of Hissarlik, and the wisest archaeologist would be deceived.' A socketed celt, almost identical in form with some Italian celts and ornamented with the chevron, has been found in Chili (J. Evans, *Anc. Bronze Implements*, p. 145).

[1] A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 216, 227; *Proc. Soc. Ant. Scot.*, xix, 1885, pp. 346–8; xxxix, 1905, p. 333; *L'Anthr.*, iv, 1893, p. 489; xvi, 1905, p. 2.

[2] Sir A. Mitchell, *The Past in the Present*, p. 28.

[3] A. Pitt Rivers, *Excavations in Cranborne Chase*, iv, 238–9. Mr. Coffey (*Journ. Roy. Soc. Ant. Ireland*, 5th ser., v, 1895, p. 195) thinks that 'herring-bone, chevron, and triangle ornament' may be native in Europe, and (*ib.*, vi, 1896, p. 42) that lozenge, chequer, and saltire patterns 'may be original in Britain'. He holds, however (*ib.*), that 'we have no reason to believe that geometrical forms have ever been spontaneously invented', and adds that 'they appear to have been invariably derived by a process of conventionalisation from realistic prototypes': he cites examples (*ib.*, iv, 1894, pp. 364–6) from the pottery of Cyprus to show how 'the body of the lotus flower is simplified to a triangular form, and the central sepal to an enlarged lozenge, enriched by cross-hatching and chequer patterns'; and he argues (*ib.*, v, 1895, pp. 210–1) that 'the occurrence of chequers of lozenges on Early Bronze Age remains from Scotland, in some instances identical with Cyprian forms, and the close association of lozenge, chequer, and × [saltire] patterns with the spiral in the Bronze Age ornament of Ireland is . . . strong evidence that lozenge and chequer patterns travelled northward across Europe on the path of the spiral'.

That geometrical forms were in certain cases derived from 'realistic prototypes' may be granted, but does not exclude the possibility that in others the same forms were 'spontaneously invented'. Moreover, certain geometrical forms occur, as we have seen (pp. 197–8), on neolithic and even on palaeolithic objects, to which they could not have found their way by the route and from the source to which Mr. Coffey refers. Others again are of such a kind that it

circles—a form of ornament which, as we have seen, is also characteristic of the shields of the Bronze Age—were generally symbolical of sun-worship,[1] is hardly likely to be proved. Probably in some cases they had this or some other religious meaning: but in others they may have been purely decorative; and they are to be seen on the *churingas* or sacred stones of the Aruntas of Central Australia,[2] who, it need hardly be said, do not worship the sun. More interesting are the few vessels which bear incised designs inlaid with white earth, and resemble, though in a ruder style, pottery from the lake-dwellings of Switzerland and Austria and from Hissarlik.[3] It is conceivable that this kind of decoration may have arisen independently in the different lands in which it has been observed: but the most sceptical would hardly deny the evidence of indirect connexion with the Aegean which has been furnished by the famous chalk 'drums' of Folkton Wold. Associated with the body of a child in a trench which partially surrounded the barrow were three solid drum-shaped cylinders of chalk, decorated

The 'drums' of Folkton Wold and their significance.

is difficult to conceive of any 'realistic prototype' from which they could have been derived; and there are lozenge, chequer, and saltire patterns on pottery of the Bronze Age in parts of England to which, according to Mr. Coffey, the spiral did not penetrate until the Bronze Age was at an end. Mr. Coffey (*ib.*, iv, 1894, p. 356) is himself disposed to except 'some zigzag, chevron, and triangle ornaments' from the list of geometric patterns which, as he insists, 'have been invariably derived from naturalistic forms'; but the truth is that, as Mr. Romilly Allen has shown (*Celtic Art in Pagan and Christian Times*, pp. 27–37), all the patterns of the Bronze Age, except spirals, circles and other curves, and mere dots, punch marks, and straight lines, are simply combinations of diagonal straight lines based upon the chevron.

Another theory of Mr. Coffey's (*op. cit.*, v, 1895, p. 202) is that 'as far as the ornament of primitive peoples has been studied, it appears to be generally associated with religious ideas', and that the 'naturalistic objects' to the conventionalization of which he would trace the geometric patterns of the Bronze Age had 'a religious and talismanic meaning'. I am not concerned to deny that certain geometric patterns, for instance the swastika and the circle, may sometimes have had such a meaning; but Mr. Coffey's theory is too sweeping. It would be difficult to prove that oblong punch-marks or impressions of finger-nails and finger-tips, or the herring-bone pattern were connected with religion. [See A. Lang, *Magic and Religion*, 1901, p. 248.]

[1] *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 51, 79.

[2] A. Lang, *The Clyde Mystery*, p. 80, fig. 5.

[3] *Archaeologia*, lii, 1890, p. 53; A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 164; *Proc. Soc. Ant. Scot.*, xxxix, 1905, pp. 336–7.

not only with familiar geometrical designs, but also with concentric circles, which in one case seemed to be degenerate spirals, figures called 'double horse-shoes', which occur at New Grange and at Gavr' Inis in Brittany, and quaint representations of the eyes and eyebrows of the human face, closely resembling the so-called owl-heads which Schliemann found on vases at Hissarlik. Similar faces are sculptured on standing stones and the walls of sepulchral grottoes in the departments of the Marne, the Gard, and the Tarn, and incised on Spanish pottery of the early Bronze Age; and probably it was by way of Spain that this Mediterranean influence found its way to a remote Yorkshire moor.[1]

FIG. 36. ⅔

Sepulchral evidence as to religion.

We have already examined the evidence which the articles deposited in graves afford as to the wealth and social condition of the people who were buried there. They also suggest problems connected with their religious faith. The custom of depositing implements, weapons, or ornaments with the dead was the exception rather than the rule. Less than one-fourth of the interments in the Yorkshire Wolds were associated with any article whatever; and even in South Wiltshire barely two-thirds. In Derbyshire and Scotland relics were comparatively frequent, but by no means universal; in Cornwall almost entirely absent.[2]

[1] *Archaeologia*, lii, 1890, pp. 25–7; E. Cartailhac, *La France préhist.*, pp. 241–3; *Congrès internat. d'anthr. et d'archéol. préhist.*, 1900 (1902), p. 338; *Rév. de l'École d'anthr.*, 1904, p. 135; *L'Anthr.*, xvii, 1906, p. 135.

[2] W. Greenwell, *Brit. Barrows*, pp. 54–6, 59; *Archaeologia*, xliii, 1871, p. 401;

When we find that daggers were often placed in the hands of corpses [1] and that nearly all the flint tools on the Wolds were brand-new,[2] we may be disposed to reject the theory that the motive of those who deposited them was simple affection or superstitious dread of using what had belonged to the living; but when, on the other hand, we remember that so many of the dead were left destitute, we ask ourselves whether the articles that were placed in graves were really intended to be used in a future state.[3] But it is a mistake to expect either uniformity of custom or rigid consistency. Different tribes and different individuals may well have had different beliefs; and it is not likely that belief was always translated into action. Articles that belonged to the living have sometimes been buried from mere motives of affection or from a wish to get rid of that which was associated with the idea of death; sometimes from a vague desire to please or to avoid the displeasure of the dead.[4] Often, however, as we learn not only from historians, such as Caesar [5] and Tacitus,[6] but also from the evidence that has been collected respecting the customs of savage tribes, objects have been deposited with the dead in the full ex-

xlix, 1885, pp. 188–9; *Journ. Brit. Archaeol. Association*, N. S., vi, 1900, pp. 8–10; *Vict. Hist. of . . . Derby*, i, 175; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 56, 67–9, 86–7, 94; *Proc. Soc. Ant. Scot., passim.* Pitt-Rivers (*Excavations in Cranborne Chase*, ii, 8), speaking of twenty-two round barrows near Rushmore, remarks that 'Here, as in other places, the smaller barrows have, as a rule, been found to contain the larger number of relics'.

[1] W. Greenwell, *Brit. Barrows*, p. 39.

[2] *Ib.*, p. 60.

[3] See Lord Avebury, *Prehist. Times*, 1900, pp. 133, 135, 144, and *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 57. Seventy-nine flint saws were found by Canon Greenwell (*Brit. Barrows*, p. 262) in one barrow in the parish of Rudstone, East Riding of Yorkshire. They could hardly have been intended for the use of the deceased, unless, indeed, he was a dealer in implements, and his relatives wished to provide him with the means of plying his trade in a future state. 'On ensevelit le guerrier,' says M. Salomon Reinach (*L'Anthr.*, xvii, 1906, p. 354), 'avec ses armes, la femme avec ses objets de parure, parce qu'ils sont *tabous* et, à ce titre, retirés de la circulation et du commerce', &c. Very likely this motive sometimes operated; but it will not account for many of the deposits which I have mentioned.

[4] E. B. Tylor, *Prim. Culture*, 1903, i, 483–5.

[5] *B. G.*, vi, 19, § 4.

[6] *Germ.*, 27.

pectation that their souls would be of use to the souls of their owners in another life;[1] and when not inanimate objects only but wives, slaves, and animals have been sacrificed, it may be safely assumed that this was the motive. Nor is the belief absolutely extinct even in civilized lands. Less than half a century ago the widow of an Ulster farmer killed his horse, and, in reply to a remonstrance, asked, 'Would you have my man go about on foot in the next world?'[2] All these motives may have worked in the Bronze Age. We have seen that offerings of food were placed in food-vessels and drinking-cups; and they may sometimes have been laid beside the dead even when no vessels contained them. The bones of domestic animals, deer, and wild boars which have been found in scores of barrows, and most of which had been pounded for the extraction of the marrow, were doubtless in many cases the remains of the food upon which the survivors had feasted, but perhaps also of food offered to the dead.[3] It is possible too that the burnt bones which are sometimes mixed with cremated human bones may be the remains of animals sacrificed at the funeral, and may represent the custom, described by Homer[4] and Caesar,[5] of slaying animals of which the dead had been fond and burning them on the funeral pile;[6]

[1] E. B. Tylor, *Prim. Culture*, 1903, i, 477–90.

[2] A. Lang, *Custom and Myth*, 1885, p. 11, n. 2. See also W. Greenwell, *Brit. Barrows*, pp. 59–60, 121.

[3] *Archaeologia*, xliii, 1871, pp. 536–8; lii, 1890, p. 24; W. Greenwell, *Brit. Barrows*, p. 10. Pitt-Rivers, however (*Excavations in Cranborne Chase*, ii, 2), found that only one of the twenty-two barrows which he opened at Rushmore contained animal bones. Mr. J. R. Mortimer (*Forty Years' Researches*, p. lxx) believes that 'many of the small dish-shaped cavities containing burnt matter that are found scooped into the old turf-line under the barrows were probably made to serve as cooking ovens for roasting the funeral feasts'. Some of these cavities contained pieces of animal and human bones, charcoal, and potsherds; but Canon Greenwell (*Brit. Barrows*, p. 9) observes that 'there is no appearance of a fire having ever been kindled within them, the burnt matter, when they contain any, having evidently been placed there in that condition'. Their object remains unexplained.

[4] *Iliad*, xxiii, 171–4.

[5] *B. G.*, vi, 19, § 4.

[6] *Trans. Ethn. Soc.*, N. S., iii, 1865, p. 320; *Proc. Soc. Ant. Scot.*, vii, 1870, p. 375; W. Greenwell, *Brit. Barrows*, pp. 15–6; *Rev. de l'École d'anthr.*, xv, 1905, p. 217.

and when we are told that the skulls of oxen were carefully interred in several barrows and that a horse was buried near the summit of a barrow in Wiltshire above a cremated interment,[1] we are tempted to accept a similar explanation. We can understand why implements and weapons were often placed inside urns along with the burnt bones ; [2] but it would be vain to ask why a cow's tooth was frequently placed in juxtaposition with a corpse ; [3] and who would venture to account for the presence of the burnt bones of a fox inside an urn in a barrow on Ridgeway Hill in Dorsetshire, of the skeleton of a mole and the bones of mice in an urn in Glamorganshire, or of the skeleton of a hog in a cist in a Staffordshire barrow ? [4] We can only suppose that these mysterious deposits had some religious meaning.

But whether animals were sacrificed or not, there can hardly be a doubt of the prevalence of human sacrifice. It has been pointed out that several bodies were frequently interred in one barrow at the same time ; that in some cases a man and a woman were laid in one grave or in adjoining graves of the same date ; and that in a barrow overlooking the valley of the Derwent a woman was buried with a man whose head her hands clasped, while his legs were above hers and his right hand upon her hip ; and of these facts one finds it difficult to suggest any explanation save that of sacrifice or of suicide.[5] The innumerable potsherds which

[1] T. Bateman, *Ten Years' Diggings*, pp. 126, 129 ; *Archaeologia*, xliii, 1871, pp. 539–40. Cf. J. R. Mortimer, *Forty Years' Researches*, p. 355.

[2] A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 180.

[3] T. Bateman, *Ten Years' Diggings*, p. 25 ; *Proc. Soc. Ant. Scot.*, vi, 1867, p. 343, n. 2. The works of Canon Greenwell and Mr. Mortimer abound with instances of this practice.

[4] *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 50 ; T. Bateman, *Ten Years' Diggings*, p. 135 ; *Archaeologia*, xliii, 1871, p. 540. See also *Trans. Ethn. Soc.*, N. S., iii, 1865, pp. 317–8, and *Proc. Soc. Ant.*, 2nd ser., xv, 1893–5, pp. 424–5. Prof. Robertson Smith (*The Religion of the Semites*, 1901, p. 293) remarks that 'the mouse appears as an abominable sacrifice in Isa. lxvi, 17' ; and Sir A. Mitchell (*The Past in the Present*, p. 145) states, as a fact within his own experience, that in the last century cocks were buried alive in Scotland by church-going people in order to cure epilepsy by the propitiation of some supernatural power.

[5] T. Bateman, *Ten Years' Diggings*, pp. 78–9 ; W. Greenwell, *Brit. Barrows*,

lay scattered in many barrows when they were first opened, and the minute flint chips with which cinerary urns were sometimes crammed [1] remind one of the words in *Hamlet* :—

> For charitable prayers
> Shards, flints, and pebbles should be thrown on her,

though we should be mistaken if we supposed that in the Bronze Age such offerings were made in the spirit which animated the 'churlish priest' who grudged decent burial to Ophelia.[2]

A distinguished archaeologist has argued that not only in Greece and the Eastern Mediterranean but also in Gaul and Britain inhumation and cremation were associated with different conceptions of a future life; the ghost of the body which was interred being regarded as tenanting the grave, whereas, when cremation was practised, the soul was supposed to take flight to Hades or to some far land, though it could not enter the confines until the body which it had quitted was duly burned.[3] But whatever the Mycenaeans and the Greeks may have believed, there is no reason to suppose that in the West cremation was attended with any such doctrinal change. We have seen that both in the Neolithic Age and after, cremation and inhumation were practised contemporaneously and sometimes even in the same grave;[4] and recent excavations have shown that in the caves of Mentone, even in the Old Stone Age, the two modes of sepulture were in use.[5] If the Celts of the Early Iron Age believed that 'on the burning of the body the soul departed to a distant region', there is no proof that their belief

pp. 120, 164–5, 177, 243–4. Cf. O. Schrader, *Prehist. Ant. of the Aryan Peoples*, pp. 390–1. Mr. E. Sidney Hartland (*Folk-Lore*, xi, 1900, p. 91), criticizing Sir A. Lyall's remark (*Asiatic Studies*, 2nd ser., 1899, p. 247) that 'a Calabar chief explained to Miss Kingsley that the custom [of sacrificing wives at their husbands' funerals] was also a salutary check upon husband-poisoning', says that this does not explain the origin of the custom. Sir Alfred did not quote it in this sense; but it may explain the persistence of the custom even among certain ancient tribes. Cf. Caesar, *B. G.*, vi, 19, § 3.

[1] W. Greenwell, *Brit. Barrows*, pp. 10–1; A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii. 4, 34, 42, 252, 258.

[2] Act v, scene i, 218–9.

[3] W. Ridgeway, *Early Age of Greece*, i, 1901, pp. 507, 509–10, 512, 520, 524, &c.

[4] See pp. 110, 185–6, *supra*.

[5] *L'Anthr.*, xvii, 1906, p. 293.

was different when they laid the body in the grave ; and who will maintain that the religious ideas of the Gauls were revolutionized when in the second century before Christ cremation once more became the rule, or that among the Britons of Caesar's time cremation and inhumation, which had each their votaries, were the outward signs of religious beliefs that were utterly unlike?[1]

Engraved stones.

We may perhaps hope to find other clues to the religious ideas of the Bronze Age in megalithic monuments and in the engraved stones which have been already mentioned.[2] There are certain designs upon the latter of which the meaning is evident. The figure of an axe graven on a cist at Kilmartin in Argyllshire has many analogues on dolmens in the Morbihan and on the walls of artificial sepulchral grottoes in the department of the Marne ; and, as the axe in the Mycenaean Age was a symbol of Zeus, we may suppose that such engravings represented a widespread cult of one of the most fruitful of human inventions, which originated in neolithic times, and survived in the manufacture of miniature celts to serve as pendants and, still later, in the use of stone celts as amulets.[3] The most common devices, however, are small circular depressions, called cup-markings, and concentric circles ; while occasionally groups of concentric circles are united by grooves. Cup and ring markings are found on the stones of cists, on standing stones, on boulders, and on rocks in most parts of Scotland, in Carnarvonshire and Merionethshire, in Northumberland, Cumberland, Westmorland, Lancashire, Yorkshire, Man, Derbyshire, Staffordshire, Dorsetshire, and Cornwall, and likewise in Ireland, France, Germany, Switzerland, Portugal, Scandinavia, Asia, Africa, and America.[4] Natural cup-

[1] See p. 286, *infra*.

[2] See pp. 177, 183, *supra*.

[3] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 55–65, 145 ; *Anc. Bronze Implements*, pp. 39, 134 ; *Archaeol. Cambr.*, 6th ser., iii, 1903, pp. 224-38 (especially 235–8).

[4] *Journ. Brit. Archaeol. Association*, xvi, 1860, p. 120 ; xxxv, 1879, pp. 16–8, 21–5 ; *Mem. Anthr. Soc.*, ii, 1866, pp. 277–9 ; Sir J. Y. Simpson, *Archaic Sculpturings*, &c., 1867, pl. xiii, figs. 3 and 5 ; W. Greenwell, *Brit. Barrows*, p. 7 ; *Proc. Soc. Ant. Scot.*, xvi, 1882, pp. 79–80, 85, 101, 104, 121-43, 300-401 ;

markings have been noticed on the covering-stones of certain dolmens;[1] and it may be that such stones were deemed lucky and that, when they could not be obtained, they were imitated; but of those which are artificial the significance remains unknown.[2] The rings may perhaps in some instances be symbolical of sun-worship, for on the cairn of Lough Crew in Ireland and in Scandinavia a few have rays;[3] and since we find them on the covering-stones of cists, while in Australia similar designs, drawn on rocks, are magical or sacred,[4] it would seem probable that they had some religious meaning.[5] Sun-worship undoubtedly pre-

xviii, 1884, pp. 109–28; xix, 1885, pp. 394–5; xx, 1886, pp. 41–6, 135, 358–60; xxi, 1887, pp. 143–51; xxiii, 1889, pp. 125–37, 140; xxix, 1895, pp. 68–71, 73, 91, 193; xxxiii, 1899, pp. 363–4, 368, 371; xxxvii, 1903, p. 22; xxxviii, 1904, p. 148; E. Cartailhac, *La France préhist.*, 1889, pp. 246–7; *Archaeologia*, lii, 1890, p. 39; A. Bertrand, *La religion des Gaulois*, 1897, pp. 62–3; *Congrès internat. d'anthr. et d'archéol. préhist.*, 1900 (1902), pp. 269–70; *Rev. mensuelle de l'École d'anthr.*, xi, 1901, p. 55; *L'Anthr.*, xiii, 1902, pp. 696, 701, 710–1; xiv, 1903, pp. 536–7; *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., ii, 1902, pp. 381–2; B. C. A. Windle, *Remains of the Prehist. Age*, p. 127. It would seem that certain cup-markings, at all events in the British Isles, France, Spain, and Scandinavia, belong to the Neolithic Age (Sir J. Y. Simpson, *Archaic Sculpturings*, &c., p. 29; E. Cartailhac, *Age préhist. de l'Espagne*, 1886, pp. 174–5; *La France préhist.*, 1889, pp. 246–7.

[1] W. C. Borlase, *Dolmens of Ireland*, ii, 437. Mr. W. Frazer (*Journ. Roy. Soc. Ant. Ireland*, 5th ser., v, 1895, pp. 69–70) affirms that 'almost without exception the simple "cupules" . . . on our rude stone monuments are to be attributed to . . . *Echinus lividus*' (a sea-urchin). In many cases, however, the marks of tools are unmistakable (W. Greenwell, *Brit. Barrows*, pp. 342–3, 433).

[2] They have been found on the porches of churches at Quimperlé, on the north porch of the cathedral at Quimper, and (with crosses) on the thresholds of houses of the fifteenth, sixteenth, and seventeenth centuries, near Carnac (*Proc. Soc. Ant.*, 2nd ser., xvii, 1897–9, pp. 328–9).

[3] *Journ. Roy. Soc. Ant. Ireland*, 5th ser., vi, 1896, p. 59. Concentric circles with rays appear to belong to the later period of the Bronze Age (*ib.*, pp. 59, 65–6). Rays are also found on a spiral carved on a megalith in New Caledonia (*L'Anthr.*, xiii, 1902, p. 697, fig. 9).

[4] A. Lang, *Magic and Religion*, pp. 245–6, 253–4. Mr. Lang (*The Clyde Mystery*, pp. 66, 79) observes that similar markings on rocks, &c., in different countries may have different meanings.

[5] Rectilinear figures like those which are common on pottery of the Bronze Age have also been found on these stones (*Archaeol. Cambr.*, 6th ser., ii, 1902, pp. 209, 226–7). It seems probable that the famous 'Cerne Giant'—a colossal human figure wielding a club—which is cut in the chalk on the hill-side east of Cerne Abbas in Dorsetshire, may belong to the Bronze Age and be connected with phallus-worship. See *Vict. Hist. of . . . Buckingham*, i, 189. It has been pointed out (*Proc. Dorset Nat. Hist. and Ant. Field Club*, xxii, 1901, pp. 107–9)

vailed in certain parts of the British Isles. A few years ago there was found in Zeeland a gold-plated bronze disk, engraved with concentric circles and mounted on a miniature car with the model of a horse attached, which was recognized by all archaeologists as a votive object, connected with the worship of the sun. Similar disks, two of which are ornamented with a cruciform pattern—a well-known solar symbol—have been exhumed in Ireland, and a fragment of one in a barrow near Bath.[1] Besides the spirals which have been already mentioned, the most remarkable of all the rock-carvings is a swastika on a rock near Ilkley, identical with one which has been discovered in Sweden, not far north of Gothenburg: the oldest known examples of this mystical figure come from the second city that was built upon the site of Troy.[2]

Sun-worship.

We have seen that many barrows and cairns were immediately surrounded by, or enclosed, rings of standing stones which were part of the sepulchral structure. It is now time to consider the larger stone circles and other megalithic monuments which have occasioned voluminous controversies. They were not invented in the Bronze Age; for, as we have seen,[3] some of the long barrows were surrounded by peristaliths: the famous circle of Callernish in the island of Lewis contains a chambered cairn, from which it is structurally distinct;[4] and some of our circles which are apparently non-sepulchral may have been set up in transitional times. But the development of the circle, which can be traced most clearly in Scotland, was gradual. In the chambered cairns and chambered long barrows the

Stone circles and other megalithic monuments.

that it is petrographic, colossal, nude, ithyphallic, and clavigerous; and that 'forms which possess these five characteristics have been found in the rock carvings of Scandinavia... and belong only to the Bronze Age and to its overlap with the Early Age of Iron'. See J. J. Worsaee, *Industrial Arts of Denmark*, 1883, pp. 112–3.

[1] *Proc. Soc. Ant.*, 2nd ser., xx, part i, 1903–4, pp. 6–13, part ii, 1904–5, pp. 254–5; *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 151–2.

[2] *L'Anthr.*, iv, 1893, pp. 564, 721; *Report of... the Brit. Association*, 1904 (1905), p. 723. Cf. *Proc. Soc. Ant. Scot.*, xix, 1885, p. 391.

[3] See p. 105, *supra*.

[4] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 122–3.

peristalith as a rule was merely an adjunct: in many unchambered cairns and round barrows the stone setting is still a subordinate part of the whole; but, gradually separating itself, it became the leading feature of the monument, while the central cairn or barrow frequently disappeared, and was replaced by a simple cist.[1] By similar stages the encircling trenches and banks in Wiltshire and Oxfordshire became distinct from the small disk barrows which they contained.[2]

Stone circles are to be seen in the northern counties of England, in Derbyshire and Staffordshire, Shropshire and Cheshire, Oxfordshire, Gloucestershire, Wiltshire, Dorsetshire, Devonshire, Somersetshire, and Cornwall; and also in Glamorganshire, Orkney, the islands of Arran and Lewis, Argyllshire, Perthshire, Inverness-shire, Banffshire, Aberdeenshire, and Kincardineshire.[3] Menhirs, or isolated standing stones, and stone rows are found in this island only on Dartmoor, in Cornwall, Northumberland, Scotland, and Wales.[4]

[1] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 122–3; *Archaeol. Cambr.*, 5th ser., xvii, 1900, p. 224.

[2] I do not mean to suggest that all stone circles were derived from peristaliths; but I do not think that we should be justified in differentiating from the peristaliths by a hard and fast line those larger circles in which no traces of interment have been found. Mr. John Ward (*Vict. Hist. of . . . Derby*, i, 169), remarking that most of the round barrows, or rather cairns, of Derbyshire consist merely of stones 'thrown together anyhow', says that 'a slight advance is the introduction of a kerb of larger stones laid upon the ground to confine the proposed mound'; and he goes on to observe that in those cases in which the stones of the mound itself have been removed, the kerb 'may remain as a ring of stones easily mistaken for a circle'. He evidently believes that the kerb was merely a structural improvement. Perhaps in Derbyshire, though even this is not certain. The object of the stone rings which have been found within cairns, and of those which stood upon barrows in Northern Germany, was certainly not utilitarian; and the kerb may have had a religious or mystical meaning. Nor is there any evidence that it was an 'advance' upon the structureless cairn.

[3] *Archaeologia*, xxxv, 1853, pp. 232–58; lii, 1890, p. 39; W. Greenwell, *Brit. Barrows*, p. 402; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 111, 113–4, 119–23, 300–1; *Journ. Brit. Archaeol. Association*, N. S., vi, 1900, pp. 11–2; *Journ. Anthr. Inst.*, xxx, 1900, pp. 57, 60, 67, 70; B. C. A. Windle, *Remains of the Prehist. Age*, pp. 197–204; *Vict. Hist. of . . . Derby*, i, 181–4; *Archaeol. Cambr.*, 6th ser., vi, 1906, p. 282.

[4] *Journ. Brit. Archaeol. Association*, xviii, 1862, p. 50; W. Greenwell, *Brit.*

In form as well as in size British stone circles present numerous varieties.[1] It would, however, be useless, at all events until circles of every kind had been excavated, to attempt to account for their distinctive features; and it is significant that, although various districts have types of their own, there are examples of divers kinds in close proximity.[2] Many were simple rings. Some consisted of concentric rings; and here and there small circles, each of which was outside the others, were enclosed within a greater. Sometimes the stones were set up in close proximity; sometimes in open order.[3] Among circles of the latter kind Stonehenge, Avebury, and Callernish were approached by stone avenues,[4] the existence of which has been tentatively explained by the supposition that originally the spaces between the stones of the circle were filled by walls intended to keep out beasts.[5] A few circles are surrounded by ditches, which were spanned by causeways; others by both ditches and banks; and it is noteworthy that at Avebury the ditch lies within the bank, while at Stonehenge in the same county it surrounds it.[6] In many circles of Banffshire, Kincardineshire, and Aberdeenshire, there is a recumbent stone, placed intentionally in that position,

Barrows, p. 402; W. C. Lukis, *Prehist. Stone Monuments of the Brit. Isles,—Cornwall*, p. 16. The extreme rarity of stone rows in Cornwall, contrasted with their abundance on Dartmoor, suggests to Mr. G. F. Tregelles (*Vict. Hist. of . . . Cornwall*, i, 402) 'a difference in cult'.

[1] See *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., ii, 1902, pp. 60–2.

[2] For instance in Cornwall (*Vict. Hist. of . . . Cornwall*, i, 379), Inverurie (*Proc. Soc. Ant. Scot.*, xxxv, 1901, pp. 246–7), and Lewis (*ib.*, xxxviii, 1904, p. 190).

[3] *Proc. Soc. Ant.*, 2nd ser., ix, 1881–3, p. 151; *Proc. Soc. Ant. Scot.*, xxxv, 1901, p. 246; xxxviii, 1904, p. 190; *Journ. Roy. Inst. Cornwall*, xiv, 1901, p. 378.

[4] *Journ. Anthr. Inst.*, i, 1872, p. cxi. Roger Gale (whose testimony is accepted by Mr. W. C. Lukis (*Proc. Soc. Ant.*, 2nd ser., vii, 1876–8, pp. 270–1), writing in 1740 to Stukeley, said that he remembered having seen the holes in which the stones of the Stonehenge avenue had been placed.

[5] *Proc. Soc. Ant.*, 2nd ser., x, 1884–5, p. 320.

[6] *Ib.*, ix, 1881–3, pp. 150–1; *Proc. Soc. Ant. Scot.*, xxxv, 1901, p. 246; *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., ii, 1902, pp. 60–2.

—a feature which appears to be elsewhere unknown:[1] a few in Aberdeenshire have a solitary pillar in the centre;[2] while Stonehenge, the Rollright Stones in the Cotswold Hills, and some Scottish circles are distinguished by a similar stone which stands outside.[3]

The imaginative Stukeley, whose teaching is still echoed in many handbooks, regarded stone circles as Druidical temples; and although nearly every modern antiquary feels bound to ridicule this theory, none can prove that it does not contain a kernel of truth. Druids presided at all religious ceremonies;[4] and it would be rash to deny that in stone circles religion had any part. The foremost archaeologist of France has virtually sanctioned the discredited theory;[5] and if there is any truth in the view, which still has respectable advocates, that some circles were solar temples, Druids may well have directed the worshippers. It has been contended that many circles were orientated to the place of the midsummer sunrise, and that the presence of the solitary outlying stones would be inexplicable unless they were set up as pointers. These monoliths, however, are very rare: some are in positions which cannot be reconciled with any theory of sun-worship; and when they are absent and there is no avenue, it is clearly impossible to prove that the circle was orientated at all.[6] It is true that the existence of an interment within a circle no more proves that it was not a temple than the graves in Poets' Corner prove that Westminster Abbey is not a church: but the most enthusiastic advocates are forced to admit that many circles show no trace of orientation,

[1] *Proc. Soc. Ant. Scot.*, xxxiv, 1900, pp. 143, 196–7; xxxv, 1901, p. 246; xxxvii, 1903, p. 141; xxxviii, 1904, pp. 293–4.

[2] *Ib.*, xxxv, 1901, pp. 246–7.

[3] *Ib.*; *Folk-Lore*, vi, 1895, pp. 7, 12. See also *Vict. Hist. of ... Cumberland*, i, 245, 247.

[4] Caesar, *B. G.*, vi, 13, § 4; 16, § 2; 21, § 1.

[5] *Rev. celt.*, xiii, 1892, p. 194. Demeter was worshipped in stone circles in the city of Hermion (Pausanias, ii, 34, § 10).

[6] Cf. *Vict. Hist. of ... Cumberland*, i, 245, 247, with *Proc. Soc. Ant.*, 2nd ser., x, 1884–5, p. 312. Mr. A. L. Lewis (*Journ. Anthr. Inst.*, xv, 1886, p. 479) assumes that in some cases the external object by which the orientation was determined was a mountain.

and the evidence upon which they rely is sometimes of the flimsiest kind.[1] The one statement which can be positively made about the object of stone circles is that very many of them were erected in honour of the dead. Many enclose cairns or barrows: many others contained human remains, almost always cremated, in cists.[2] Stone circles are associated with sepulchres not only in Britain but in Scandinavia, Northern Germany, France, Spain, Italy, North Africa, Syria, and India, indeed in every country in which they exist.[3] It is true that in many English circles evidence of

[1] See *Journ. Anthr. Inst.*, xi, 1882, pp. 3–7, 117–22; xv, 1886, pp. 471–81; xx, 1891, p. 285; xxx, 1900, p. 70; *Archaeol. Journal*, xlix, 1892, pp. 139, 146; *Journ. Roy. Inst. Cornwall*, xiii, 1895, pp. 111–2; and *Proc. Soc. Ant. Scot.*, xxxiv, 1900, p. 196. Mr. A. L. Lewis, the principal advocate of the solar temple theory, seems to be satisfied with almost any kind of orientation. Thus he tells us that of twenty-one circles which he observed in Southern Britain nineteen 'had a special reference to the north-east', that is to the midsummer sunrise: but he maintains that a 'line due east through the Stannon and Fernacre circles to Brown Willy evidently was meant to indicate the equinoctial sunrise'; and in another case he insists that the object pointed at was the pole star.

Mr. G. F. Tregelles (*Vict. Hist. of . . . Cornwall*, i, 404–5), after making careful investigations with his compass in Cornwall, has arrived at results 'mainly negative', and concludes that 'there is not apparently such evidence of orientation as would satisfy a critical observer'.

Mr. W. C. Lukis, on the other hand (*Prehist. Stone Monuments of the Brit. Isles,—Cornwall*, p. vi), remarking that circles sometimes occur in groups, asks, 'if they were temples . . . why should the worshippers have been divided into so many different congregations?' As it is not contended that all circles were solar temples, this argument would obviously apply only to those particular instances; and even with this limitation it is inconclusive. Each circle was probably erected in honour of some one chieftain; and it remains possible that sun-worship may have been practised by his clan. We can hardly suppose that the erection of circles was supervised by a central hierarchy who aimed at economizing labour! See p. 479, *infra*.

[2] *Proc. Soc. Ant. Scot.*, vi, 1867, pp. 337–9; xviii, 1884, pp. 319–23; xxix, 1895, p. 302; xxxiii, 1899, p. 363; xxxiv, 1900, pp. 151, 186, 197; xxxv, 1901, pp. 194, 219, 247; xxxvi, 1902, p. 579; xxxvii, 1903, p. 141; xxxviii, 1904, pp. 293–4; xxxix, 1905, pp. 192–5; *Proc. Soc. Ant.*, 2nd ser., viii, 1879–81, pp. 291–2, 389–92, 471–2; x, 1884–5, p. 312; J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, pp. 111, 113–4, 116–8; *Archaeol. Review*, ii, 1889, pp. 313–5; *Trans. Devon. Association*, xxvii, 1895, p. 442; xxx, 1898, p. 107; xxxv, 1903, p. 142; *Journ. Anthr. Inst.*, xxx, 1900, pp. 57, 67; *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., ii, 1902, pp. 60–1; *Vict. Hist. of . . . Cumberland*, i, 236 n. 5, 245, 247, 249; *Vict. Hist. of . . . Derby*, i, 183; *Vict. Hist. of Cornwall*, i, 401; *Archaeol. Cambr.*, 6th ser., vi, 1906, pp. 286–92.

[3] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 124;

such association is lacking;[1] but we may doubt whether in any case its absence has been absolutely proved; and if the excavations had been directed by an antiquary as wealthy and as diligent as Pitt-Rivers, it might have been forthcoming.[2] But supposing that there are circles in which no burial ever took place, it does not follow that they were unconnected with sepulchral usage: like the empty barrows which, as we have seen, are cenotaphs, they may have been erected in honour of brave men who had fallen in battle or of some chief whose body could not be recovered. Nor are circles the only megalithic monuments the object of which was sepulchral. The menhirs of France are often grouped with dolmens and burial mounds;[3] and there is not a single stone row or avenue on Dartmoor which is not associated with cairns, barrows, or cists.[4] One, which is more than two miles long—longer than any in Brittany—

Matériaux pour l'hist . . . de l'homme, 3e sér., ii, 1885, pp. 368–70; A. Bertrand and S. Reinach, *Les Celtes dans les vallées du Pô et du Danube*, 1894, pp. 80–5; W. C. Borlase, *Dolmens of Ireland*, ii, 507, 644; iii, 720, 728, 753; *Comptes rendus . . . de l'Acad. des inscr.*, 1904, pp. 560-4.

[1] See *Archaeol. Journal*, xlix, 1892, p. 139; *Proc. Soc. Ant.*, 2nd ser., xix, 1902, p. 98; and *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., ii, 1902, p. 60, §§ 5–6.

[2] Mr. G. F. Tregelles (*Vict. Hist. of . . . Cornwall*, i, 404) remarks that 'the principal English [as distinguished from most Cumbrian and Scottish] circles have never been proved to be' sepulchral; but neither have they been proved to be non-sepulchral. It is said (*Proc. Soc. Ant.*, 2nd ser., xix, 1902, p. 98) that the circle of Sunken Kirk in Westmorland has been subjected to a 'searching exploration'. Was the whole area excavated? Pitt-Rivers (*Excavations in Cranborne Chase*, iv, 148), speaking of the fifty two secondary interments which he discovered just outside a barrow at Rushmore (see p. 178, *supra*) says, 'They showed no trace whatever on the surface . . . and would never have been discovered had it not been for the practice I have established of trenching down to the undisturbed chalk the entire surface of the ground contained within the area of the contoured plan of the Barrow'.

Mr. R. Burnard (*Vict. Hist. of . . . Devon*, i, 359–60) observes that 'fires seem to have been kindled all over the [Fernworthy] circle, for every scoop of the pick and shovel . . . displayed charcoal', and, remarking that this monument is the 'predominant feature of a group of sepulchral remains', conjectures that it was 'the crematorium or the site of the funeral feasts or both'.

[3] A. Bertrand, *Archéol. celt. et gaul.*, 1889, p. 103. Cf. *Proc. Soc. Ant.*, 2nd ser., viii, 1879–81, p. 288, and *L'Anthr.*, xvi, 1905, p. 530.

[4] *Journ. Anthr. Inst.*, i, 1872, pp. cxi, cxiii–cxvi; *Trans. . . . Devon. Association*, xxxiv, 1902, p. 117; xxxv, 1903, p. 429; *Vict. Hist. of . . . Devon*, i, 370.

links a circle to a cairn, and was perhaps designed to perpetuate the memory of two ancestors who had done great deeds.[1]

Perhaps among the many superstitions about these monuments which have survived into modern times there are some that recall the purpose for which they were designed. When Camden wrote, the Rollright Stones were still regarded as petrified men; and it has been suggested that the belief pointed to a time when popular imagination 'transferred to the stone that marked the resting-place of the departed something of his very material being'.[2]

Stonehenge.

But of all the megalithic circles of our island one only is familiar, even by name, to us all. Stonehenge is the most famous and in its artlessness the most artistic of all rude stone monuments. Even those who have never visited it are acquainted with its form; and the imagination of Turner has caught the spirit of the scene. The grandeur of Stonehenge does not depend upon size: in its best days it bore much the same relation to Avebury as the Sainte Chapelle to the cathedral of Notre Dame; but, weather-worn and mutilated, with many of its stones fallen and others gone, it impresses all who are sensitive to nobility of design as the creation of a master mind. When the work was finished, if indeed it was not left incomplete, the outer circle probably formed a continuous architrave, all the stones supporting imposts, whose ends were wrought into bosses that rested in hollows prepared for their reception. Within was an incomplete circle of smaller stones, which in their turn surrounded five great trilithons, disposed in the form of a horse-shoe, of which two only remain. They have analogues in Tripoli and in Syria; but in this island they are unique.[3] On their inner side was a similar group of lesser stones; and within this choir lies a vast block, which is known as the Altar Stone.[4] From the north-

[1] *Proc. Soc. Ant.*, 2nd ser., viii, 1879–81, pp. 288, 292, 471–4.

[2] See Dr. A. J. Evans's interesting account of the folk-lore of the Rollright Stones in *Folk-Lore*, vi, 1895, pp. 21–3, 30–2, and cf. Lord Avebury's *Origin of Civilisation*, 1902, p. 388.

[3] *Journ. Ethn. Soc.*, i, 1869, p. 59; *Archaeol. Review*, ii, 1889, p. 325.

[4] *Ib.*, p. 316.

eastern point of the trench that surrounds the rampart an avenue, flanked on either side by a bank and a shallow ditch, may still be traced for some four hundred yards; and on it stands the huge pillar called the Friar's Heel.

A portion of the area of Stonehenge has recently been excavated; and more than a hundred of the rude tools have been recovered with which the stones were dressed. It was proved that the great sandstone boulders, commonly called sarsens, had been roughly trimmed where they were found on Salisbury Plain; for the fragments that were found by the excavators were very few.[1] After they had been carried to the place where they now stand [2] they were dressed with a skill which shows how far superior the masons were to those who had set up the rough blocks of Avebury. Each pillar was gradually uplifted by levers until it could slide down the sloping rim of the pit which the workers with their deer-horn picks had excavated, and of which the other three walls were vertical: then it was hoisted by ropes till it stood upright, and finally secured by a packing of smaller stones which supported it below. It is thus that megaliths are commonly erected in Japan to this day.[3] How the huge imposts were elevated is somewhat doubtful. The Khasis shove theirs up an earthen bank.[4] In Japan the stone is raised at one end by wooden levers, logs being inserted beneath it: the other end is raised by the same means; and thus by slow degrees the proper level is attained, when the stone is forced on to its supports.[5] Once it was thought that the 'blue-stones' of which the inner circle is composed had been fetched from Cornwall or Dartmoor,[6] or oversea from Ireland; but the geologist who was consulted after the excavation inferred from the vast number of angular chips which were discovered within the small area of operations that the stones had been not only dressed

[1] *Archaeologia*, lviii, 1902, pp. 15, 20, 40, 42, 115.
[2] See *Archaeol. Review*, ii, 1889, p. 318.
[3] *Archaeologia*, lviii, 1902, pp. 73–5, 80–2.
[4] *Archaeol. Review*, ii, 1889, p. 318.
[5] *Archaeologia*, lviii, 1902, p. 83.
[6] Some geologists have suggested that they may have come from Wales or even Cumberland!

but also chipped into shape by the site of Stonehenge; and one can hardly believe that if it had been necessary to carry them from afar, the builders would not have reduced their weight by rough-hewing them where they were found.[1]

Stonehenge has a literature of its own which comprises nearly a thousand works. It has been assigned to the Neolithic Age, to the Bronze Age, to the era of Roman dominion, and to a time when the Saxons had been long settled in Wessex. Many years ago Pitt-Rivers pointed out the only way in which these controversies could be closed; but unfortunately the recent excavation was confined to a small area. It only proved that the use of copper was not unknown in Wiltshire when the stones were set up; for on one of the sarsens, seven feet below the surface, was found a stain produced by contact either with copper or bronze. Deer-horn picks were commonly used in the Bronze Age, and bronze tools are useless for working stone; therefore the stone implements which the excavations brought to light leave the question of date unsettled. The absence of bronze implements is of course no proof that the monument belonged to the Stone Age; not a single article of bronze was found in twenty-four barrows of Rushmore in South Wiltshire, every one of which was erected when bronze was common.[2] Moreover, with hardly an exception, every primary interment that has been found within a megalithic circle in Britain was made in the Age of Bronze.[3] All antiquaries agree that of all the British circles Stonehenge was the most elaborate; and the natural conclusion is that it was one of the latest of them all. Two barrows are encroached upon and partially surrounded by the rampart, which must therefore be of later date; and chippings of both sarsens and blue-stones were found by Hoare in one of the surrounding barrows along with a bronze dagger and a bronze pin. On the other

[1] *Archaeologia*, lviii, 1902, pp. 115–6.

[2] Cf. p. 156, *supra*.

[3] The circle at Callernish, which belonged to the Scottish Stone Age, and within which an interment was made, may have been contemporary with the Bronze Age of England.

hand this discovery proves that Stonehenge existed before the period of the barrows, not one of which is later than the Bronze Age, came to an end.

Nevertheless a distinguished astronomer, who has been a President of the British Association, recently assigned a date to Stonehenge with which these facts are irreconcilable; and although his theory was demolished by a brother astronomer, he has not hesitated to republish it. Stonehenge, he insists, was originally built a thousand years before the trilithons were added; and the trilithons represent a reconstruction and a re-dedication, which took place about sixteen hundred and eighty years before the birth of Christ. His chronological argument rests upon the assumption that Stonehenge was a temple, consecrated, at its hypothetical second dedication, to the cult of the solstitial sun. Remarking that the avenue extends in the general direction of the sunrise at the summer solstice, he attempted to determine its azimuth. Unhappily the bearing was not everywhere the same. He took the mean, and found that it nearly coincided with a line drawn from the principal bench mark of the Ordnance Survey on Sidbury Hill, the site of an ancient fort, to the centre of Stonehenge. Although there was no evidence that the erection of Stonehenge had the remotest connexion with Sidbury Hill, although the hill itself is not visible from Stonehenge, he found it convenient to discard his own calculation of the azimuth of the avenue and to adopt instead the bearing of the bench mark. Then, making the further assumption that the sun-worshippers adopted as the moment of sunrise the time when the upper tip of the sun first appeared above Sidbury Hill—a phenomenon which is very rare—he ascertained from the rate of change in the obliquity of the ecliptic that it would have been there visible about sixteen hundred and eighty years before the Christian era; or perhaps two centuries earlier or later. Nor did his assumptions end here. Although the Alexandrian astronomer who constructed the Julian calendar miscalculated the date of the summer solstice, he assumed that sixteen centuries earlier the barbarous inhabitants of a northern

island could tell it exactly; and he assumed that, in order to observe the sunrise, they stood at the exact point within the circle at which it was convenient to him to place them.[1]

But such laborious puerilities will not trouble the unlearned wayfarer who feels the enchantment of the past. For him it is enough that Stonehenge was the work of men who felt the majesty of death, and for whom no toil was too great that could do honour to the dead. Chronology has little interest for him: whether Stonehenge was built to hallow the vast necropolis in which it stands, or the dead were brought from afar to lie beneath its shadow, he knows that the three hundred barrows and the great monument are indissolubly connected. The moment when he descried the grey weather-beaten stones on the lonely Wiltshire upland will not fade from his mind. Above the south horizon appeared the slender spire of Salisbury; and the work of the Middle Age and of the Age of Bronze awakened emotions of the same kind: for both were erected in obedience to the thought that man cannot live by bread alone. It may be that those who set up the circle thought differently from the believers who thronged it in later times: the cult of ancestors, the worship of the sun, the adoration of the Celtic deity who was the counterpart of Zeus may have called successive generations of pilgrims to the holy place. Passing beneath the trilithons and among the prostrate stones, one thinks of all that has been done and suffered since mason and digger worked side by side to execute the nameless architect's design. Time-honoured even when the Roman first landed on our shore, Stonehenge was standing in all its glory when the Greek explorer came who first made known our island to the civilized world.

The voyage of Pytheas.

It was about the time when the conquests of Alexander the Great were revealing the far east to the eager curiosity of the Greeks that Pytheas set forth from Massilia on the

[1] See pp. 468–77, *infra*. In a case containing a model of Stonehenge in the Prehistoric Room of the British Museum it is actually stated that 'on the supposition that Stonehenge was a Sun-temple, its date has been astronomically determined'! I would ask the Keeper to consult Mr. Hinks's paper in the *Nineteenth Century*, June, 1903.

peaceful voyage which was to bring Northern Europe within their ken. Such knowledge of Gaul and Britain as had already reached the Mediterranean was of the vaguest kind.[1] It has indeed been argued that the Greek word for tin, *cassiteros*, which occurs in Homer, was of Celtic origin, and was learned by the Greeks from traders who as early as the ninth century before the Christian era procured tin from Cornwall.[2] If this conjecture were accepted, it would suggest that the existence of an island somewhere in the far northern ocean was at that time known to a few dwellers in the south. It has also been supposed that the lines in the *Odyssey* which describe the country of the Laestrygones, where the summer nights were short, were founded upon stories told by sailors who had seen the British Isles;[3] but the passage seems more applicable to Scandinavia, which, owing to the amber trade, was from an early period of the Bronze Age connected with South-Eastern Europe. The knowledge that tin was to be got from Cornwall must, however, have reached the Mediterranean at a remote epoch through the ties that connected Britain with Gaul. Himilco, the Carthaginian admiral who, more than a century before the birth of Pytheas, sailed into the English Channel, perhaps undertook his voyage for the purpose of opening up trade with Cornwall at a time when the tin mines of Galicia were nearly exhausted; but it is unlikely that his report, upon which the poem of Festus Avienus was ultimately based, was originally known except to his own government.[4] In the time of Pytheas, however, there was a regular overland trade in tin between Cornwall and Massilia, and doubtless also a seaborne trade between Cornwall and the Carthaginian port of Cadiz.[5]

Pytheas was a great man. As an explorer he was the forerunner of Columbus; and it is not easy for us, who live in an age when hardly any part of the earth's surface, except the polar regions, remains untrodden, to conceive

[1] See Polybius, iii, 38, §§ 1-2; 58-9; xxxiv, 10, § 7; Dion Cassius, xxxix, 50, §§ 3-4.

[2] See p. 494 *infra*.

[3] See p. 513, note, *infra*.

[4] See p. pp. 490-1, 512, *infra*.

[5] See pp. 499-513, *infra*.

the animation with which his narrative was discussed by his Greek contemporaries and by the geographers of a later time.[1] His scientific eminence is attested by the use which was made of his writings by Eratosthenes, the Alexandrian geographer and poet, and by Hipparchus, the greatest astronomer of the ancient world.[2] With a gnomon which he erected in his native town he obtained an estimate of its latitude which erred by no more than a few seconds;[3] the observations which he made in the Atlantic enabled him to announce that the height of the tides had a definite relation to the moon's age;[4] he determined with some approach to accuracy the configuration both of Gaul and Britain;[5] and at four stations in or near our island he took observations of the altitude of the sun at noon, from which Hipparchus calculated their respective latitudes.[6] Unfortunately the work 'On the Ocean', which he based on the diary of his voyage,[7] has perished. All that we know of it is contained in a few fragments, quoted with more or less accuracy by the astronomer Geminus, who was contemporary with Caesar, by Strabo, Diodorus Siculus, Pliny, and other writers.[8] Strabo, influenced by the unimaginative mind of Polybius, was bitterly hostile;[9]

[1] See H. F. Tozer, *Hist. of Anc. Geogr.*, 1897, pp. 15–6.

[2] Strabo, ii, 4, § 2. Cf. A. Bertrand and S. Reinach, *Les Celtes dans les vallées du Pô et du Danube*, 1894, p. 15.

[3] Strabo, ii, 1, § 12; 5, § 8. Cf. V. de St. Martin, *Hist. de la géogr.*, 1873, p. 101.

[4] Plutarch (*De placitis philosophorum*, iii, 17, § 2), who evidently knew nothing about the tides, ascribed to Pytheas the absurd statement that high tide occurs at full moon, and low tide at new moon (Πυθέας ὁ Μασσαλιώτης τῇ πληρώσει τῆς σελήνης τὰς πλημμύρας γίνεσθαι, τῇ δὲ μειώσει τὰς ἀμπώτιδας), a blunder which, as Müllenhoff (*Deutsche Altertumskunde*, i, 1890, p. 365) remarks, nobody could have made who had spent twenty-four hours on the Atlantic coast. I agree with him that Pytheas had anticipated the discovery of Posidonius (Strabo, iii, 5, § 8), which, needless to say, must have been made long before by Phoenician mariners, but that he was unfortunate in his reporter. Cf. H. F. Tozer, *Hist. of Anc. Geogr.*, p. 155.

[5] See p. 221, *infra*, and H. F. Tozer, *Hist. of Anc. Geogr.*, pp. 155–7.

[6] See *ib.*, p. 160, and *Geogr. Journal*, i, 1893, p. 520.

[7] Geminus, *Elem. astron.*, ed. C. Manitius, 1898, p. 70 (c. vi, § 9); K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, p. 311.

[8] *Ib.*, p. 367.

[9] *Geogr.*, i, 4, § 3; ii, 3, § 5; iv, 5, § 5; vii, 3, § 1, &c. Cf. M. Dubois,

and his treatise on geography taught many generations of readers to regard Pytheas as a romancer.

It has been supposed that the Government of Massilia, jealous of the commercial predominance of the Carthaginians, and hoping to wrest from them a share of the trade in tin, employed Pytheas as their agent. But the Massiliots already received a constant supply of tin directly from the British mines; and it is hardly credible that they could have expected to profit by importing it oversea round Spain instead of overland.[1] Nor indeed could they have expected the Carthaginians, who were all-powerful at sea, to allow their vessels to penetrate into waters which they jealously policed. Polybius, who was affluent, sneeringly remarked that a private individual, in poor circumstances, could not have travelled such distances as Pytheas claimed to have done.[2] It is no longer necessary to prove that Pytheas's travels were real; but, supposing that he could not afford to pay his own expenses, we can only conclude that the Massilian Government, or perhaps a syndicate of merchants, were sufficiently public-spirited to spend money on scientific aims. For although it would seem probable from his having extended his voyage to the amber districts that his object was partly commercial, the fact that he sailed far away from the trade routes, and spent a large part of his time in collecting ethnographical information and making astronomical and geographical observations shows that his own purpose was the advancement of science. It is unnecessary to refute the quaint suggestion that poverty compelled him to work his passage on board a Carthaginian merchantman:[3] Carthaginian ship-owners would hardly have permitted a captain to circumnavigate

Examen de la géogr. de Strabon, 1891, pp. 253–4, 264–5. In regard to the scientific eminence of Pytheas see V. de St. Martin, *Hist. de la géogr.*, p. 107; K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, pp. 311–3; *Geogr. Journal*, i, 1893, pp. 520–1; and H. F. Tozer, *Hist. of Anc. Geogr.*, pp. 47–50.

[1] H. Berger, *Gesch. der wissenschaftlichen Erdkunde der Griechen*, iii, 1891, p. 27.

[2] Strabo, ii, 4, § 2.

[3] H. d'A. de Jubainville, *Principaux auteurs de l'ant. à consulter sur l'hist. des Celtes*, 1902, p. 65.

Britain in order to gratify the whim of an alien scientist in the forecastle. If anything that relates to the voyage of Pytheas is certain, it is that he was free to direct the movements of the vessel as he pleased.[1]

The outward voyage, even before he first saw the British coast, was full of interest. After passing Cape Finisterre, he steered eastward along the northern coast of Spain, and found that, owing to the set of the current and the prevailing westerly winds, the rate of sailing was much more rapid than along the southern side of the Peninsula.[2] He touched at Corbilo, a port on the estuary of the Loire, where British tin was unshipped; noted the great bend which the Breton coast makes towards the north-west; and found in the peninsula the same tribe of Osismii whom Caesar encountered nearly three centuries later in his campaign against the Veneti. Having visited Uxisama, the modern Ushant, he struck thence along the course followed by the Phoenicians, and in twenty-four hours crossed the western arm of the Channel and landed near Belerium, the Land's End.[3] He conversed freely with the inhabitants, doubtless through the medium of an interpreter, and found them friendly and comparatively civilized. They told him that the tin was cast into ingots, shaped like ankle-bones, two of which would form a suitable load for a pack-horse, and conveyed to an island off the Cornish coast, called Ictis, which was accessible at low tide to their wagons. There it was shipped and carried to Corbilo; and thence it was Ictis.

[1] K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, p. 311.

[2] See pp. 495–6, *infra*.

[3] Strabo, i, 4, §§ 3, 5; iii, 2, §§ 1, 11; iv, 4, § 1; Diodorus Siculus, v, 21, § 3; K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, pp. 368–70, 375–7; Pauly's *Real-Encyclopädie*, iii, part i, 1897, p. 863; H. d'A. de Jubainville, *Principaux auteurs à consulter sur l'hist. des Celtes*, pp. 66–71. Sir Clements Markham (*Geogr. Journal*, i, 1893, p. 516) holds that Pytheas sailed from Uxisama to Kent, 'because he reported that the coast of Gaul, where he left it, was some days' sail from Cantion'. But what he reported was simply that Cantium was some days' sail from Gaul (*καὶ τὸ Κάντιον ἡμερῶν τινων πλοῦν ἀπέχειν τῆς Κελτικῆς φησι* [Strabo, *Geogr.*, i, 4, § 3]); and this estimate may have been based upon his homeward voyage. Professor Ridgeway (*Folk-Lore*, i, 1890, p. 97) referring to the same passage in Strabo, argues that he sailed from Brittany to the Isle of Wight. Müllenhoff gives satisfactory reasons for the view adopted in the text.

transported on horseback to the mouth of the Rhone. The whereabouts of Ictis has long been a subject of dispute. It has been identified with St. Michael's Mount, with the Isle of Wight, and even with the Isle of Thanet. This guess has, however, been discarded,[1] and no longer needs refutation. It has recently been shown that a natural causeway, formed by a limestone reef, connected in prehistoric times the coast off Lymington with Yarmouth. But this does not prove that Ictis was the Isle of Wight; nor does the fact, on which much stress has been laid, that coins of a certain type are common to Brittany, the Channel Islands, and the south-western districts of Britain. Doubtless much traffic passed by way of the Channel Islands, but not necessarily that which Pytheas described; and the Dumnonii, who produced the tin, never struck coins at all.[2] We are told that in those days St. Michael's Mount was an isolated rock begirt by a swampy wood; and that the voyage from Cornwall to the mouth of the Loire would have been too long and dangerous for ancient seamen to attempt. The former argument, in so far as it leans upon tradition, was demolished forty years ago: the legend that St. Michael's Mount was 'The Hoar Rock in the Wood' was based upon a mediaeval story which confounded St. Michael's Mount with Mont St. Michel. It is true that the eminent geologist who has proved the former existence of a causeway between the Isle of Wight and the mainland has attempted to reinforce tradition by science; but his calculations, which assume that alluvium was dispersed by marine action at a constant rate, seem hardly less liable to error than the discredited estimates of the antiquity of man which were based upon assumptions regarding the rate of deposition of stalagmite in caves.[3] Nor would any one who knows that long before the time of Pytheas men were not afraid to sail from Norway to Ireland, that the

[1] Except by Prof. Rhys (*Celtic Britain*, 3rd ed., 1904, p. 46), who, however, may perhaps have changed his mind since the appearance of Mr. Clement Reid's article in *Archaeologia*, lix, part ii, 1905, pp. 281–8.

[2] See p. 359, *infra*.

[3] See *Association franç. pour l'avancement des sc.*, 1902, 1re part., p. 268; 1903, 2e part., p. 911.

distance between Rome and Sardinia is greater than the greatest breadth of the English Channel, and that before the invention of the compass Irish monks made the voyage to Iceland, believe that the Phoenicians or the Veneti in their stout ships were too timid to cross from Cornwall to the Loire. It is not credible that shrewd merchants would have submitted to pay the heavy additional price which would have been exacted if the tin had been conveyed two hundred miles by land before it was shipped, and then to saddle themselves with the cost of conveying it by sea from the Isle of Wight to the Loire,—a voyage much longer and not less dangerous than the direct route from Cornwall. St. Michael's Mount is the one island off the south coast of Britain between the Land's End and the Isle of Wight which corresponds with Diodorus's description; it is opposite the only part of the Cornish coast where wagons could have descended to the shore; and Pengelly, Lyell, and Ussher testify that its main features have persisted unchanged for more than two thousand years.[1]

As far as the Land's End the route of Pytheas is evident: thenceforward all becomes obscure. We know that he circumnavigated Britain; for he mentioned the South Foreland and alluded to the northern extremity of Scotland, and he attempted to estimate the circumference of the island.[2] We know that he explored the amber coast, and some conjecture that he sailed to 'far-off Thule'; but it is safe to prophesy that on the details of his itinerary agreement will never be reached. He accurately indicated the position of Ireland, which Eratosthenes, guided by his observations, placed west of Britain, but which, Strabo notwithstanding insisted, was the most northerly of all inhabited lands.[3] It would seem that he landed more than once; for he had much to tell of the manners and customs of the Britons. He was especially struck by the gloominess

[1] See pp. 499–507, *infra*.

[2] Strabo, i, 4, § 3; ii, 4, § 1. Müllenhoff (*Deutsche Altertumskunde*, i, 1890, pp. 379–81) makes an ingenious attempt to explain Pytheas's exaggeration. Cf. H. Berger, *Geschichte der wissenschaftlichen Erdkunde der Griechen*, iii, 1891, p. 37.

[3] *Geogr.*, i, 4, § 4; ii, 1, §§ 13, 17; iv, 5, § 4.

of the climate; the corn, he remarked, was not threshed on open threshing-floors on account of the heavy rains and the lack of sunshine, but the ears were cut off, carried into barns, and there ground; and he learned that the grain was not merely used for food, but also for brewing a kind of beer. In the far northern districts he observed that domestic animals were few, that the fruits of more favoured lands were not to be seen, and that the only cereal was oats.[1] According to Pliny,[2] he stated that the tide rose in one place to the prodigious height of eighty cubits, or about one hundred and twenty feet. It has been supposed that this passage refers to the race of the current through the Pentland Firth;[3] but more probably Pytheas had seen the tidal wave in the Bristol Channel, which actually rises sixty feet;[4] and it must remain doubtful whether he exaggerated its volume or Pliny misrepresented his meaning.

'Ultima Thule.'

The voyage which Pytheas made to the amber coast has no place in the history of Britain; but we cannot but be interested in his account of Thule, which he called the most northerly of the British Isles.[5] It is doubtful, however, whether he even saw it.[6] He says that it was six days' sail from Britain;[7] but this statement may have been made upon the authority of natives[8] who had conversed with Scandinavian mariners on their way to or from Ireland. His description of the manners and customs of the northern peoples, of their agriculture, their domestic animals, and their food is reproduced by Strabo in a paragraph so vague that one cannot be sure whether it was intended to refer

[1] Strabo, iv, 5, § 5; Diodorus Siculus, v, 21, § 5. Cf. K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, pp. 394–6.

[2] *Nat. Hist.*, ii, 97 (99), § 217.

[3] *Mém. de l'Acad. des inscr.*, xxxvii, 1724, p. 437; H. F. Tozer, *Hist. of Anc. Geogr.*, p. 159.

[4] *Whitaker's Almanack*, 1897, p. 71. Cf. K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, p. 367, and *Geogr. Journal*, xix, 1902, p. 53.

[5] Strabo, ii, 5, § 8; iv, 5, § 5. Cf. K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, p. 392.

[6] I have come independently to the same conclusion as M. V. de St. Martin (*Hist. de la géogr.*, p. 103) and Mr. H. F. Tozer (*Hist. of Anc. Geogr.*, pp. 158–9).

[7] Strabo, i, 4, § 2.

[8] H. Berger, *Gesch. der wissenschaftlichen Erdkunde der Griechen*, iii, 1891, p. 37.

only to Britain, or to Thule as well.[1] Strabo, if he had any clear notion on the subject, must have applied it to Britain, for Thule was in his eyes a mythical land;[2] but, if Pytheas was thinking of Thule, his account may have been based upon hearsay. He described it as situated on or near the Arctic Circle,[3] and since he called it an island, his description, if he sailed thither himself, can only refer to Iceland: but Iceland, when the Northmen took possession of it, was found uninhabited except by a few monks;[4] and it may be that he simply drew his own conclusions from the reports of Britons who told him that in Thule there was one night every year on which the sun never set.[5] Again, when he said that Thule was near the frozen ocean,[6] he may only have reported what he had heard; though it is unlikely that the natives of North Britain would have made a statement so misleading about any of the Shetlands, which were within a few hours' sail of their own land. But perhaps we may find a clue in a well-known passage in Geminus's *Elements of Astronomy*.[7] 'The natives,' said Pytheas, according to this extract, 'pointed out to us the sleeping-place of the sun; for in these parts the nights were very short, in some only two, in others three hours long, so that the sun re-appeared soon after it had set.' Even in the Shetlands the duration of the shortest

[1] *Geogr.*, iv, 5, § 5. Müllenhoff (*Deutsche Altertumskunde*, i, 1890, p. 393) insists that Strabo's description must include Thule; for, he says, it compares the observations made by Pytheas in Thule with others, made in more southerly tracts, where wheat was grown and beer brewed. But, as I observe in the text, it is questionable whether Pytheas was ever in Thule.

[2] *Geogr.*, i, 4 § 3.

[3] *Ib.*, ii, 5, § 8. See H. F. Tozer, *Hist. of Anc. Geogr.*, p. 159.

[4] K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, p. 389. Cf. *Mém. de l'Acad. des inscr.*, xxxvii, 1724, pp. 436–42.

[5] Müllenhoff (*Deutsche Altertumskunde*, i, 1890, pp. 386, 401) gives sufficient reasons for rejecting the statement of Cleomedes that there was continuous night in Thule for one month. The statement of Pliny (*Nat. Hist.*, iv, 16 [30], § 104) that the winter night lasted six months needs no refutation.

[6] Strabo, i, 4 § 2.

[7] *Elem. astron.*, ed. C. Manitius, 1898, p. 70 (c. vi, § 9).—*ἐπὶ δὲ τοὺς τόπους τούτους δοκεῖ καὶ Πυθέας . . . παρεῖναι. φησὶ γοῦν . . . ὅτι 'ἐδείκνυον ἡμῖν οἱ βάρβαροι ὅπου ὁ ἥλιος κοιμᾶται. συνέβαινε γὰρ περὶ τούτους τοὺς τόπους τὴν μὲν νύκτα παντελῶς μικρὸν γίνεσθαι, ὡρῶν οἷς μὲν δύο, οἷς δὲ τριῶν, ὥστε μετὰ τὴν δύσιν μικροῦ διαλείμματος γινομένου ἐπανατέλλειν εὐθέως τὸν ἥλιον.'*

night is about five hours; but Cosmas Indicopleustes,[1] a traveller and geographer of the sixth century, affirms that the natives explained 'the sleeping-place of the sun' as the place where for twenty-four hours there was unbroken darkness. We may well conceive how Pytheas stood talking to Shetlanders or to people who lived near Cape Wrath, while they pointed in the direction of Norway, in the remoter parts of which, as they had learned from Norwegian sailors, was to be seen the midnight sun, and at midwinter there was for twenty-four hours continuous night. But Pytheas would not have told this tale if he had himself watched the sun above the horizon throughout the midsummer night; nor would he have placed Thule on the Arctic Circle if he had not believed that such a spectacle was there to be seen. For the Romans of the Empire Thule, as the northernmost of the British Isles, was Mainland, which Agricola visited.[2] But on the whole it seems most probable that Pytheas described it from hearsay;[3] that he was misled into believing it to be in the British archipelago; and that the Thule to which his informants pointed was the Scandinavian peninsula.[4]

[1] See H. Berger, *Gesch. der wissenschaftlichen Erdkunde*, &c., iii, 1891, pp. 16–7.

[2] Cf. K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, pp. 388, 392.

[3] Strabo (ii, 4, § 1) seems to imply that Pytheas avowed that he described Thule from hearsay. He tells us on the authority of Polybius that Pytheas wrote an account of *τὰ περὶ τῆς Θούλης καὶ τῶν τόπων ἐκείνων, ἐν οἷς οὔτε γῇ καθ' αὑτὴν ὑπῆρχεν ἔτι οὔτε θάλαττα, οὔτ' ἀήρ, ἀλλὰ σύγκριμά τι ἐκ τούτων πλεύμονι θαλαττίῳ ἐοικός, ἐν ᾧ φησι τὴν γῆν καὶ τὴν θάλατταν αἰωρεῖσθαι καὶ τὰ σύμπαντα, καὶ τοῦτον ὡς ἂν δεσμὸν εἶναι τῶν ὅλων, μήτε πορευτὸν μήτε πλωτὸν ὑπάρχοντα· τὸ μὲν οὖν τῷ πλεύμονι ἐοικὸς αὐτὸς ἑωρακέναι, τἆλλα δὲ λέγειν ἐξ ἀκοῆς.*

[4] My view coincides, in regard to the identification of Thule, with that of G. Hergt (*Die Nordlandfahrt des Pytheas*, 1894), and also with that of M. Camille Jullian (*Journ. des Savants*, 1905, pp. 95, n. 1, 101, n. 2). Hergt's work is not in the British Museum, and I have not been able to procure a copy; but his conclusions are summarized in *Jahresberichte der Geschichtswissenschaft*, 1895, iii, 167. He and M. Jullian (*op. cit.*, p. 101) hold that Pytheas landed in Norway, and that the Norwegians with whom he conversed pointed out to him in the distance 'le lieu mystérieux où le soleil repose durant les longues nuits du cercle polaire'. On this theory I cannot conceive how Pytheas came to regard Thule as one of the British Isles.

Müllenhoff, who identifies Thule with Mainland, argues, first (*Deutsche Altertumskunde*, i, 1890, pp. 387–8) that Pytheas would hardly have succeeded in sailing to Norway in six days on account of difficulties, which he points

Pytheas and the ethnology of Britain.

But, apart from the deeds of Pytheas himself, perhaps the most interesting information which we owe to the fragmentary record of his voyage relates to the ethnology of Britain. He learned that it was called the Pretanic Island. Before his time the Gauls for the most part had come to change the original sound *qu* into *p*; whereas certain tribes of Western Gaul[1] as well as all those Celtic-speaking inhabitants of the British Isles from whose dialect Gaelic, Irish, and Manx have been evolved retained it, though the latter afterwards modified it into *c*. On the other hand, wherever the Indo-European tongue from which Celtic was an offshoot had the sound of *p*, most of the Celtic-speaking tribes both of Britain and Gaul had let it disappear. The word *Pretanic* therefore implied the exist-

out, in navigation; secondly (p. 393), that Pytheas, who distinguished between the Celtic and the Germanic populations of Northern Europe and must have been accompanied by an interpreter, would not have confounded Norway—a non-Celtic country—with Thule; thirdly (pp. 398–9), that agriculture was introduced into Norway by the Germans, that Pytheas, in his description of Britain and of Thule, did not say that corn was not cultivated there, and that, if he had visited Norway, he would have mentioned the Lapps and the reindeer; and lastly (pp. 399–400), that the place where 'the barbarians' showed him 'the sleeping-place of the sun' was evidently the most northerly land which he reached, and was not in the Arctic Circle.

Every one of these arguments rests upon the assumption that Pytheas visited Thule, for which, as we have seen, there is no evidence. Neither is there any that Thule was inhabited by a Celtic-speaking people: it is, as we have seen (p. 152, n. 2), absolutely certain that corn was cultivated in Scandinavia in the Bronze Age; and even if Pytheas did visit Thule, there is no reason to suppose that he went sufficiently far northward to come in contact with Lapps.

Mr. Tozer (*Hist. of Anc. Geogr.*, pp. 159–60), who does not believe that Pytheas travelled further northward than 'the extremity of Britain', nevertheless holds with Müllenhoff that Thule was Mainland. He points out that 'the sleeping-place of the sun', which he of course locates in Thule, was in the Arctic Circle. 'This of course,' he continues, 'would not apply to Shetland . . . but on such a question the report of "barbarians" could hardly be expected to be accurate.' Is not this a weak argument for identifying Thule with Mainland, where, even at the winter solstice, the sun is above the horizon five hours out of the twenty-four? The 'barbarians' had not themselves penetrated within the Arctic Circle; and that darkness was anywhere continuous for twenty-four hours would not have occurred to them if they had not learned the fact from Scandinavian sailors who had seen the phenomenon or had been informed of it by eye-witnesses. Moreover, Pytheas, who so accurately determined the latitude of Massilia, would hardly have allowed himself to be persuaded that Mainland was on the Arctic Circle.

[1] See, however, pp. 410, 449, *infra*.

ence of an earlier word *Qrtanic*; and supposing that Pytheas, as some believe, heard *Pretanic* only in Gaul, it might be argued that *Qrtanic* was still the British pronunciation. If so, none of the tribes who had changed *qu* into *p*, from whose dialect Welsh, Cornish, and Breton descended, and who are commonly called Brythons, had yet invaded Britain. But if, as seems much more probable, Pytheas derived his information from Britons, the Brythons were already predominant at all events in those parts of Britain in which he conversed with them. Indeed, as we shall afterwards see,[1] it is morally certain that Brythonic tribes had been settled here at least half a century before he came.

The subject of the ethnology of the Celtic-speaking tribes of Britain is extremely difficult; and on nearly every important point Celtic philologists differ widely among themselves. It is almost an article of faith that the earlier Celtic invaders were Goidels, or tribes who had not changed *qu* into *p*; but there are some who maintain that neither in the time of Pytheas nor even in that of Caesar were there any Goidels in Britain; and that those who were settled in Wales in the third century of our era were all of Irish origin. No direct evidence indeed can be adduced for the common view; but it is hard to conceive that the earliest Celtic immigrants, unless they set out from Spain or from North-Western Gaul, should have passed by Britain in order to settle in Ireland. Even those who admit the priority of the Goidels in Britain are not of one mind. While the foremost Celtic scholar of this country maintains that when Celts first reached Britain the distinction between the Goidelic and Brythonic dialects already existed, the foremost Celtic scholar of France insists that at that time the Celtic language was everywhere the same: according to him none of the Celts had then changed *qu* into *p*: that change was made later by Celtic conquerors of Gaul, some of whose descendants afterwards colonized Britain; and the people with whom Pytheas conversed were not, strictly

[1] See pp. 232-3, *infra*.

speaking, Goidels, but simply Celts who spoke a language from which the Goidelic dialects—Gaelic, Manx, and Irish—were subsequently evolved.

On its chronological no less than on its ethnological side the Celtic question is involved in obscurity. History, archaeology, and physical anthropology can give the philologists little aid. The slender historical evidence does not warrant us in assigning the earliest Celtic invasion of Britain to a period more than six or seven centuries before the Christian era. Philologists who, a few years ago, acquiesced in this date, now put it back three centuries or more without troubling themselves to give a reason. The Hallstatt period of culture, which, in its earlier stage, coincided on the Continent with the transition from the use of bronze to that of iron, is believed to have lasted in Gaul from about 800 to about 400 B.C. As it is all but entirely unrepresented in this country by iron weapons, one might perhaps argue that Celts invaded Britain before iron implements of Hallstatt type began to be common in Gaul; but this date gives us no help, for it certainly was not earlier than the sixth century before Christ.[1] Assuming that Goidelic and Brythonic were distinct dialects before the Celts invaded Britain, there is no evidence that the Goidelic invaders (if they existed) were physically different from their Brythonic kinsmen; and if they were, the fact would throw no light upon the Goidelic invasion. For, as we have seen, even if the period of the round barrows lasted to the end of the Bronze Age, cremation, which destroys evidence of physical type, was then in vogue. Therefore we must rest satisfied with the probability that at some time after the earlier period of the British Bronze Age tribes began to invade Britain who spoke a language from which the Gaelic that we know was descended; and with the certainty that when Pytheas landed on our shore he found Brythons already in possession.[2]

[1] See *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 87, and *Guide to the Ant. of the Early Iron Age*, p. xii. Hallstattian objects are also very rare in Northern Gaul (*Rev. de synthèse hist.*, iii, 1901, p. 38, n. 1).

[2] See pp. 411–2, 445–6, 449, *infra*.

The passing of the Bronze Age.

The coming of Pytheas marks the beginning of a new era. Bronze and even stone implements were still used in the north and probably even in the greater part of Southern Britain.[1] But the Bronze Age, properly so called, had passed away : the Early Iron Age had begun.

[1] See A. Pitt-Rivers, *Excavations in Cranborne Chase*, i, 163, ii, 179–87, iv, 11, 13, 61, and *Proc. Somerset Archaeol. and Nat. Hist. Soc.*, li, 1905, p. 26.

CHAPTER V

THE EARLY IRON AGE

Iron probably introduced into Britain by Gallic invaders.

IRON-WORKING was of course familiar to the people of the Mediterranean and even to the continental Celts long before it was introduced into Britain;[1] but, it need scarcely be said, everywhere until the Middle Ages, the metal was not cast, but only wrought. Not far from Hallstatt, the only place in Europe where the gradual transition from the use of bronze to that of iron can be traced, were the iron mines of Noreia, which were certainly worked at a very early period, and from which, some archaeologists still insist, the use of iron spread to all European lands.[2] Since iron tools and weapons of the later Hallstatt type, ranging from about the beginning of the sixth to the end of the fifth century before the Christian era, are almost entirely wanting in Britain, the earliest products of our Iron Age can hardly be older than the later of these dates. Were they introduced by immigrants or in the ordinary course of trade? Among the round barrows on the Yorkshire Wolds are two, situated in the parish of Cowlam, each of which contained the skeleton of a woman. The appearance of these mounds was not different from that of many others, most of which belonged to the Bronze Age and a few perhaps to that of stone: the skeletons were interred in the contracted position which had been common for many centuries; and the pottery exactly resembled the domestic pottery which is associated with bronze. The practised explorer who opened the barrows confessed that but for

[1] *Journ. Anthr. Inst.*, xxxv, 1905, p. 262.

[2] See *Report of . . . the Brit. Association*, 1896, pp. 930–1, and W. Ridgeway, *The Early Age of Greece*, i, 407–52, 594–630. Cf. *Class. Rev.*, xvi, 1902, pp. 74–5, 88–90.

the presence of a brooch and certain ornaments of the Iron Age he would unhesitatingly have assigned them to the older period; and he accordingly concluded that no new people had come in with iron.[1] But the conclusion is not warranted except perhaps for the particular district to which these graves belong. The use of iron might have spread by barter to Yorkshire after it had been introduced by new-comers into lands nearer Gaul; and the prevalent opinion is that it was introduced about the beginning of the fourth century before Christ by Gallic invaders who spoke a Brythonic dialect.[2]

The Belgae preceded by other Brythons, who began to arrive about 400 B.C.

Caesar knew nothing of any Gallic invaders of Britain except the Belgae, who, as he gathered, inhabited the maritime districts, evidently of the south-east and south: the people of the interior, according to his informants, were aborigines. This statement, however, made no distinction between the real aborigines and the round-headed immigrants who found them in possession. It is impossible to say certainly which of the tribes in Caesar's time were Belgic, except the Belgae, the Catuvellauni, and the Atrebates, none of whom possessed territory north of the basin of the Thames;[3] but the names of tribes and of places

[1] W. Greenwell, *Brit. Barrows*, pp. 208–12. Cf. *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 109–11.

[2] Mr. Romilly Allen (*Archaeol. Cambr.*, 5th ser., xiii, 1896, p. 223) argues from the fewness of the known Late Celtic burials that the period between the introduction of iron and the Roman conquest 'cannot have been very long'. So also thought Canon Greenwell (*Brit. Barrows*, p. 212), apparently forgetting what he had very judiciously said on p. 50. The argument would lead to the conclusion that in Scotland the Late Celtic Period was almost non-existent; for only one interment of the Early Iron Age has been found there (p. 435, *infra*). Many such interments, unmarked by any tumulus, have doubtless escaped notice; and in many French departments they are unknown (*L'Anthr.*, xiv, 1903, p. 386; *Rev. de l'École d'anthr.*, xv, 1905, pp. 218–26).

[3] Sir J. Evans (*Coins of the Anc. Britons*, p. 39) remarks that, according to Ptolemy (*Geogr.*, ii, 3, § 13), the territory of the Belgae included Ischalis (Ilchester), Aquae Calidae (Bath), and Venta (Winchester), and must therefore have comprised nearly all the area corresponding with Hampshire, Wiltshire, and Somersetshire; but that in each of these counties we find a distinct coinage. Probably, he argues, when the inscribed coins of Somersetshire were struck, the Belgae only occupied the east of Hampshire and the west of Sussex. Without disputing this conclusion I would suggest that since no one Gallic tribe was called the Belgae, the British Belgae, in the narrower sense of the

mentioned by Ptolemy and other late writers show that the greater part of England and Wales and at least a considerable part of Southern Scotland were in the first century of the Roman occupation inhabited by Brythons; and it is morally certain that they did not arrive after Caesar's departure. Evidently, therefore, the Belgae had been preceded by other Brythons. But when did the first Belgic invaders appear? Those who are not content to take on trust the widely different dates which have been assigned by archaeologists will find that it is impossible to achieve any definite result. Dr. Arthur Evans has at different times conjectured that the invasion began about two hundred,[1] about one hundred and fifty,[2] and about three hundred years before the birth of Christ.[3] It would appear, however, from the time that must have been required for the gradual evolution of the successive types of British coins which will be noticed hereafter, that the prototype was introduced not less than a century and a half, possibly two centuries, before the Christian era; but it is impossible to prove, though it is generally assumed, that

term, may have been a loose confederation or aggregate of tribes or of *pagi*, each of which perhaps had its own coinage.

All scholars are, however, aware that it is generally impossible to determine the frontiers of the British tribes, even for the period of the Roman conquest, with any approach to the comparative accuracy which has been attained in the case of those of Gaul (see my *Caesar's Conquest of Gaul*, 1899, pp. 330–2). The delimitation of the tribal areas of independent Gaul depends mainly upon the reasonable assumption that they correspond for the most part exactly or nearly with those of the Gallo-Roman cantons (*civitates*). But in Britain we are not only baffled by the political changes which took place in the restless century that intervened between the invasions of Caesar and the Claudian conquest: we also find that although a recently discovered inscription (*Archaeologia*, lix, 1904, pp. 121–2) has shown that at all events in the case of the Ordovices the cantonal organization was preserved or adopted by the Roman Empire, yet, as Mommsen says (*Provinces*, i, 191 [*Röm. Gesch.*, v, 1885, p. 174]), 'the Britannic tribes, taken in the strict sense, [apparently] disappear as soon as they fall under Roman rule, and of the individual cantons after their annexation there is virtually no mention at all.' Moreover, the boundaries of the Gallo-Roman *civitates* served, in principle, to define the areas of episcopal jurisdiction; and the areas of the Gallic dioceses are known. In Britain this source of information is wanting.

[1] *Archaeologia*, lii, 1890, pp. 387–8.

[2] *Archaeol. Rev.*, ii, 1889, p. 324.

[3] *Archaeol. Oxon*, 1892–5 (1895), pp. 159–60.

money was coined by the first Belgic invaders. The date of the commencement of the earlier Brythonic invasion is equally uncertain. It is now provisionally fixed about 400 B.C.[1]

Ethnology of the invaders.

Classical writers are practically unanimous in describing the Celts as a tall stalwart people with fair or red hair; and physical anthropology confirms the general accuracy of their statements. But this science shows that the Celts, Goidelic and Brythonic, who successively invaded Gaul were mixed themselves, and that the population whom they found there were composed of two intermingled elements—a small dark people who resembled the older neolithic inhabitants of our own islands, and a short sturdy people, also dark but round-skulled, who began to enter Gaul in the Neolithic Age. Doubtless the Belgae as well as the earlier Brythonic invaders of Britain were an amalgam of all these elements, the tall red Celts whose ancestors had introduced the Celtic language into Gaul being the most conspicuous. But it is remarkable that although Strabo emphasizes the great stature of the Britons, such sepulchral evidence as we possess does not bear out his description. The skeletons of the Early Iron Age that have been exhumed in Britain are mainly those of small or middle-sized men, who to an untrained eye seem hardly distinguishable from the neolithic race, but whose skulls, although they too are long and narrow, generally differ from theirs in the sight of an expert. Even the skeletons that have been found interred with war-chariots are unlike those of the cemeteries of North-Eastern Gaul. Unfortunately the chariot-burials of Britain are very few: many of the later British interments of the Early Iron Age

[1] See *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 83, 98, and p. 240, *infra*. It has been pointed out (*Archaeologia*, lii, 1890, pp. 385–7) that the interments by inhumation which have been discovered at Arras in the East Riding of Yorkshire correspond with the Gallic interments of the fourth century before Christ. Since the Belgae appear to have practised cremation, these interments very likely indicate, what we know already, that there was a pre-Belgic Brythonic invasion of Britain: but it does not follow that they were contemporary with those of Gaul, and belonged to the time that immediately followed the close of the British Bronze Age; for, as we shall see (p. 286), inhumation persisted in Britain long after it had become obsolete on the other side of the Channel.

were made by cremation ; and it can only be concluded that the evidence which might have enabled us to recognize the Celtic conquerors of the classical type has perished or has not yet come to light.[1]

The order in which the various tribes arrived unknown.

Attempts, based upon the geographical positions of the various Brythonic tribes, as they were defined by Caesar, Ptolemy, and other ancient writers, have been made to determine the order in which they arrived. Thus it has been supposed that the Britanni, coming from the country near the mouth of the Somme, crossed the Straits and took possession of Kent ; that the Atrebates sailed up Southampton Water and pushed inland till they reached those parts of Hampshire and Berkshire in which they were afterwards found ; that the Trinovantes, who in Caesar's time occupied Essex, steered for the mouth of the Thames ; that the Catuvellauni, arriving a little later, were obliged to move higher up the valley and content themselves with parts of Middlesex, Hertfordshire, and Oxfordshire ; that the Eceni, whose settlements were in East Anglia, came later still ; and after them the Coritani, who dwelled beyond the Wash, the Parisi, who seized the region of the Humber, and the Brigantes, who held the greater part of Yorkshire and Durham. The Cornavii of Cheshire and Derbyshire, whose name seems to mean the inhabitants of the horn or peninsula, are accordingly assumed to have landed between the Mersey and the Dee. Last of all, we are told, came the Votadini, who took to themselves the tract between the Tyne and the Firth of Forth.[2]

It would be surprising if these conjectures did not attain some measure of truth ; but those who will not accept guesses even from the highest authority without testing them will perceive that they bristle with difficulties. It is not certain that the obscure Britanni, who are known to history only as a Gaulish tribe and are not even mentioned by Caesar, ever invaded Britain at all : the same writer who tells us that they were the first comers tells us also that they were Belgic, and that the Belgae were preceded by other

[1] See my article on the ethnology of Britain (pp. 428–45, *infra*).

[2] J. Rhys, *The Welsh People*, 3rd ed., 1902, pp. 111–3.

Brythons ;[1] and the Belgae, although they were last in the field, were not forced to seek distant abodes, but conquered the best parts of the country which were nearest to the Continent. We know nothing and can learn nothing of the history of the Belgic or the earlier Brythonic settlements.

'Late Celtic' art.

The Brythonic invaders introduced the first beginnings of the so-called Late Celtic art, which, remotely connected with that of Central and Southern Europe, attained its highest development in the British Isles. It was partly an outgrowth of the culture which on the Continent is called after the Helvetian settlement of La Tène, a village built on piles in a bay of the lake of Neuchâtel. This culture, which owed much to that of Hallstatt, has also been traced to classical and even to Oriental sources ; but in the century which preceded the Roman conquest of Britain, while the Continent was dominated by the influence of Rome, its offspring asserted its own individuality.[2] The Belgic conquest, which brought Britain into closer connexion with the Continent, gave a powerful impetus to the spread of Late Celtic art. The study of its details and of the evolution of its various types belongs to archaeology ; but a general knowledge of its main features is essential to the understanding of British history.

Late Celtic works of art are in general as easily recognized as those of the Bronze Age, although only an expert could assign a given specimen to its proper period ; but they are far more difficult to describe. While the chevron is the characteristic feature of the older culture, that of the younger is the curve. Rectilinear patterns, inherited from the Bronze Age, appear on many Late Celtic objects, but generally combined with those of curvilinear form.[3] Anthro-

[1] See pp. 459–60, *infra*.

[2] J. Anderson, *Scotland in Pagan Times,—the Iron Age*, 1883, pp. 172–3 ; *Archaeologia*, lii, 1890, pp. 360–4 ; *L'Anthr.*, iv, 1893, pp. 573–4 ; vii, 1896, p. 693 ; *Archaeol. Oxon*, 1895, p. 160 ; *Scotsman*, Dec. 14, 1895, p. 7, col. 6, Dec. 17, p. 7, col. 3, Dec. 19, p. 6, col. 5 ; *Report of . . . the Brit. Association*, 1896, p. 921 ; *Rev. de synthèse hist.*, iii, 1901, pp. 40–1 ; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 12, 16–23, 29 30, 103 ; *Rev. des études anc.*, viii, 1906, p. 119.

[3] J. Romilly Allen, *Celtic Art*, pp. 60, 143–4, 159.

pomorphic and zoomorphic designs occasionally occur; and although the examples which best illustrate this tendency—two bronze-mounted buckets found at Marlborough[1] and Aylesford[2]—were imported from Gaul, a bronze shield, dredged up from the river Witham, which is decorated with the figure of a boar, was undoubtedly of British workmanship.[3] Geometrical designs are associated with representations of natural forms; and in certain cases one may see the latter becoming so conventionalized that they are tending to pass into the former. The scroll-like curves which hang from the mouths of the pair of confronted animals on the Marlborough bucket represent twigs on which they are supposed to have been browsing: certain scabbards are embellished with undulating curves, of which the original motive was an attempt to depict foliage; and everywhere the effect of successive copying was to transmute forms suggested by nature into sinuous lines, the origin of which is veiled by their very beauty. The ultimate result was a system of decoration which has been likened to the *flamboyant*,—the flame-like tracery of decadent French Gothic architecture.[4]

Coral and enamel.

The Late Celtic artist was not content with merely devising graceful lines on metal, wood, or earthenware: he often adorned his creations with coral and enamel. Coral, which was imported from the islands of Hyères, was no longer used in Gaul after the middle of the third century before our era; but in this country it remained in vogue until a much later period.[5] The art of enamelling, which had been practised long before in the Caucasus, was already known in Gaul before coral fell into disuse. The centre of the industry was the Aeduan town of Bibracte, on Mont Beuvray near

[1] *Archaeologia*, lii, 1890, pp. 371–3.

[2] *Ib.*, pp. 360–70, 374–5.

[3] A. W. Franks, *Horae ferales*, 1863, pl. xiv, fig. 1; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 93.

[4] J. Romilly Allen, *Celtic Art*, pp. 144–51.

[5] *Rev. celt.*, xx, 1899, pp. 13–29, 117–31; *Congrès internat. d'anthr. et d'archéol. préhist.*, 1900 (1902), p. 417; *Rev. arch.*, 4e sér., ii, 1903, p. 368; *Proc. Soc. Ant.*, 2nd ser., xx, 1904–5, p. 214; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 87, 105, 108.

Autun, where the crucibles, moulds, and polishing-stones of the workers have been discovered; but the enamellers of Britain elaborated the art to a far higher pitch of perfection. Enamels of many colours were produced at a late stage, but in pre-Roman times only red.[1] Originally, as on a bronze helmet found in the Thames by Waterloo Bridge, the enamel was let into parallel or crossed grooves scored on the surface of the metal;[2] but afterwards, by the *champlevé* process, a bed was scooped out for the reception of the fused material, and thus, by the covering of larger surfaces, the brilliancy of the effect was enhanced. The earlier British enamels, which show no vestiges of Roman influence, are found principally upon bridle-bits and harness-rings.[3]

Swords and scabbards.

But Late Celtic art may be studied on many other objects besides those which have been already mentioned. Though British swords of the Early Iron Age are rare, and belong for the most part to dates subsequent to the Belgic invasion, a beautiful specimen of La Tène type was found in its bronze sheath in the village-stronghold of Hunsbury near Northampton;[4] and several have been recovered from the Thames, the scabbard of one being ornamented with a basket-pattern and open-work and an S-shaped scroll, another with transverse bars like examples from La Tène and Somme Bionne.[5] Late Celtic swords, which invariably had bronze handles,[6] were not, like those of the Bronze Age, leaf-shaped: their edges were nearly straight, and only tapered slightly near the point. Some late specimens, more than three feet

[1] *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 90–2; J. G. Bulliot, *Fouilles du mont Beuvray*, 1899, i, 123–6, 129–46; ii, 3–44.

[2] *Proc. Soc. Ant.*, 2nd ser., iii, 1864–7, pp. 342–4; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 88–95. Cf. Diodorus Siculus, v, 30, § 2; Herodian, iii, 14, § 7; J. Evans, *Coins of the Anc. Britons*, p. 330; and *Vict. Hist. of... Hertford*, i, 236. Helmets, as Sir J. Evans says, 'could never have been in general use in Britain'; and the only two British specimens that have come to light are not earlier than the first century of our era.

[3] J. Anderson, *Scotland in Pagan Times,—the Iron Age*, p. 125; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 88, 90.

[4] *Reports Architect. Soc. of ... Lincoln*, &c., xviii, 1885–6, p. 58; *Archaeologia*, lii, 1890, pp. 761–2.

[5] *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 95–6.

[6] Sometimes the bronze covered some other material,—probably wood (*Archaeologia*, xlv, 1880, p. 263).

long and with blunt points, intended not for thrusting but cutting, correspond to the description of Tacitus;[1] but others are much shorter. A dagger-sheath, found in Oxfordshire, is noticeable for its unusual decoration,—minute punched ornament between two pairs of ribs, which follow the outline of the edge, and not a single curve;[2] while a scabbard from the Thames at Wandsworth is adorned with mock spirals and lozenges enclosed between parallel ribs.[3]

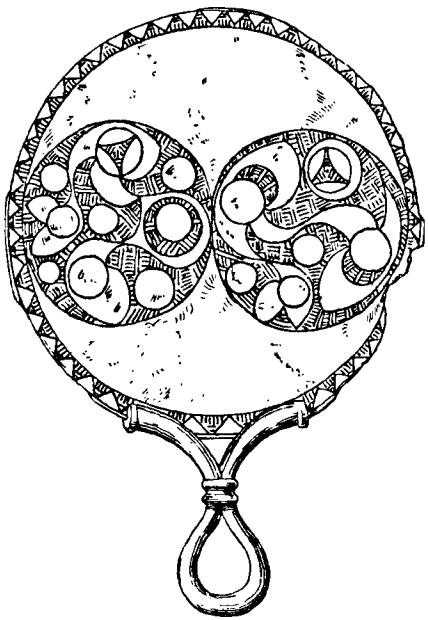

FIG. 37. ½

Mirrors.

The reader who has been taught to regard his British forefathers as savages would not expect to find that they used mirrors; but although some of those whose pre-Roman age is certain are quite plain, a beautiful specimen which was found at Trelan Bahow in Cornwall, where to the last Roman influence was hardly felt, is probably representative of many which were made in the century before the Roman conquest, even though its own date may be later than the time of Claudius. Unlike the primitive mirrors, which were of iron mounted with bronze, it is made entirely of the brighter metal, and ornamented on the back with three circles, which

[1] *Agricola*, 36.

[2] *Archaeologia*, liv, 1895, p. 498.

[3] *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 98. See also, in regard to swords, *Archaeologia*, xviii, 1817, p. 341; xlv, 1880, pp. 251-66; J. Evans, *Anc. Bronze Implements*, p. 275; *Archaeol. Journal*, xxxix, 1882, p. 442; and *Vict. Hist. of . . . Essex*, i, 268.

enclose patterns of engraved scroll-work, filled with cross-hatching.[1]

Brooches and pins.

The *fibula* or brooch—the prototype of the modern safety-pin—which had come into use on the Continent in the earliest period of the Hallstatt culture, was not known in our island before the Iron Age. Brooches of the successive La Tène types, in all of which the pin was straight and the body curved like a bow, have been found in considerable numbers; one of the earliest, from Water Eaton in Oxfordshire, being engraved with scrolls and the familiar ring-and-dot pattern, while another, from Avebury, was set with coral.[2] Some brooches discovered in the stronghold of Hod Hill, near Blandford, had been modelled upon an Italian pattern of much earlier date.[3] Pins, however, were still used for fastening the dress. Plain ones, which may be as old as the fourth century before Christ, have been found at Hagbourne Hill in Berkshire, and on the site of a pile-dwelling at Hammersmith, and others, which are hardly distinguishable in shape from a modern scarf-pin and belong to the period immediately preceding the coming of the Romans, in various parts of Scotland;[4] but one which lay among the relics in a grave near Driffield was far more elaborately designed, its head being a miniature chariot-wheel with four spokes, curiously inlaid with shell.[5]

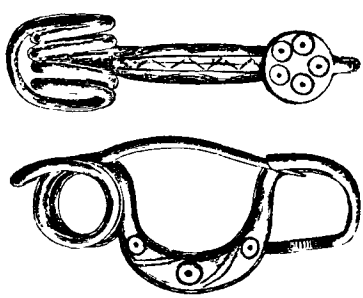

FIG. 38. ½

[1] *Archaeologia*, xl, 1866, pp. 502, 510; W. Greenwell, *Brit. Barrows*, pp. 454–5; J. Romilly Allen, *Celtic Art*, pp. 115–6; *Proc. Soc. Ant.*, 2nd ser., xx, 1904–5, p. 214; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 57, 127.

[2] *Ib.*, pp. 100, 127; *Crania Britannica*, ii, pl. 6 and 7, pp. 3–4; *Archaeologia*, lii, 1890, p. 382; *Archaeol. Oxon.*, 1892–5 (1895), p. 163; *Congrès internat. d'anthr. et d'archéol préhist.*, 1900 (1902), p. 423.

[3] *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 99. Cf. A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 117–8.

[4] *Proc. Soc. Ant.*, 2nd ser., xx, 1904–5, pp. 344–7.

[5] *Ib.*, xvii, 1897-9, p. 120.

Of our Late Celtic ornaments many are undatable; and while the torques and richly decorated collars which are familiar to all antiquarians are common in early Gaulish graves, those of this country which are most characteristic of Late Celtic art appear to belong to the Roman period:[1] but bronze bracelets set with paste were worn even in Yorkshire; and a penannular bracelet with small tooth-like projections, which closely resembles far earlier specimens from Hallstatt, belongs to the same district.[2] Of less costly trinkets lathe-turned bangles of Kimmeridge shale,[3] glass armlets,[4] and glass beads[5] can hardly perhaps be classified as works of art; but it is noteworthy that the beads, yellow, green, and blue, with their zigzag patterns and wavy white lines, which have been found at Glastonbury and in Yorkshire barrows, are utterly different from those of the Bronze Age, and belong mainly to a late period of the La Tène culture, though some had analogues in the cemetery of Hallstatt. As Glastonbury has also yielded pieces of glass slag and of crucibles, the beads were probably manufactured on the spot.[6] For some reason which has not been explained gold ornaments were apparently far rarer both in this country and in Gaul than in the preceding period.[7] Ornaments.

Among the finest examples of woodwork are bronze-mounted tankards which have been found in Suffolk[8] and Woodwork.

[1] *Archaeol. Journal*, iii, 1846, pp. 27–38; J. Anderson, *Scotland in Pagan Times,—the Iron Age*, pp. 131, 135–6; *Archaeologia*, liv, 1895, pp. 495–6; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 55–6, 137–8. A plain bronze torque was found on the neck of a skeleton in a grave at Arras in the East Riding of Yorkshire (*Crania Britannica*, ii, pl. 6 and 7, pp. 1–2) and a plain iron one on another (*Guide to the Ant. of the Early Iron Age* [Brit. Museum], p. 138).

[2] *Crania Britannica*, ii, pl. 6 and 7, pp. 3–4; W. Greenwell, *Brit. Barrows*, p. 210. Cf. *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., iv, 1904, pp. 80–4.

[3] *Proc. Somerset. Archaeol. and Nat. Hist. Soc.*, li, 1905, pp. 97–8.

[4] *Hist. Berwickshire Naturalists' Club*, 1856–62 (1863), p. 307.

[5] *Crania Britannica*, ii, pl. 6 and 7, p. 3; *Archaeologia*, xliii, 1871, pp. 475, 497; W. Greenwell, *Brit. Barrows*, p. 208; J. Evans, *Anc. Bronze Implements*, pp. 134–5; *Proc. Somerset. Archaeol. and Nat. Hist. Soc.*, li, 1905, p. 102.

[6] *Vict. Hist. of . . . Somerset*, i, 198.

[7] *Man*, vi, 1906, No. 63, p. 96; *L'Anthr.*, xvii, 1906, pp. 130, 137.

[8] *Archaeologia*, lii, 1890, pp. 358–9.

Merionethshire,[1] the former being ornamented with circles enclosed between bronze bands, and each containing the mystic three-limbed figure, called the *triskele*, which seems to have been akin to the swastika; while the handle of the latter is notable for its flamboyant tracery. Specimens of a different kind include a beautiful bowl from Glastonbury, the sweeping curves incised on its surface expanding into circles and trumpet-like projections which enclose diagonal cross-hatching, and a rectangular object from the same site,

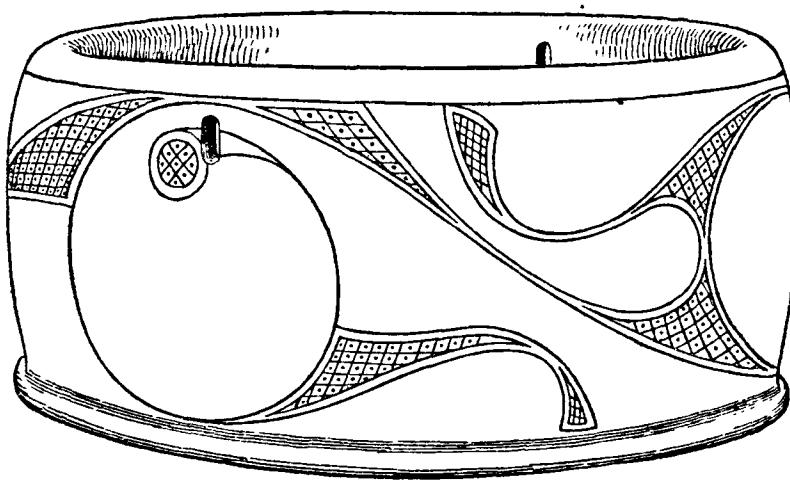

FIG. 39. ¼

which has no curves but is engraved with a step-like pattern shaded with cross-hatching of double diagonals.[2]

Pottery.

Not less interesting is the Late Celtic pottery, which is generally very different from that of the Bronze Age, and the distinctive forms of which were first classified a few years ago by the explorer of the cemetery at Aylesford. Since then numerous examples of the same types have been found in other parts of Kent and in Essex; but the influence was felt as far north as Northamptonshire, and as far west as Dorsetshire. These vessels were turned upon the wheel and were much finer in quality than those of the Bronze Age.

[1] *Archaeol. Cambr.*, 5th ser., xiii, 1896, pp. 213–6.

[2] J. Romilly Allen, *Celtic Art*, pp. 126, 147, 160.

The most characteristic of the cinerary urns, which in outline may be likened to a truncated pear, stand upon narrow pedestals and are generally divided into zones by ridges and corresponding grooves ; while a few are incised on the bottom with concentric circles. They closely resemble urns found in Belgic cemeteries near St. Valéry-sur-Somme and in the lower valley of the Seine, which are nearly contemporary with them, belonging to the latest period of Gallic independence ; but vases of the same form had been deposited three centuries earlier in the cemetery of Somme-Bionne, where the bodies had all been simply interred, whereas the urns of Aylesford were filled with cremated bones. The type, however, was not indigenous in Gaul. Its descent has been traced to vessels of earthenware found in North Italian graves of the fifth and fourth centuries before Christ, which were in their turn

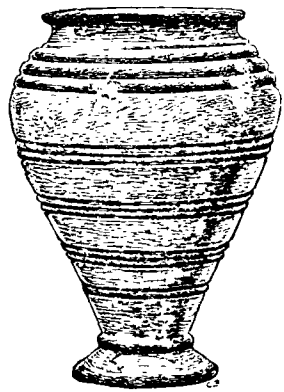

FIG. 40. $\frac{1}{8}$

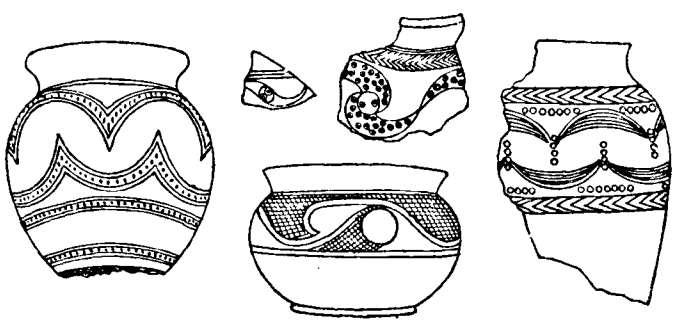

FIG. 41.

derived from bronze vases common on both shores of the Northern Adriatic. The cordons on the bronze vessels were simply survivals of wooden rings that compacted a frame of staves to which metal plates had been riveted.[1]

[1] *Journ. Brit. Archaeol. Association*, xlv, 1889, p. 81 ; *Archaeologia*, lii, 1890, pp. 328–31, 333–4, 340–1, 343, 344–6, 350–5 ; *Essex Naturalist*, xiii, 1903, pp. 110–2 ; *Proc. Soc. Ant.*, 2nd ser., xvi, 1895–7, pp. 258–60 ; xx, 1901–5,

Pedestalled vases were not the only pottery found at Aylesford and the analogous sites. There were others, bowl-shaped or with low globular bodies, some of which were also cordoned, while a few had the triangular decoration characteristic of the Bronze Age.

Domestic vessels of wholly different forms have also been recovered, some with handles on either side, and perforated bases, which were perhaps used for draining honey-combs, and others which are more easily recognized as Late Celtic by their flamboyant decoration. A fragment of this ware was taken from the same cavern near Torquay which had been used as a dwelling-place in palaeolithic times. Household pottery was still commonly made by hand; and while some specimens were without any ornament, others had rectilinear patterns of such a kind that, but for the associations in which they were found, they would have been referred unhesitatingly to the Age of Bronze.[1]

The noblest creation of Late Celtic art.

If archaeologists were invited to name the noblest creation of Late Celtic art, I think that with one consent they would point to the bronze shield which was lost in the Thames, and found after it had lain there some nineteen hundred years. Oblong with rounded ends and gently contracted in the middle, the outline forming an endless curve, it is adorned with three successive circles of *repoussé* work, a large central one and two smaller, connected by sinuous lines, within which lesser circles are contained. The central piece of each greater circle is a boss enclosing enamelled swastika designs and surrounded by curves, S-shaped and C-shaped, which begin and end with the same mysterious device. Yet, though the beauty of form remains, the glory of colouring is gone; and one can only now imagine how, when the shield hung upon the forgotten warrior's arm, gleaming bronze and raised

p. 212; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 49, 66–8, 117–8, 122, 140; *Journ. Anthr. Inst.*, xxxv, 1905, p. 393. An urn figured in *26th ann. report Roy. Inst. Cornwall*, 1844 (1845), p. 22, appears to me to be of the Aylesford type.

[1] *Archaeol. Oxon.*, 1892–5 (1895), p. 163; *Archaeol. Cambr.*, 6th ser., iii, 1903, p. 11; *Report of . . . the Brit. Association*, 1904 (1905), p. 329; *Proc. Somerset. Archaeol. and Nat. Hist. Soc.*, li, 1905, pp. 100–1; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 67, 141–2.

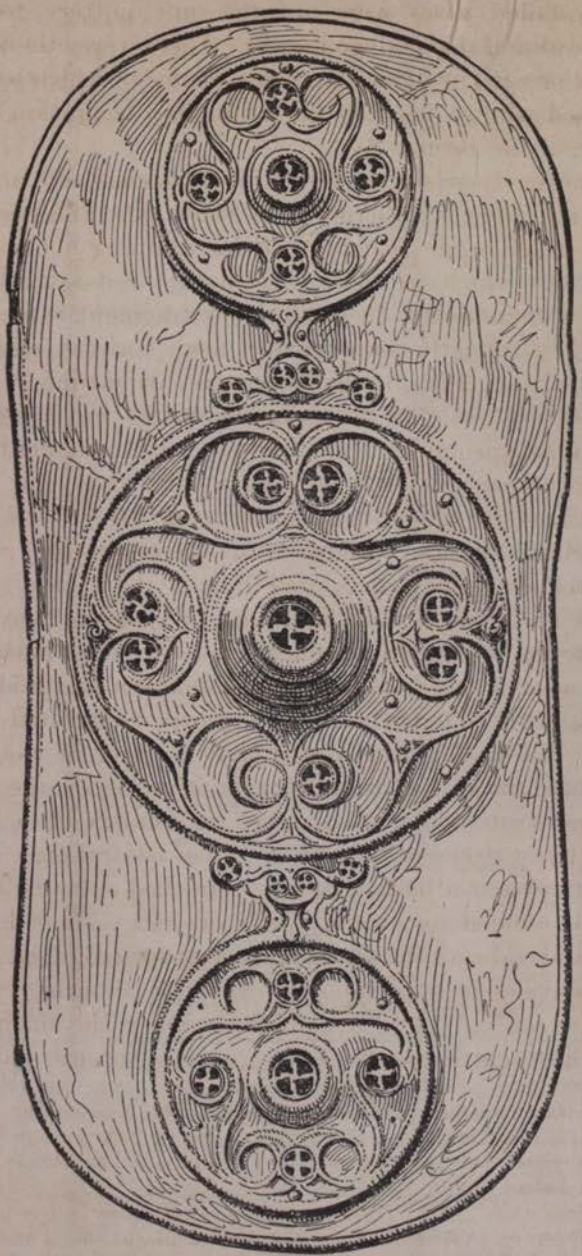

FIG. 42. $\frac{1}{6}$

curves and red enamel combined to produce their due effect. Like Stonehenge this was the work of a master: not one detail could be altered, or removed, or added without impairing its perfection.[1]

Imported objects of art.

Among the products of Late Celtic art that have been found in Britain are some of foreign manufacture, which testify to the increased commercial activity that followed the Belgic invasion. Besides the bronze-mounted bucket, already mentioned, the Aylesford cemetery yielded a bronze flagon, which had been made in Northern Italy:[2] an elegant Graeco-Italian two-handled cup of black glazed earthenware with white foliated ornament encircling its inner margin was discovered in the rick-yard of the Manor Farm at Dorchester in Oxfordshire;[3] while the Marlborough bucket is adorned with figures of sea-horses which are common on Gallic coins of the neighbourhood of Rennes, and which warrant the conjecture that it was imported from North-Western Gaul,[4] perhaps in one of the vessels that plied between the Loire and Ictis. What else besides tin the Britons in the days of their independence exported in return for such articles we do not know; but in a later chapter we shall see that a long list of their exports and imports was compiled by Strabo.[5] The carrying-trade was for the most part in the hands of Gallic ship-owners; but some cargoes were perhaps loaded in British bottoms. The British envoys who presented themselves in Caesar's camp in 55 B.C. may indeed have crossed the Channel in a Gallic merchantman, and so may the hostages who were sent to him after his first invasion of Britain; but it is unlikely that the maritime Belgic tribes, who must

[1] A. W. Franks, *Horae ferales*, pl. xv, fig. 1; J. Romilly Allen, *Celtic Art*, pp. 93–4. Although the known Late Celtic shields were oblong, long double-pointed shields and even round ones, which may have resembled those of the Late Bronze Age (p. 146, *supra*), are figured on gold coins belonging to the period between the invasions of Caesar and the Roman conquest (Tacitus, *Agricola*, 36; J. Evans, *Anc. Bronze Implements*, p. 354; *Vict. Hist. of . . . Hertford*, i, 239).

[2] *Archaeologia*, lii, 1890, pp. 376–7, 380.

[3] *Archaeol. Oxon.*, 1892–5 (1895), pp. 160–2.

[4] *Archaeologia*, lii, 1890, p. 373; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 29.

[5] See p. 357 *infra*.

have set out from Gaul in ships of their own, built none after they had settled in Britain, or that the numerous British adventurers who reinforced Caesar's Gallic enemies depended for their transport upon the latter. The only British vessels, however, which are expressly mentioned by our authorities were light coracles of lath covered with hides, which Caesar observed when he was in Kent and afterwards copied when he was fighting in Spain against Pompey's lieutenants,[1] and which are still used by Irish fishermen off the coast of Connaught.[2] These boats were doubtless employed in coastal navigation and on inland waterways; but much of the intertribal traffic must have been carried on along trackways, which are still traceable, and the prehistoric antiquity of which is proved by their association with hill-forts. Most of them, like the Pilgrim's Way, which is known to all who have tramped the high grounds of Surrey and Kent, ran along ridges or the slopes of downs which were generally unencumbered by forest or morass. If their origin could be traced, we should find that they were formed by the earliest settlers who felt the need of communication, along the lines of least resistance which nomadic hunters had followed when they passed from one temporary settlement to another;[3] and doubtless attempts were made to render them more suitable for wheeled traction when the Cornish miners began to convey their tin in wagons to the coast, and the invaders of the Iron Age brought their chariots from Gaul. Even then,

British ships and coracles.

Trackways.

[1] *B. C.*, i, 54, §§ 1-2.

[2] *Journ. Anthr. Inst.*, iv, 1875, p. 425.

[3] See G. Payne, *Collectanea Cantiana*, p. 129; *Archaeol. Journal*, lix, 1902, p. 217; lx, 1903, pp. 209-10; lxiv, 1904, pp. 309, 313, 318; *Vict. Hist. of . . . Surrey*, i, 249; and J. R. Mortimer, *Forty Years' Researches*, pp. 381-5. Ancient 'corduroy' roads, made of 'cross timbers laid side by side on three lines of supporting logs parallel to the direction of the road', have been discovered near Gilpin Bridge in Cumberland (*Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, N. S., iv, 1904, pp. 207-10); but their date cannot yet be fixed. It has been said that trackways were made (1) by digging two parallel ditches and throwing up the earth so as to form a bank between them, and (2) by digging one ditch and building the bank on one or on both sides. See *Proc. Dorset Nat. Hist. and Ant. Field Club*, xxi, 1900, pp. 105-6; *Trans. Birmingham and Midland Inst.*, xxv, 1900, p. 41; and *Vict. Hist. of . . . Berks*, i, 192.

however, wheel-less vehicles, like those which Sir Arthur Mitchell noticed a few years ago in Strathglass and Kintail, must have been used for carting timber down steep hills or over heaths where no wheeled carriage could have moved.[1]

Coinage.

Foreign commerce as well as domestic trade were greatly stimulated by the introduction of coinage and by the development of a ruder form of currency. Towards the end of the fourth century before the Christian era the Greeks of Massilia had introduced into Gaul gold coins of Philip of Macedon, which bore on the obverse a representation of the head of Apollo wreathed in laurel, and on the reverse a charioteer driving a pair of horses with the name *Philippos* stamped underneath. On these coins the Gallic coinage was modelled, and the British coinage was derived mainly from that of Gaul or through Gaul from a Macedonian stater; for certain peculiarities are noticeable on our earliest coins which distinguish them from those of Gaul.[2] Evidently a considerable time must have elapsed before the new art travelled from Southern to Northern Gaul, and again before it crossed the Channel; and it is only natural to find that the oldest and heaviest British coins weigh no more than a hundred and twenty grains, or thirteen grains lighter than the Philippus, although, on the other hand, they are heavier than Gallic coins which belong to the latter half of the second century before Christ.[3] Until about a quarter of a century after Caesar's invasion the British coins were uninscribed: indeed uninscribed coins were still current during the earlier years of the Roman

[1] *The Past in the Present*, p. 97 and figs. 70, 71, and 72. See also A. Pitt-Rivers, *Excavations in Cranborne Chase*, i, 78–9.

[2] J. Evans, *Coins of the Anc. Britons*, 1864, pp. 25–6, 47–8. See, however, A. Blanchet, *Traité des monn. gaul.*, 1905, pp. 478–9. Sir J. Evans (*Coins*, &c., Suppl., p. 424) admits that the Macedonian stater may not have been the sole progenitor of British coins. His son, Dr. A. J. Evans (*Archaeol. Oxon.*, 1892–5[1895]) affirms that 'Massalia, Rhoda, and Emporiae . . . each contributed their part', and that he has 'succeeded in tracing back . . . certain scrolls and outlines that appear on a class of late British coin-types that extend from Tewkesbury and Oxford, through Armoric and Iberic Gaul, and the Greek colonies beyond, still further . . . to the head of Persephonê on the medallions of Syracuse'.

[3] J. Evans, *Coins of the Anc. Britons*, pp. 26–8.

occupation.[1] Their weight gradually diminished; and gradually, owing to successive copying, the head of Apollo and his wreath, the charioteer, the chariot, and the horses became more and more conventionalized and degraded, the head in certain cases passing ultimately into a cruciform pattern or even into a four-leaved flower, the charioteer being evolved into pellets, and the pair of horses becoming first one, then more and more grotesque until it lost all resemblance to a quadruped. Die-sinkers (who were doubtless few) would use the same dies or follow the same general type during their working career; and new types appeared when their successors came to engrave new dies. By estimating the time which would have been required for these successive alterations, it has been calculated that the earliest British coins must have been struck about a hundred and fifty or perhaps two hundred years before the birth of Christ.[2]

For many years the only coins of Britain were gold of two values, the smaller being a quarter of the weight of the larger;[3] and it may be gathered from the testimony of Strabo[4] and Tacitus[5] that they were made, at all events in part, from metal extracted from the alluvial deposits of the Cornish peninsula. Coins of silver, bronze, and even tin were afterwards circulated, but probably not before the era of redoubled commercial activity which began when the British islands became more closely connected with the Continent in consequence of Caesar's invasion: indeed many of the silver coins are little earlier than the time of Claudius.[6] Specimens of all these metals are much scarcer than those of gold. Only two British tin coins are known to exist; and in the western counties no bronze coin has ever been found.[7]

Specimens of the prototype of British gold coins have been found more frequently in Kent than in any other

[1] *Num. Chron.*, 3rd ser., xvi, 1896, p. 184. M. A. Blanchet (*Traité des monn. gaul.* p. 75) believes that the inscribed coinage of Gaul dates from about 150 B.C.

[2] J. Evans, *Coins of the Anc. Britons*, pp. 25–6, 31, 38; *ib.*, Suppl., p. 423.

[3] J. Evans, *Coins of the Anc. Britons*, pp. 49–50, 69–70, 79, 81, &c.

[4] *Geogr.*, iv. 5, § 2. [5] *Agricola*, 12.

[6] J. Evans, *Coins of the Anc. Britons*, p. 33; Suppl, pp. 473, 484–6.

[7] J. Evans, *Coins of the Anc. Britons*, pp. 99, 116–7, 123, 133.

county; and it may be inferred that, as might have been expected, they were first struck in the more civilized district which was nearest to the Continent.[1] For a long period indeed the gold currency was confined to the southern and eastern districts: before Caesar's time there is no evidence that any tribes coined money except those whose territories lay south of a line drawn from the Wash to the Bristol Channel; and even from these the peoples of Gloucestershire, Northern Somersetshire, and Northern Wiltshire must probably be excluded.[2] Uninscribed coins have indeed occurred as far north as Yorkshire,[3] and as far west as Cornwall;[4] but they had found their way thither from other tribes.[5]

Many coins of British origin which have been discovered in France, especially in the Belgic territory,[6] and many Gallic coins in South-Eastern Britain, bear further witness to the development of international trade.[7]

Iron currency bars.

But coins were not the only medium of exchange. Caesar, in his description of the manners and customs of the Britons, remarked that some of them made use of iron bars of specified weights as a substitute for coins.[8] Until a very recent period antiquaries were waiting for some lucky find which might corroborate the accuracy of Caesar's statement, not knowing that the evidence was before their eyes and only

[1] J. Evans, *Coins of the Anc. Britons*, pp. 36–7.

[2] *Ib.*, pp. 35–6, 41.

[3] *Ib.*, Suppl., p. 434.

[4] J. Evans, *Coins of the Anc. Britons*, pp. 50–2, 62, 81, 94.

[5] *Ib.*, p. 40.

[6] *Ib.*, pp. 38, 51, 95–7.

[7] *Ib.*, pp. 62–5, 81–3; Suppl., pp. 442, 481–3; *Archaeologia*, lii, 1890, p. 327; A. Blanchet, *Traité des monn. gaul.*, p. 515.

[8] *B. G.*, v, 12, § 4.—*utuntur aut aere aut nummo aureo aut anulis ferreis ad certum pondus examinatis pro nummo.* So runs the passage in the British Museum Add. MS. 10084; but the Paris MS. 5764 has *taleis* (bars) instead of *anulis* (rings). 'The phrase *aut aere*,' says Dr. Haverfield (*Proc. Soc. Ant.*, 2nd ser., xx, 1904–5, p. 186), 'must be wrong, and the conjecture *anulis* in Add. MS. 10084 is plainly an attempt to explain *aliis*. As *aliis* is the reading of A and part of B [the two principal families of the MSS. of Caesar's *Commentaries*, generally quoted as α and β], and *taleis* of the rest of B, and *aut aliis* can hardly be other than a misreading of *aut taleis*, this latter may be accepted.' E. Hübner (Pauly's *Real-Encyclopädie*, iii, 1897, p. 864) accepts *aut aere*.

needed interpretation. Within the last eighty years a large number of iron bars have been unearthed in Berkshire, Northamptonshire, Worcestershire, Gloucestershire, Somersetshire, Dorsetshire, Hampshire, and the Isle of Wight. Many of them were found on well-known sites of the Early Iron Age, such as the lake-village of Glastonbury, and the forts of Hod Hill and Spettisbury; and some of the hoards comprised very numerous specimens—amounting in two cases to about one hundred and fifty, and in a third to three hundred and ninety-four—which had been buried deep in the ground. A tiro might take them for swords; but to the experts who compare them with the known swords of the Late Celtic Period it is evident that they contain too much metal; and, moreover, they may be arranged, according to their weight, in three groups, the heaviest being twice as valuable as the intermediate, and four times as valuable as the lightest. Not a single specimen has come to light in the eastern and south-eastern counties, in which coins are most abundant.[1]

Mining.

The British iron-mines of which Caesar speaks were situated in the Wealden Forest; and although they were not finally abandoned before the nineteenth century, it is probable that some of the pits which mark the site of the works were excavated by British miners.[2] But the iron from which some of the currency-bars were wrought was obtained, in the opinion of an eminent metallurgist, from the Forest of Dean,[3] and, as we shall presently see,[4] those which were found in Northamptonshire may have been manufactured on the spot. Mining indeed was one of the principal industries of Britain. Tin was still exported, if not in Caesar's time, at least as late as that of Posidonius;[5] copper was still needed for bronze ornaments, horse-trappings,

About 100 B.C.

[1] *Proc. Soc. Ant.*, 2nd ser., xx, 1904–5, pp. 179–91; *Class. Rev.*, xix, 1905, pp. 206–7. Iron bars were also used as currency by the Spartans, and are still so used by the natives of West Africa near Sierra Leone.

[2] *B. G.*, v, 12, § 5; *Trans. Internat. Congress of Prehist. Archaeol.*, 1868 (1869), pp. 185–90.

[3] *Proc. Soc. Ant.*, 2nd ser., xx, 1904–5, p. 194.

[4] See p. 260, *infra*.

[5] See p. 499, *infra*.

sword-sheaths, and other objects, and indeed in certain districts for cutting-tools ;[1] and although the numerous 'pigs' of lead which have been found in Staffordshire and Cheshire belong to the time of the Empire, the discovery of leaden celts and sword-pommels of the Bronze Age[2] raises the presumption that the mines of those districts, of the Mendip Hills, Flintshire, and the neighbourhood of Matlock may have been worked even by the Britons.[3]

Agriculture.

Looking at all these tokens of industrial enterprise, one is prepared to find evidences of increased comfort and more settled conditions of life. Since the Bronze Age agriculture had undoubtedly made a notable advance. It is impossible to tell whether the Britons, like the Gauls, recognized private property in land ;[4] but archaeology has furnished abundant evidence, which confirms Caesar's statement, that at all

[1] See p. 267, *infra*. Caesar's statement, that the Britons imported copper or bronze (*aere utuntur importato* [*B. G.*, v, 12, § 4]), has always been a puzzle. I doubt whether any scholar would now infer from it that the cakes of copper which have been found in bronze-founders' hoards were of foreign origin; but it has been suggested (*Guide to the Ant. of the Early Iron Age* [Brit. Museum], p. 86) that Caesar may have referred to articles of foreign manufacture such as the bronze flagon mentioned on p. 246, *supra*. See also J. Evans, *Anc. Bronze Implements*, p. 419.

M. S. Reinach (*Rev. celt.*, xxi, 1900, p. 173) infers from Caesar's observation that 'the industrial activity to which the relics of the Bronze Age testify had long ceased', and that there was an arrest, or rather a recoil of civilization. But, as we shall see hereafter (p. 267), the culture of the Bronze Age persisted in certain parts of Britain until the Roman conquest. Were the bronze implements that were used in those parts imported? If so, how could they have been paid for without industrial activity; and what conceivable reason can be suggested for the assumed paralysis? The industrial activity of the Early Iron Age in Britain is unquestionable; and I doubt whether any theory could be framed to account for a cessation, contemporaneous with the manufacture of iron, of the trade of the bronze-founder.

[2] See p. 148, *supra*.

[3] See J. Evans, *Coins of the Anc. Britons*, Suppl., p. 492; F. J. Haverfield, *The Romanization of Roman Britain*, 1906, pp. 20–1; and *Vict. Hist. of . . . Somerset*, i, 198.

[4] I have nothing to add to what I have already written on this question (*Caesar's Conquest of Gaul*, 1899, pp. 521–3) except to refer, in support of my conclusion, to *Rev. crit. d'hist. et de litt.*, nouv. sér., xxx, 1890, pp. 441–2, and E. Lavisse, *Hist. de France*, i, 1900 (by G. Bloch), p. 61, n. 2; and, for a very clear but hardly complete summary of the controversy, to M. G. Dottin's *Manuel pour servir à l'étude de l'ant. celt.*, 1906, pp. 184–6. Pasture land was not improbably common property both in Gaul and Britain. See W. Robertson Smith, *The Religion of the Semites*, 1901, p. 95.

events in the south-eastern districts corn was grown in plenty. When he made his first expedition to Britain, his army, numbering at least twelve thousand men, reaped enough wheat in the near neighbourhood of Walmer to supply its wants for a fortnight or more ; while in the following year he requisitioned from the people of Essex grain for four legions with their auxiliaries and seventeen hundred cavalry, which was delivered within a few days.[1] An iron sickle and a ploughshare found in Bigbury camp near Canterbury ;[2] traces of terrace cultivation on the Sussex downs ;[3] grain of several kinds stored in Worlebury Fort, in the Glastonbury lake-village, and in Hunsbury, where also were found fragments of stone querns in such profusion that every family may well have possessed its own, bear witness to the industry of the British farmers.[4] So also perhaps do the famous dene-holes of Kent, Essex, and Norfolk, whose purpose has been a theme of voluminous controversy, but of which the most satisfactory explanation seems to be that they were for the most part subterranean granaries, which may have been used as refuges in time of danger, and that the chalk extracted in the process of excavation was used, as Pliny says, for manuring fields.[5] Under the necessity of cultivating fresh land considerable progress must have been made in clearing the forests ; and axes, saws, and bill-hooks, with which the woodmen worked, are still to be seen.[6] It is true that even in the more civilized south the great Wealden Forest, in which swine, guarded by fierce dogs, fed secure among wolves and foxes, badgers, and deer, still extended beyond the chalk downs from the neighbourhood of West

[1] See pp. 339, 346, *infra*.

[2] *Archaeol. Journal*, lix, 1902, pp. 213–6.

[3] *Archaeologia*, xlvi, 1881, p. 422. Cf. A. Pitt-Rivers, *Excavations in Cranborne Chase*, iii, 4–6.

[4] *Archaeologia*, xlvi, 1881, p. 451 ; *Reports Archit. Soc. of . . . Lincoln*, &c., xviii, 1885–6, p. 61 ; C. W. Dymond and H. G. Tomkins, *Worlebury*, 1886, pp. 69, 78.

[5] *Nat. Hist.*, xvii, 6 (4), § 42 ; 8, § 45. Cf. Varro, *Rerum rust.*, i, 7, § 8. See pp. 515–7, *infra*.

[6] *Archaeologia*, xlvi, 1881, pp. 438–9 ; *Archaeol. Cambr.*, 5th ser., xiii, 1896, pp. 238–9 ; *Archaeol. Journal*, lix, 1902, pp. 213–6 ; *Reports Archit. Soc. of . . . Lincoln*, &c., xviii, 1885–6, p. 60.

Hythe to the eastern border of Hampshire, reached northward as far as Sevenoaks, and skirted the Surrey Hills; while great parts of Essex were overgrown with wood; another forest overshadowed the valley of the Kennet from Hungerford to Windsor; and the Isle of Ely was surrounded by broad meres, swelled by the heavier rains which fell in those days.[1] But even in Essex much timber must have been removed to make room for the cornfields from which the Trinovantes supplied Caesar's legions, and in Kent to form the denes in which cattle grazed; while of those myriad homesteads which Caesar passed on his devastating march not a few must have been built upon reclaimed land.

Dwellings of the rich.

The researches of the eminent scholar who has so greatly enlarged our knowledge of Roman Britain have led him to suggest that among these homesteads there may have been, besides the round Celtic huts, dwellings, belonging to the rich, which might almost be described as country houses. Under Roman administration the rural parts of Britain, as of Northern Gaul, were parcelled into estates, the owners of which let out the greater part to cultivators who were in a state of semi-serfdom, while their demesne lands were tilled by slaves. The houses belong to two types, known as the Corridor type and the Courtyard type, neither of which exists anywhere save in Britain and the north of Gaul. The corridor house consisted of a row of rooms with a passage running along them: the other of three such rows, which formed three sides of a quadrangle. Since there is little resemblance between either of these types and those of Italy, it may be assumed that the extant examples of both, although they had been made luxurious by Roman mosaics and hypocausts and baths, were but modified representatives of the chieftains' houses which Caesar saw.[2]

Towns.

Nor were petty hamlets and isolated cottages the only places of abode. Town-life was beginning to emerge. The

[1] W. Holloway, *Hist. of Romney Marsh*, 1849, pp. 10–1; C. H. Pearson, *Hist. Maps of England*, 1870, pp. 4–5; R. Furley, *Hist. of the Weald of Kent*, i, 1871, p. 387, and map facing p. 26; *Journ. Brit. Archaeol. Association*, N. S., iii, 1897, p. 36; *Archaeol. Journal*, lx, 1903, p. 157. Cf. A. Pitt-Rivers, *Excavations in Cranborne Chase*, i, 27, ii, 56, iii, 3.

[2] *Vict. Hist. of . . . Hants*, i, 268–9; *ib., Somerset*, i, 213–4.

Britons, like the Gauls, had large fortified villages, which afterwards gave place to the flourishing Romano-British towns whose secrets are being revealed by pick and shovel. Camulodunum, or Colchester, the chief town of the Trinovantes, and Verulamium, hard by St. Albans, the chief town of the Catuvellauni, each of which had its mint before the Roman conquest, were doubtless tribal centres before Caesar came.[1] So too, probably, was Corinium, the capital of the Dobuni, which stood upon the site of Cirencester;[2] and Calleva, now Silchester, the excavation of which has been pursued for many years with illuminating results, was surrounded by a rampart which had evidently defended the capital of the Atrebates in pre-Roman times.[3] London, which, if we may trust Ptolemy,[4] was in the territory of the Cantii, was probably not less ancient; for *Augusta*, the name which Roman officialism endeavoured to impose upon it, was unable to resist the vitality of the Celtic appellation.[5] Imaginative historians have pictured British London in the midst of a vast lagoon;[6] but although the site of Westminster Abbey was an island surrounded by a marsh, and the Walbrook, where it flowed into the Thames, was little less than a hundred yards in width, it was proved during the construction of a sewer in London Wall that the land on the north side of the city had in Roman times been as dry as it is to-day.[7]

Hill-forts.

The tribal capitals were of course fortified; but the old hill strongholds of the Neolithic Age and the Bronze Age had not been abandoned; and new ones were doubtless constructed as occasion required. Among those that have yielded remains of the Late Celtic Period the most famous are Worlebury, which crowns a headland just north of Weston-super-mare; Hod Hill, which rises sheer above the

[1] J. Evans, *Coins of the Anc. Britons*, pp. 291, 338. See pp. 358–9, *infra*.

[2] *Archaeol. Journal*, lxii, 1905, p. 265.

[3] *Ib.*, li, 1894, p. 338.

[4] *Geogr.*, ii, 3, § 12. Cf. *Archaeologia*, xlviii, 1885, map facing p. 380.

[5] See *Vict. Hist. of . . . Warwick*, i, 227, and pp. 704–5, *infra*.

[6] *Archaeol. Journal*, xlii, 1885, pp. 274, 300–2. See also pp. 272 n. 1, 275–6, 297.

[7] *Ib.*, lx, 1903, pp. 155–6, 174.

valley of the Stour, four miles north-west of Blandford; Bigbury Camp, through which runs the Pilgrim's Way; and Winkelbury Camp in South Wiltshire, Mount Caburn, overhanging Lewes, and Cissbury Camp, already mentioned for its neolithic factory, which have been excavated by General Pitt-Rivers. Worlebury is the most remarkable of the few stone forts in the west of England. Unlike the great earthworks it has no ditch, because it needed none; and on its northern side a limestone precipice rendered fortification superfluous. The rampart is a vast wall, compacted with rubble and faced on either side with dry masonry; and, to prevent an enemy from demolishing it, the outer face was buttressed by heaps of loose stones. Many of the modern walls in the neighbourhood of the fortress are indistinguishable from it in structure.[1] At Winkelbury large openings were left in that part of the rampart which is contiguous to the plain, probably to enable cattle to be driven in rapidly when marauders were near; while another rampart, which bisects the camp, may have been designed to separate the cattle-pound from the quarters of the garrison.[2] Cissbury, the principal fort on the Sussex Downs, was one of the few British strongholds which appear to have had access to a permanent supply of water: about a mile and a half off, at a place called Broadwater, is a spring, abundant enough for an army, which is connected by a trackway with the southern entrance.[3] The most characteristic feature of Mount Caburn is the number of pits which, as at Worlebury, are contained within its area. In both camps these pits are so small that they could not have been ordinarily inhabited, although, during a siege, they might have afforded shelter: probably they were used as store-rooms, for some of them contained corn.[4] Dwellings, however, were connected with them; for the remains of a clay wall were discovered on

[1] *Journ. Brit. Archaeol. Association*, xxxi, 1875, pp. 266–75; C. W. Dymond and H. G. Tomkins, *Worlebury*, 1886, pp. 8 n. 3, 19–23, 29 n. 19, 50 § 4, 67 § 45, 69, 78; *Proc. Somerset. Archaeol. and Nat. Hist. Soc.*, li, 1905, pp. 17–28.

[2] A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 233–46.

[3] *Archaeologia*, xlii, 1869, p. 46.

[4] A few pits like those of Mount Caburn, and containing similar relics, were found at Cissbury and Winkelbury (*Archaeol. Journal*, xli, 1884, p. 76).

Mount Caburn, impressed with marks of wattle-work; and it may be inferred that many such huts, which have left no trace, once existed within the ramparts.[1] Bigbury was probably one of the entrenchments of which Caesar was thinking when he said that 'the Britons apply the term fortress to woods difficult of access and fortified with rampart and trench in which they are in the habit of taking refuge from a hostile raid'.[2] The familiar sentence was a stumbling-block to Pitt-Rivers; for, as we have seen, the British forts were as a rule constructed upon treeless heights, and the presence of trees upon the slopes would have been incompatible with the designs of the engineers: but Caesar's observations must of course be accepted; and we can only suppose that the entrenchments which he described were exceptional even in the region which was the theatre of his campaign.[3] May we conjecture that they had been erected in the Iron Age by Celtic immigrants, and that their lack of finish was due to the lazy shrinking from the hard labour of fortification which Caesar regarded as characteristic of the Gauls?[4]

The fort of Pen-y-Gaer, which overlooks the valley of the Conway, is remarkable as an almost unique specimen of ancient military engineering. A storming-party which had succeeded in passing the two outer ditches would have fallen, in attempting the next, under the missiles that showered from the rampart, on to *chevaux de frise* of pointed stones.[5]

Some permanently inhabited.

The relics that have been collected from the hill-forts of the Iron Age prove that the forts themselves, like those of Gaul, were not merely places of refuge but permanent abodes. Those that were situated on heights extremely difficult of access or remote from water were of course very sparsely inhabited in time of peace; but others were analogous to

[1] *Archaeologia*, xlii, 1869, pp. 39, 48–50; xlvi, 1881, pp. 450–1, 456–8.

[2] Oppidum autem Britanni vocant, cum silvas impeditas vallo atque fossa munierunt, quo incursionis hostium vitandae causa convenire consuerunt. *B. G.*, v, 21, § 3.

[3] See p. 136, *supra*, and *Archaeologia*, xlvi, 1881, p. 458.

[4] *B. G.*, vii, 30, § 4.

[5] *Archaeol. Cambr.*, 6th ser., vi, 1906, pp. 266–7. Two forts with defences of this kind are known in Peebles-shire.

the Gallic fortresses which Caesar called *oppida*, and which were evidently distinct from the refuges, such as Aduatuca, which he designated as *castella*.[1] Pottery, it is true, would have been indispensable even during a few days' siege; and the stone lamp, resembling that of Grimes's Graves,[2] and blackened by use, which was recovered from Castle Law in Perthshire,[3] might well have been needed at such a time. But when we find bill-hooks, ploughshares, bridle-bits, and fragments of querns among the objects that had been left in the forts which have been mentioned, it is clear that they were occupied by an industrial population: iron slag, which lay among the deposits on Hod Hill, was evidence of metallurgy; while the loom-weights which were collected on the same spot, the bone weaving-combs of Cissbury and Mount Caburn, and the spindle-whorls which abounded not only in these comparatively civilized settlements but also in a stone fortress on far St. David's Head show that among the inhabitants were women who pursued their ordinary domestic avocations.[4] This Welsh stronghold was almost identical in construction with Carn Brea,[5] and the hut-circles which the two contain are exactly alike; yet the time which had elapsed since the Cornish ramparts were thrown up was as long as that which separates us from Alfred the Great.[6]

Although many of the Scottish forts can be referred to the Early Iron Age, it would perhaps be impossible to prove that the relics found in any of them were earlier than the time of Caesar's invasion;[7] but two have an interest of their own as being the only examples that have yet been observed in Britain of fortifications constructed, like the Gallic walls

[1] *B. G.*, ii, 29, § 2; vi, 32, § 4. Cf. *Mém. de la Soc. nat. des ant. de France*, 4e sér., ii, 1871, pp. 141–2.

[2] See p. 70, *supra*.

[3] *Proc. Soc. Ant. Scot.*, xxxiii, 1899, pp. 29–30

[4] See p. 138, *supra*, and also *Archaeologia*, xlvi, 1881, pp. 438–9, 467; A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 238–9; *Archaeol. Cambr.*, 5th ser., xvi, 1899, pp. 106–8, 130; xvii, 1900, pp. 189, 195, 206, 209; *Archaeol. Journal*, lvii, 1900, pp. 52–6, 60–3, 66–7; *Journ. Roy. Inst. Cornwall*, xvi, 1904, pp. 73–83; and *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 122–4.

[5] See p. 134, *supra*.

[6] *Trans. Hon. Soc. Cymmrodorion*, 1898–9 (1900), p. 20.

[7] See *Proc. Soc. Ant. Scot.*, xxix, 1895, pp. 131, 149–50.

which he described,[1] conjointly of timber and stone. In one of them, situated at Burghead near Elgin, wooden logs were actually discovered in the stone walls;[2] while at Castle Law, which stands upon a hill commanding a view over the Tay, as it winds through the carse on the west and loses itself in its eastern estuary, the outer face of the wall contained rectangular openings, which had manifestly been designed for the reception of beams.[3]

Hunsbury.

While the hill-forts were probably only inhabited permanently by comparatively small numbers, and, like Gergovia, the mountain-city of Auvergne, where Vercingetorix defeated Caesar, may have sheltered thousands of fugitives in time of need, one stronghold at least of the other group was a town in the strictest sense of the word. Hunsbury, the most celebrated representative of this class, which

[1] *B. G.*, vii, 22.

[2] *Proc. Soc. Ant. Scot.*, xxv, 1891, pp. 428, 438, 440, 444–5.

[3] *Ib.*, xxxiii, 1899, pp. 15, 20–3, 26–32; xxxiv, 1900, p. 74. A similar method of fortification was practised by the Dacians (*Congrès archéol. de France*, 1874 [1875], p. 444), 'in the Danne-werk at Korborg, near Schleswig' (A. Pitt-Rivers, *Excavations in Cranborne Chase*, iii, 254), and in Nassau (*Rev. de synthèse hist.*, iii, 1901, p. 45).

The well-known camp on Herefordshire Beacon is interesting because, like Old Sarum (Sorbiodunum), it contains a citadel. Though it is locally described as a 'British camp', its date is at present uncertain. While most of the objects which have been found in it are comparatively late, Pitt-Rivers (*Journ. Anthr. Inst.*, x, 1881, p. 331) pointed out that the pottery seemed to indicate its Celtic origin; but the citadel presents a difficulty. Was it a later addition? See also F. J. Haverfield, *Archaeol. Survey of Herefordshire*, 1896, pp. 3–4.

The 'vitrified' stone forts of the British Isles demand a brief notice. There are none in England, but many in the northern and western counties of Scotland and some in France. It is very doubtful whether any exist in Wales or Ireland (*Archaeol. Journal*, xxxvii, 1880, pp. 227, 234; D. Christison, *Early Fortifications in Scotland*, pp. 187, 190). The question is whether the vitrifaction, which was due to fire, was accidental or designed; and in some cases the only way of settling this is to ascertain by excavation the extent of the vitrifaction (*ib.*, p. 192). The best authorities have concluded that when the vitrified part of the fort is small the phenomenon may be safely ascribed to accident,—perhaps to a beacon fire; but that when it may be traced almost all round the rampart it was intentional (*ib.*, pp. 186–7; *Archaeol. Journal*, xxxvii, 1880, pp. 240–1; R. Munro, *Prehist. Scotland*, pp. 382–3). Probably the builders intended to give cohesion to the walls and make it impossible for assailants to demolish them (*L'Anthr.*, xiv, 1903, pp. 330–1); or when the vitrifaction was confined to the upper surface the defenders would have secured firm foothold while the assailants would have stumbled over loose stones (D. Christison, *op. cit.*, pp. 186–7). [See Addenda]

stands upon high ground about two miles south-west of Northampton, might never have surrendered its precious relics if the iron ore which was known to underlie the site had not attracted the prospector. About thirty years ago a company was formed to win the iron; and navvies accidentally did the work which would have been better performed under scientific direction. Hunsbury is so small that it could hardly have been a tribal centre: the entrenchment encloses only four acres,—less than the twelfth part of the area of Hod Hill. Not the faintest trace of Roman influence could be detected among the remains, which are now arranged in the Northampton Museum; and the experts who examined them concluded that they belonged to the time of Caesar's invasion. They were found in pits, resembling those of Mount Caburn, about three hundred of which had been dug inside the rampart; and here too there was evidence that the dwellings had been huts of wattle-work. The townspeople were well armed: they kept horses and chariots, wove their own cloth, sawed their own timber, made their own earthenware, and grew their own corn; and heaps of slag showed that they had smelted the ore, which lay thenceforward undisturbed for nineteen hundred years.[1] One of several skulls which were found just outside the town was perforated with three holes, which suggest that the British Celts, like the Gauls and their neolithic predecessors, made amulets out of the remains of their own dead.[2]

Inhabited caves;

But perhaps not many British settlements were of this

[1] *Reports Archit. Soc. of . . . Lincoln*, &c., xviii, 1885-6, pp. 53-61; *Archaeologia*, lii, 1890, pp. 382-4; *Vict. Hist. of . . . Northampton*, i, 147-9, 151-2. At Beansale and Claverdon in Warwickshire there are camps which in many respects resemble that of Hunsbury, but have not been excavated (*Vict. Hist. of . . . Warwick*, i, 350).

Professor T. McKenny Hughes (*Archaeologia*, liii, 1892, p. 484) suggests that Offa's Dyke may have 'belonged to the defensive system of the Britons'. All we know is that those dykes which have been excavated—Bokerly Dyke and Wansdyke—were Roman or post-Roman (A. Pitt-Rivers, *Excavations in Cranborne Chase*, iii, p. xiii); and it is in the last degree improbable that earthworks which extend over territory that belonged to several tribes should have been constructed at a time when tribes only combined for brief periods and in the presence of urgent and common peril. Cf. F. J. Haverfield, *Archaeol. Survey of Herefordshire*, 1896, p. 7, and *Eng. Hist. Rev.*, xvii, 1902, pp. 628-9.

[2] See p. 93, *supra*.

comparatively advanced type. In the Late Celtic Period, and indeed long after its close, caves were still inhabited, as throughout the prehistoric ages, in some cases by outlaws, who made a precarious livelihood by robbing wealthy travellers.[1] Pit-dwellings in small groups, which apparently differed little from those of the Neolithic Age, have been found stored with Late Celtic relics ;[2] and doubtless it was from habitations of this class that the thatched huts of mud and wattle-work which Strabo[3] describes, and the remains of which have been already noticed, were evolved. Such cottages, as Caesar[4] testifies, were much the same in Gaul and Britain. Posidonius was made welcome in them when he travelled in Gaul. He tells us how his hosts, seated on straw round low tables, took their meat in their fingers and tore it like lions or chopped it in pieces with their pocket-knives, and washed it down with draughts of beer from earthenware or silver beakers ; how the meal was sometimes interrupted by a quarrel, when the disputants sprang to their feet and fought till one was slain.[5] In the far north and in the Cornish peninsula men lived in underground dwellings, commonly called ' Picts' houses ', which generally consisted of a paved trench lined with dry masonry, roofed over with slabs, and terminating in a round chamber ; while in some Scottish examples rooms were grouped on both sides of the gallery.[6] Related to these structures are the

pit-dwellings ; ' Picts' houses ' ; beehive houses ; and brochs.

[1] *Journ. Derbyshire Archaeol. and Nat. Hist. Soc.*, xiii, 1891, pp. 194–9 ; xiv, 1892, pp. 247–8 ; xvii, 1895, p. 76 ; *Vict. Hist. of . . . Derby*, i, 231–42. Cf. *Association franç. pour l'avancement des sc.*, 32e sess., 1903, 2e partie, p. 890.

[2] *Vict. Hist. of . . . Bedford*, i, 172. See also p. 84, n. 1, *supra*.

[3] *Geogr.*, iv, 4, § 3. Cf. Caesar, *B. G.*, v, 12, § 3, 43, § 1, and Diodorus Siculus, v, 21, § 5. Woodcuts, one of the Romano-British villages explored by Pitt-Rivers, was constructed and chiefly occupied by Britons (*Excavations in Cranborne Chase*, ii, 65, iii, 3) ; but, as Prof. Haverfield has pointed out (*The Romanization of Roman Britain*, pp. 18–9), ' the material life was Roman '.

[4] *B. G.*, v, 12, § 3.

[5] Athenaeus, iv, 36. Cf. Diodorus Siculus, v, 28, §§ 4–5 and Strabo, iv, 4, § 3.

[6] *Proc. Soc. Ant.*, 2nd ser., iv, 1867–70, pp. 164–70 ; *Journ. Brit. Archaeol. Association*, xxxvi, 1880, pp. 254–61 ; J. Anderson, *Scotland in Pagan Times,—the Iron Age*, p. 207 ; R. Munro, *Prehist. Scotland*, pp. 348–9 ; B. C. A. Windle, *Remains of the Prehist. Age*, p. 266 ; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 541–7. It must be admitted that conclusive evidence is wanting to prove that any of the Cornish subterranean dwellings were inhabited before the

Scottish mound-dwellings or bee-hive houses, specimens of which in the island of Lewis were still inhabited in the nineteenth century. They may be looked for in places such as the Hebrides, where branches large enough to form roofs like those of pit-dwellings were not to be had. In some a central chamber was connected with others which opened out of it: a hole, which could be closed at will, was left in the roof for the escape of smoke; the chinks between the stones were stuffed with grass or moss; and the roof was covered with turf, which adhered to the interstices and made the structure compact. It is impossible to assign a precise date to these huts. Some of them contained querns and were certainly occupied in the time of the Romans; but probably many had been built before, while others are comparatively modern.[1] The most elaborate buildings of this type were the brochs, whose range extends from the Orkney and Shetland Isles, which contain nearly a hundred and fifty, to Berwickshire, but which do not exist outside the Scottish area. These buildings, which were really small forts, represent the art of dry-walling at its zenith. They were round towers about sixty feet high and fifty feet in diameter. If an enemy succeeded in forcing a way in, he found himself in an inner court open to the sky and enclosed by a commanding wall, pierced by numerous apertures, which formed the windows of encircling galleries, from behind which the defenders were prepared to shoot.[2] The relics which have been found in them belong for the most part to the close of the Roman occupation and even later; but some which have been excavated in Caithness contained painted pebbles like

Roman occupation (see *Vict. Hist. of... Cornwall*, i, 367–9). The 'hut-clusters' of Cornwall, of which Chrysoister is a good example (W. C. Lukis, *Prehist. Stone Monuments of the Brit. Isles,—Cornwall*, p. 19) were probably later than the hut-circles of the same county. Some may have been built before the Christian era, but they were certainly inhabited in Roman times (*Vict. Hist. of... Cornwall*, i, 370).

[1] *Archaeol. Journal*, x, 1853, pp. 212, 215–9, 221–2; xviii, 1861, pp. 39–46; *Proc. Soc. Ant. Scot.*, iii, 1863, pp. 128, 134–8, 141; xxxviii, 1904, pp. 102–22, 173–89, 548–58; Sir A. Mitchell, *The Past in the Present*, p. 58; *Trans. Glasgow Archaeol. Soc.*, N. S., iv, 1902, pp. 189–90.

[2] *Proc. Soc. Ant. Scot.*, xxxv, 1901, pp. 116–7, 119, 147; xxxviii, 1904, p. 558.

those of the late palaeolithic cavern of Mas d'Azil; and it is possible that they may have existed in pre-Roman times.[1]

The Glastonbury marsh-village.

The most interesting, however, of all the Late Celtic settlements is the far-famed marsh-village of Glastonbury. Besides those of Holderness, which have been already mentioned, there are several lake-dwellings in Great Britain which belonged to the Early Iron Age; but almost all seem to have been built after the commencement of the Christian era.[2] Glastonbury, on the other hand, was first inhabited more than two centuries before the Roman conquest. The peat-moor on which it stands was then surrounded by a shallow mere, and is now covered by low circular mounds which mark the positions of the former huts. Timber and brushwood, surmounted by layers of clay and stones, were laid upon the peat to serve as foundations, and retained in

[1] *Proc. Soc. Ant. Scot.*, xxxv, 1901, pp. 146–8; *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 35–6; A. Lang, *The Clyde Mystery*, p. 41.

[2] *Journ. Anthr. Inst.*, xv, 1886, pp. 463–5; xxviii, 1899, pp. 150–4; R. Munro, *The Lake-Dwellings of Europe*, pp. 454, 459, 461, 475, 493. Dr. Munro (*ib.*, pp. 490–2) observes that 'in the early centuries of the Christian era the distribution of crannogs in Scotland and Ireland closely coincides with a well-defined area in which the Celtic language was spoken', though he admits that 'they have not been found in the south-eastern provinces of Scotland'. 'In this wider area' [including Southern Britain], he continues, 'on the supposition that the Celts were the introducers or founders of the system, we ought to find some vestiges of these dwellings ... This is precisely what the general researches into British lake-dwellings have shown in the stray remnants of them that have been found in Llangorse, Holderness, the *meres* of Norfolk and Suffolk, Cold Ash Common, etc. All these, with perhaps the exception of the pile-structures at London Wall, appear to be older than the majority of the crannogs of Scotland and Ireland ... Taking all these facts into account ... I am inclined to believe that we have here evidence of a widely distributed custom which underlies the subsequent [to Caesar] great development which the lake-dwellings assumed in Scotland and Ireland. Moreover, I believe it probable that the early Celts had got this knowledge from contact with the inhabitants of the pile-dwellings of Central Europe.'

Llangorse is the only Welsh site at which a lake-dwelling has been found (*ib.*, p. 464). I venture to ask the doctor why lake-dwellings are so rare in England and Wales, where, on his theory, they ought to abound; why the Scottish and Irish Celts did not apply their 'knowledge' for some centuries after they reached the British Isles; and why lake-dwellings are non-existent (*ib.*, p. 493) in Spain and Portugal, where Celts were numerous (G. Dottin, *Manuel pour servir à l'étude de l'ant. celt.*, pp. 324, 329–31, 349)? And, seeing that there are pile-dwellings in New Guinea and Central Africa, is it not conceivable that those of the British Isles had no connexion with Central Europe?

place by piles fixed round their margins. The huts were then built of wood, filled in with wattle and daub; and the entire village was protected by a palisade. The foundations were, however, so unstable that they gradually sank; and in order to keep the floors dry, fresh timber and clay were periodically added. When this was done, the old hearth-stones were left undisturbed; and their presence attests the construction of the successive floors. Among the numerous relics which excavation has revealed, and which prove that skilled agriculturists, potters, weavers, wood-carvers, and coopers lived in the village, there is hardly a single weapon: the sling-bullets evidently served only for killing game. Dozens of coloured pebbles, similar to others which have been found on Hod Hill, were perhaps used in some indoor game;[1] and the spur of a cock may suggest to those who remember that the Britons thought it impious to eat poultry that the pastime for which, as Caesar says, the birds were reared was cockfighting.[2] It is hardly necessary to mention the weaving-combs, the spindle-whorls, the querns, the harness-buckles, and the other objects which are common in Late Celtic settlements, though it is curious that the bridle-bits were made of deer-horn; but the explorers were astonished to find a bronze mirror, tweezers, rouge, and other exotic objects, which showed that continental luxury had invaded this remote region.[3]

Dress. The arts of the toilet had indeed been elaborated not only in the more civilized south but even in places which, like the Yorkshire Wolds, had no direct communication with foreign lands.[4] The tunics, the cloaks which men and women alike wore, fastened on the right shoulder with a brooch, the breeches which were common to Brythonic Celts in Britain and Gaul, and the use of which seems to have been

[1] Cf. Tacitus, *Germania*, 24, and *Archaeologia*, xliii, 1871, pp. 439–40.

[2] *Vict. Hist. of . . . Somerset*, i, 198.

[3] *Report of . . . the Brit. Association*, 1893 (1894), p. 903; 1894, pp. 431–4; 1898, pp. 694–5; 1904 (1905), pp. 324–30; *Proc. Somerset. Archaeol. and Nat. Hist. Soc.*, xlix, 1903, pp. 103, 107–8, 114–5, 120–1; l, 1904, pp. 68–93; li, 1905, pp. 77–104; *Journ. Anthr. Inst.*, xxxv, 1905, p. 395; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 126–7.

[4] *Ib.*, p. 127; *Crania Britannica*, ii, pl. 6 and 7, p. 4.

borrowed by the Continental Celts from the Scythians,[1] the kilts which, as we may perhaps infer from stone monuments,[2] were the national garb of the Goidels, were made, like the modern tartan, of many-coloured cloths; while the men whom Caesar encountered, although, like the Gauls, they wore their hair long, and cultivated moustaches, carefully shaved the rest of their faces and even their bodies.[3] The chieftain driving his chariot, his brilliant cloak clasped by a coral-studded brooch, his sword clanking in its decorated scabbard, his bronze shield gleaming like gold and adorned with enamel, his horses' bridle-bits showing enamelled cheek-pieces, and their harness jingling with open-work bronze ornaments,[4] was perhaps only a splendid barbarian; but his weapons and his trappings were not mere products of a factory:—they were true works of art.

FIG. 43. ½

Nor indeed are indications wanting that Britons of the upper class—not Druids only—had some tincture of letters. The Druids of Gaul, and presumably also of Britain, used Greek characters in official documents and private correspondence.[5] Diodorus[6] affirms that it was common among the Gauls to throw letters, addressed to the dead, on to the funeral pile. The Romans, after they had defeated the

Reading and writing.

[1] Diodorus Siculus, v, 30, § 1; Strabo, iv, 4, § 3; C. Elton, *Origins of Eng. Hist.*, 1890, p. 110; Rice Holmes, *Caesar's Conquest of Gaul*, 1903, p. 10; *Rev. arch.*, 4e sér., i, 1903, pp. 337–42; H. d'A. de Jubainville, *Les Celtes*, pp. 337–42.

[2] J. O. Westwood, *Lapidarium Walliae*, 1876–9, p. 37, and pl. xxv, fig. 3; J. Rhys, *The Welsh People*, 1902, p. 567.

[3] *B. G.*, v, 14, § 3.

[4] *Proc. Soc. Ant.*, 2nd ser., xx, 1904–5, pp. 345–6; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 50, 135.

[5] *B. G.*, vi, 14, § 3.

[6] *Bibl. Hist.*, v, 28, § 6.

Helvetii, found in their encampment a schedule, on which were recorded in Greek characters the numbers of the armed men, the women, and the children who had migrated into Gaul.[1] A few years later, when Caesar was marching through the territory of a Belgic tribe to relieve a besieged camp commanded by Quintus Cicero, he wrote him a letter in Greek characters—possibly in Greek [2]—which he entrusted to a Gallic trooper. Unless he made his interpreter write the letter in Celtic, he evidently had reason to fear that, if it were intercepted, some of the Belgae would be able to read the Latin; in any case that some of them knew how to read. Is it not reasonable to infer that a British Belgian here and there was as good a scholar as his kinsmen over the water? At all events the British inscribed coins, the earlier of which at least must have been the work of native die-sinkers, are evidence that before the birth of Christ there were Britons who had mastered the art of writing, and had even acquired some slight knowledge of Latin.[3] But the origins of Celtic literature, sacred and profane, were of course purely oral. Bards, who were apparently Druids of an inferior grade, sat at the tables of the great; accompanied them with their harps to festivals; sang their praises and satirized their enemies; and recited poems in honour of valiant warriors who had fallen in battle.[4]

Inequalities in culture

It must not, however, be supposed that the same level of culture had been attained in every part of the island. The Scottish specimens of Late Celtic workmanship are for the most part later than the Claudian conquest;[5] and it is probable that in outlying districts even of England and Wales iron tools in pre-Roman times were rare or unknown. No objects of the Early Iron Age which are regarded as purely

[1] *B. G.*, i, 29, § 1.

[2] *Ib.*, v, 48, §§ 3–4. Cf. my *Caesar's Conquest of Gaul*, 1899, p. 715.

[3] J. Evans, *Coins of the Anc. Britons*, p. 171. Cf. p. 368, *infra*, and F. J. Haverfield, *The Romanization of Roman Britain*, 1906, p. 9.

[4] Diodorus Siculus, v, 31, § 2; Strabo, iv, 4, § 4; Athenaeus, iv, 37, vi, 49; Ammianus Marcellinus, xv, 9, § 8.

[5] *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 144. Cf. *Proc. Soc. Ant.*, 2nd ser., xviii, 1901, p. 373.

British have been found in Lancashire;[1] and even on Cranborne Chase, where one might have expected that continental improvements would have been adopted at least as early as in the far western settlement at Glastonbury, the searching exploration of Pitt-Rivers could detect no signs of any interval between the Bronze Age and the period of the Roman occupation.[2] Indeed the association of late bronze implements and weapons with iron harness-rings and bridle-bits at Hagbourne Hill[3] suggests that some of the deposits which are assigned to the Bronze Age may have belonged either to a period of transition or even to the time when, in South-Eastern Britain, the use of iron was universal.[4] Readers of the *Commentaries* would see nothing surprising in this. Caesar was told that the people of the interior for the most part did not grow corn, but lived on milk and flesh-meat and clothed themselves in skins.[5] This information was somewhat misleading; for remains of four different kinds of corn were counted at Hunsbury; and since cloth and linen were worn in Yorkshire by the well-to-do even in the Bronze Age,[6] it is not to be supposed that their successors had lost the arts of spinning and weaving. Still, Caesar's statement points to an ascertained truth. It has been well observed that the western and northern uplands held out far longer against the Roman conquest than the central, eastern, and southern lowlands, and that they were never really Roman-

[1] *Vict. Hist. of . . . Lancs*, i, 246. Only one has come to light in Durham (*Vict. Hist. of . . . Durham*, i. 209).

[2] *Excavations in Cranborne Chase*, iv, 11, 59–61. A bronze socketed celt has been found at Cann, near Shaftesbury, in association with British silver coins (J. Evans, *Coins of the Anc. Britons*, p. 102).

[3] *Archaeologia*, xvi, 1812, pp. 348–9; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 83, 103–4. If it is true that coins formed part of the Hagbourne Hill deposit, bronze implements must have continued in use in Berkshire to a very late date.

[4] May the rarity of British iron weapons be partly accounted for by supposing that during the greater part of the Late Celtic Period swords and spear-heads were still in many cases made of bronze? In the Homeric Age implements were of iron, but the weapons which the poet mentions were all of bronze, doubtless because the armourers had not yet learned to temper iron (*Rev. arch.*, 4e sér., vii, 1906, pp. 284, 290–1, 294).

[5] *B. G.*, v, 14, § 2.

[6] See pp. 161, 189, *supra*.

ized at all.[1] From the earliest times their inhabitants had been less open to continental and civilizing influences; and one of the gifts which Nature had bestowed upon Britain was that the regions more accessible from over sea were also more fitted to sustain an industrial population.[2] Later on, however, we shall find reason, in the juxtaposition of old and new sepulchral rites, to believe that even in Kent such influences had not prevented the survival of the earlier culture.[3]

Intertribal war and political development.

Moreover, notwithstanding the progress in material civilization, intertribal fighting was of course still frequent even in the south, and even after the Belgic tribes had settled down in the territories which their swords had won, and established themselves as the dominant people of Britain. Both Caesar[4] and Tacitus[5] spoke of these wars; but if they had been silent, the numerous strongholds which were still occupied, permanently, or as occasion required, the weapons that have been found in them, the beach-rolled pebbles, the round chipped flints, and the bullets of baked clay which lie heaped in and near them would tell the same tale;[6] nor indeed is it

[1] F. J. Haverfield, *The Romanization of Roman Britain*, pp. 7–9. Cf. *Vict. Hist. of . . . Derby*, i, 191–2, and see also Solinus, 22, 12 (ed. Th. Mommsen, p. 234).

I hardly know whether it is worth while to notice the statements of Diodorus (v, 32, § 3) and Strabo (iv, 5, § 4) in regard to the prevalence of cannibalism in certain parts of the British Isles. If there is any truth in them, the cannibals had doubtless inherited the custom from neolithic times (p. 113, *supra*). Strabo's remark, which, as he himself warns us, does not rest upon good authority, refers only to Ireland. Diodorus says that some of the Britons were cannibals; but this observation may also refer to the Irish. A mound-dwelling near Kirkwall (*Archaeol. Journal*, x, 1853, p. 217) is said to have contained broken human bones mingled with those of sheep, which may or may not be evidence of cannibalism; and every scholar knows the speech that Caesar puts into the mouth of Critognatus, one of the Arvernian chiefs who was blockaded in Alesia (*B. G.*, vii, 77, § 12). As for the unnatural vices with which Diodorus (v, 32, § 7), Strabo (iv, 4, § 6), and others charge the Celts, they are rife among the civilized nations of modern Europe.

[2] See H. J. Mackinder, *Britain and the British Seas*, pp. 177–9.

[3] See p. 288, *infra*. [4] *B. G.*, v, 9, § 4; 11, § 9. [5] *Agricola*, 12.

[6] *Journ. Brit. Archaeol. Association*, xxviii, 1872, p. 42; *Archaeologia*, xlvi, 1881, p. 467; lii, 1890, pp. 761–2; *Trans. Epping Forest . . . Field Club*, ii, 1882, p. 65; C. W. Dymond and H. G. Tomkins, *Worlebury*, 1886, p. 78; Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 419–20; *Archaeol. Journal*, lix, 1902, pp. 213–6.

necessary to insist upon a fact which is universal in the stage of culture in which the Britons then were. What is worthy of remark is that war was probably entered upon from motives other than those which had caused the struggles of earlier ages. Raids were no doubt still undertaken, especially in the poorer and less settled districts, by mere plunderers and cattle-lifters. But clans were tending to become welded, not only by the voluntary combination which was necessary for defence, but also perhaps by the sword of the ambitious captain, into the larger communities which Caesar called *civitates*[1]; and successful chiefs were assuming the state of petty kings. As trade increased, and with it wealth, the king of a tribe which was fortunately situated would seize opportunities of acquiring dominion or overlordship over others. Though forest or mountain or fen might enable even small tribes to hold their own, and though the success of a strong king might not endure, it is possible, as we shall see, to discern in Caesar's memoirs signs that attempts were already being made to achieve such sovereignty as might eventually lead towards political union, and we may suppose that in Britain also there were astute princes who, like the Aeduan Dumnorix, saw that they could strengthen their position by diplomacy or marriage.[2]

Instances of female sovereignty: the condition of women.

We all learned in childhood that the Britons admitted the sovereignty of women. In the middle of the first century Cartismandua was queen of the Brigantes;[3] and a few years later, when the Iceni revolted against Rome, their general was Boudicca, who is better known by the barbarous misnomer of Boadicea.[4] The Gauls may have had the same institution; and perhaps it would hardly be worth noticing if it were not apparently inconsistent with what Caesar tells us about the status of Gallic wives. They were indeed permitted to own property. The bride brought a dowry to her husband; but he was obliged to add an equiva-

[1] See *Rev. arch.*, 3e sér., xli, 1902, p. 428, and my *Caesar's Conquest of Gaul*, 1903, p. 12, n. 1.

[2] See *B. G.*, i, 18, §§ 6–7.

[3] Tacitus, *Ann.*, xii, 36.

[4] See p. 296, *infra*.

lent from his own estate and to administer the whole as a joint possession, which, with its accumulated increments, went to the survivor.[1] On the other hand, the husband had the power of life and death over his wife[2] as well as his children; and when a man of rank died his relations, if they had any suspicion of foul play, examined his wife, like a slave, by torture, and, if they found her guilty, condemned her to perish in the flames of the funeral pyre.[3]

Political and social conditions of Britain and Gaul compared.

When we try to form an idea of the political and the social conditions of Britain in the later days of its independence, we naturally turn to Caesar's account of Gaul in the hope of supplementing the scanty and scattered scraps of information which he has left about the country which was less known to him. We must, however, bear in mind that Britain had not yet come under the two currents of influence, German and Roman, which had profoundly affected Gaul, and in some measure prepared it to accept Roman dominion; and also that even the south-east was in a more rudimentary stage than the neighbouring country, though perhaps not more than the backward parts of Belgic Gaul.

When Caesar came to Gaul, revolutionary forces were at work to which there are analogies in the earlier history of Greece and Rome. Many of the states had expelled their kings, whose authority had passed in some cases into the hands of annually elected magistrates, while in others perhaps the council of elders kept the government to itself. But these oligarchies were never long secure. The magistrates were fettered by rules, jealously framed, which weakened their executive power. Like the Tarquins, the banished kings or their descendants looked out for opportunities, which Caesar's policy offered to them, of regaining their position;

[1] *B. G.*, vi, 19, §§ 1–2. Cf. my *Caesar's Conquest of Gaul*, 1899, pp. 521–2.

[2] *B. G.*, vi, 19, § 3. M. d'Arbois de Jubainville (*Études sur le droit celt.*, i, 1895, p. 241) holds that if *uxores* means 'wives', Caesar's statement is inconsistent with the custom which regulated the administration of dowries, and accordingly gives the word the sense of 'concubines'. It seems to me equally rash to assume that Caesar was mistaken, and that *uxores* means 'wives' in § 1 and 'concubines' in § 3. May we not suppose that the husband's power was checked by public opinion?

[3] *B. G.*, vi, 19, § 3.

while eloquent nobles who had contrived to amass wealth summoned their retainers, hired mercenaries, surrounded themselves with desperadoes or with the discontented poor, whose grievances they promised to redress, and occasionally succeeded, like Pisistratus of Athens, in making themselves tyrants. Celtillus, the father of the great Vercingetorix, had acquired a kind of supremacy over the whole of Celtican Gaul; but he was dogged by the jealousy of his brother nobles, who put him to death on the charge of plotting to revive the kingship. Monarchy and oligarchy had each their partisans: everywhere there were adventurers who hoped to make their way to fortune by Roman aid, while others, eager to oust their rivals, were ready to welcome German invaders; and thus every state, every clan, every hamlet, nay, every household was riven by faction.[1] But in Britain there is no sign that either oligarchy or tyranny had yet anywhere supplanted monarchy. Still, there were doubtless many points of resemblance. We may suppose that in Britain, as in Gaul, the tribal king was assisted by a council of elders; that the British, like the Gallic nobles, had their devoted retainers and perhaps also dependents who had fallen into their debt;[2] that only those who became their dependents could expect protection, and that only those lords who were strong enough to protect could count upon obedience. In Britain too we may be sure that the masses were in the state of semi-serfdom which Caesar regarded as the condition of the Gallic populace; and that political power was monopolized by the nobles and the Druids.

Religion.

But, besides improved communication, developed commerce, and constant intercourse with their Continental kinsmen, there were other forces making slowly and feebly for unity, — common religious ideas and, to some extent, common ecclesiastical organization. On the other hand we may suppose that the religious union which existed together with much diversity was an effect as well as a cause of political association: when clans found it expedient to combine, the similar deities of each, which the others had before

[1] See my *Caesar's Conquest of Gaul*, 1903, pp. 12–5.

[2] *Ib.*, 1899, pp. 525–7.

regarded with hatred and jealousy, would tend to become fused, while those which were peculiar would be worshipped still.[1] Old superstitions of course continued to flourish side by side with those which the Celtic invaders had brought with them. The spirits of springs, of lakes, of rivers, of mountains, and of woods—of every weird and awesome dell, or cavern, or rock—were worshipped in the Iron Age as they had been for centuries before, and as they continued to be after what was called Christianity had become the official creed.[2] The *Dea Arduinna* who hovered over the forest of the Ardennes and Abnoba, the goddess of the Black Forest, had their counterparts in Britain. These deities, however, may have been comparatively recent; for the conception of a god whose realm was a forest was of course later than that of the spirit of a single tree.[3] Even the terror that impelled the pristine savage to propitiate demons was not yet dead: near Newcastle-on-Tyne was erected by some Roman or Romanized Briton an inscription *Lamiis tribus*—'to the Witches three'—who, it has been truly said, 'were doubtless as British as the witches in *Macbeth*'.[4] But the cult of wood and water and the dread of devils are common to all primitive peoples and to the ignorant among many who are called civilized;[5] and such survivals in Celtic Britain may well have been common to the pre-Celtic population and to the Celts who conquered them. Moreover, it is likely enough that the greater gods whom the Celts worshipped and who, variously imagined and with various names, were the common heritage of the Aryan-speaking peoples, were in part descended from deities who were not Aryan, and were adored in Britain in a somewhat different spirit before the first Celt landed on the Kentish shore.[6]

[1] See W. Robertson Smith, *The Religion of the Semites*, 1901, pp. 31–2, 38–9.

[2] Ausonius, *Clarae urbes*, xiv, 31–2; Gildas, *Hist.*, 2. Cf. J. Rhys, *Celtic Heathendom*, 1888, p. 106; Sir A. Lyall, *Asiatic Studies*, i, 1899, pp. 12, 20–2; and E. B. Tylor, *Prim. Culture*, ii, 1903, pp. 212–4.

[3] See J. G. Frazer, *Early Hist. of the Kingship*, p. 154.

[4] *Corpus Inscr. Lat.*, vii, 507; J. Rhys, *Celtic Heathendom*, p. 104.

[5] See *Rev. celt.*, ii, 1873–5, p. 1; iv, 1879–80, pp. 57–8; xviii, 1897, p. 259; E. B. Tylor, *Prim. Culture*, ii, 1903, pp. 221, 228.

[6] Cf. J. Rhys, *Celtic Heathendom*, p. 106, with G. Dottin, *La rel. des Celtes*, 1904, p. 60.

What do we know about those gods? The Celts were the first inhabitants of Britain about whose religious views definite information has been handed down to us, as distinct from what we may infer from sepulchral discoveries and from ethnography; but it is hardly an exaggeration to say that of the spirit of their religion we know little more than of that of the people who built the chambered tombs. Some five-and-twenty writers, from Timaeus, who wrote three centuries before the birth of Christ, to Ammianus Marcellinus, who was contemporary with Julian and Valens, have contributed to our knowledge; but most of them have left only a few sentences derived from hearsay or from nameless authorities of whose credibility we know nothing. They wrote of Celts who lived in widely distant countries, among various populations, and at different epochs; and very few of them referred to the Celts of Britain.[1] Supposing that official Christianity were to become extinct, what could the historian of the fifth millennium learn of the manifold doctrines preached by English clergymen if he were obliged to extract his materials from passages referring to mediaeval Catholicism, Calvinism, Methodism, or the orthodox faith which thinly disguises the Shamanism of Russia, and scattered in the works of writers who began with à Kempis and ended with Spurgeon? Coins, Gallic and British, in so far as they are not merely imitative, appear to be fraught with religious symbolism; but the ingenuity which has spent itself in the effort to explain the symbols has yielded little certain result.[2] Geographical names testify to the cult of various gods without telling us anything of their attributes; and sometimes we may fancy that we can detect the presence of divinity when we have only to do with the name of a Roman *gens*.[3] Inscriptions and altars supply names of

[1] M. Jullian (*Rev. des études anc.*, iv, 1902, p. 101) points out that the texts fall into two groups, one of which, all posterior to 100 B.C., deals with the Transalpine Celts, and the other, mostly earlier, with all the others, except the Britons.

[2] *Rev. celt.*, xii, 1891, p. 316; *Rev. num.*, 3e sér., ii, 1884, pp. 179–202; *Rev. des études anc.*, iv, 1902, p. 279, n. 2.

[3] 'On se tromperait beaucoup,' says M. Dottin (*La rel. des Celtes*, pp. 7–8), 'si l'on croyait que tous les anciens *Mercuriacus* de France, devenus aujourd'hui

deities which are names and nothing more, or bewilder us by coupling as surnames with the name of a Roman god a multiplicity of Celtic gods. Anonymous statues are attributed to divers deities by divers archaeologists, though some of them may not be deities at all. Inscriptions, altars, and statues alike belong to the period of the Roman Empire, when the introduction of Roman gods and goddesses had thrown the Celtic pantheon into wellnigh inextricable confusion; and the monuments of Britain, for the most part, were apparently the outcome of the devotion either of Romans or of Gallic, Batavian, Dacian, and other officers of auxiliaries. Nor can we tell how far British religious ideas had become estranged from those of Gaul by contact with aboriginal cults, or how far the religion of the British Goidels (if indeed they existed) differed from that of the Brythons. If we turn to the *Mabinogion*, to the *Triads*, or to Irish mythology, we are checked by the reflection, which our foremost Celticist was forced to make even while he was fascinated by the quest, that 'the gulf of ages' separates 'the literature of the Celtic nations of the present day from the narrative of the writers of antiquity and the testimony of the stones'.[1]

Cannot then Caesar help us? His evidence is of course valuable; but he did not write for the modern student of religion. Disregarding minor and local deities, perhaps ignorant of their existence, he recorded the names and summarized the attributes of the five principal Gallic gods; but,—the names are Roman. Mercury—the inventor of all arts, the pioneer of communication, the patron of commerce—was the most reverenced of all:[2]

Mercuray, Mercurey, Mercoirey, Mercury, sont dérivés du nom de dieu Mercurius. Ils proviennent plus vraisemblablement du gentilice romain Mercurius, assez fréquent dans les inscriptions, et dénomment simplement le *fundus*, la propriété d'un Gallo-Romain du nom de Mercurius.'

[1] J. Rhys, *Celtic Heathendom*, p. 235. See also *Rev. celt.*, iv, 1879–80, p. 45; x, 1889, pp. 485, 487, 489; H. Gaidoz, *Esquisse de la rel. des Gaulois*, 1879, p. 11, *Études de mythologie gaul.*,—*Le dieu gaul. du soleil*, 1886, pp. 90–1, 93; *Rev. num.*, 3e sér., ii, 1884, p. 201, n. 1; *Archaeol. Review*, ii, 1889, p. 124; *Journ. Brit. Archaeol. Association*, l, 1894, pp. 105–9; and G. Dottin, *La rel. des Celtes*, pp. 5–16, 56–7, 60.

[2] Caesar does not say that Mercury was actually the supreme deity of the Gauls, but only the most fervently worshipped: he expressly says that they

then follow the names of Apollo, Mars. Jupiter, and Minerva.[1]

Now we do not know from whom Caesar derived his information; but assume that it came from the best authority, his friend and political agent, the Aeduan Druid, Diviciacus, who was also an honoured guest of Cicero.[2] Then Caesar was in the position not of Lafcadio Hearn, who made his home in Japan, gave his life to the study of all things Japanese, and at last confessed that the more he tried to learn the more he realized his ignorance; not of Sir Alfred Lyall, who, prepared by discriminative reading, devoted all the time that he could command to the observation of Oriental creeds; but of some Anglo-Indian administrator who, in his scanty leisure, should jot down the heads of a conversation with a Brahmin, and offer them as an outline of Hindu religion. Only the Anglo-Indian could speak Hindustani; and Caesar was obliged to employ an interpreter. One of the most learned and sane of modern Celtic scholars has related that when the musician, Félicien David, was invited at Cairo by the viceroy to instruct his wives, etiquette compelled him to give the lessons to a eunuch, who passed them on as best he could.[3] Caesar, he remarks, was in the position of the eunuch. And if we could certainly identify the five great Roman gods with their Gallic counterparts, how much more of Celtic religion should we know?

But let us learn what we can. Celtic religion, in so far as it was descended from the religion of the undivided Aryan stock, was fundamentally one with the religions of Italy and Greece; and we might expect that it would resemble most closely the religion of the Italians, to whose tongue Celtic

regarded their Jupiter as the lord of the celestials. 'It must not be supposed,' says Sir Alfred Lyall (*Asiatic Studies*, i, 1899, p. 121), 'that even the uppermost gods of Hinduism have retired behind mere ceremonial altars, like constitutional monarchs . . . But there seem to be many grades of accessibility among them, from Brahma—who, since he created the world, has taken no further trouble about it, and is naturally rewarded by possessing only one or two of the million temples to Hindu gods,' &c.

[1] *B. G.*, vi, 17.

[2] *De divin.*, i, 41, § 90. Cf. my *Caesar's Conquest of Gaul*, 1899, p. 532, n. 13.

[3] H. Gaidoz, *Études de mythol. gaul.*, — *Le dieu gaul. du soleil*, p. 91. Cf. E. B. Tylor, *Prim. Culture*, ii, 1903, pp. 252, 254.

was most nearly akin. But our imperfect knowledge of the classical religions hardly helps us more to understand the religion of the Celts than the remark of Caesar, that about their deities 'they have much the same notions as the rest of mankind'.[1] For the religion of Rome had been deeply tinged by contact with the Etruscans and the Greeks, just as the religion of the Celts had been affected by their fusion with the aboriginal peoples of Central Europe, Gaul, Spain, and Britain; and the Celts were in a less advanced state of civilization than the Romans. What is certain is that, like every other polytheistic religion, that of the Celts, except perhaps in so far as it was moulded by Druidical doctrine, had no definite theology, but was an ever-expanding, ever-shifting, formless chaos,—the same in its main developments in Britain, Gaul, and Spain, yet differing in every tribe and household, and in every age;[2] that, on its practical side, it was a performance of traditional rites; that its aim was not the salvation of souls, but the safety of the state; and that it concerned the individual most as a member of a family, a community, or a tribe.[3] Like all other polytheists too the Celts were ready to believe in gods who were not theirs: in the reign of Tiberius the boatmen of Paris set up an altar on which, side by side with their own Esus and Tarvos Trigaranus, were figured Jupiter and Vulcan.[4] The theory, which has been defended with vast if somewhat uncritical erudition, that the king was regarded as an incarnation of the sky-god, may possibly be true both of the Celts and of other Indo-European peoples.[5] Perhaps the Celts, like the Romans, gave more thought to the ritual by which their gods might be persuaded to grant them their hearts' desire

[1] De his eandem fere quam reliquae gentes habent opinionem. *B. G.*, vi, 17, § 2.

[2] See *Rev. des études anc.*, vi, 1904, p. 329. Cf. Sir A. Lyall, *Asiatic Studies*, i, 1899, pp. 2–3, 6.

[3] See W. Robertson Smith, *The Religion of the Semites*, 1901, pp. 16–8, 29, 253–6, 263.

[4] See pp. 273 n. 7, 284, *infra*, and G. Boissier, *La rel. des Romains*, i, 1892, pp. 335, 340–1.

[5] *Folk-Lore*, xvii, 1906, pp. 32, 324. See Mr. A. B. Cook's series of articles in the same volume and in the first number of vol. xviii.

than to the persons of the gods themselves.[1] Doubtless to the Celt, as to the Roman, however little his religion may have fostered nobility of life or contrition for sin, dread of the mysterious was a salutary discipline.[2] But what we want to apprehend is this,—wherein the spirit of Celtic religion differed from that of the religion of ancient Latium, of Greece, of the Semitic tribes ; and if the effort is not wholly vain, we may only hope to attain a distant and hazy view. He who desires to understand the subject will work at it for himself. All that I can hope to do is to put him on the road and to set up a sign-post here and there. The reader who has absorbed what is valuable in the teaching of Tylor, Boissier, Lyall, Frazer, Robertson Smith, Reinach, and Camille Jullian will be best able to discern what is suggested by the texts and monuments that preserve a few fragments of Celtic faith.

Why was the god whom Caesar equated with Mercury honoured above all others by the Continental Celts ? Did the Britons share their devotion ? And is Caesar's statement confirmed ? Some centuries earlier, when the Celts were a host of warriors, the war-god had been the most conspicuous figure in their Olympus ; and his subsequent inferiority to Mercury is regarded, perhaps justly, as an indication of the progress which they had made meantime in the arts of peace.[3] Possibly Lug, the Irish representative of the Gaulish Lugos, whose name appears in Lugudunum, or Lyons, in Luguvallum, or Carlisle, and in Lugotorix, a Kentish chieftain,[4] and who in an Irish legend figures as a carpenter, a smith, a harpist, a poet, and a musician, may have been the British Mercury ;[5] but we cannot tell whether he ranked higher than Mars. Assuming that votive stones

[1] W. Warde Fowler, *The Roman Festivals*, 1899, p. 333.

[2] *Ib.*, p. 347. Cf. W. Robertson Smith, *The Religion of the Semites*, 1901, p. 64.

[3] J. Rhys, *Celtic Heathendom*, p. 49 ; G. Dottin, *La rel. des Celtes*, p. 12. Mercury was also reverenced more than any other god by the Germans of whom Tacitus wrote (*Germ.*, 9).

[4] *B. G.*, v, 22, § 3.

[5] H. d'A. de Jubainville, *Les Celtes*, pp. 39-40, 44. Cf. J. Rhys, *Celtic Heathendom*, p. 220.

in some measure reflect the faith of the native Celts, Mars was deeply reverenced in Britain. He appears with various epithets, the names of Celtic deities, one of which, *Camulus*, meaning 'the god of heaven',[1] was commemorated in Camulodunum, and perhaps bears witness to his former greatness. It is remarkable, in view of Caesar's statement, that in British inscriptions the name of Mercury is far less common than that of Mars;[2] but if the discrepancy is at all connected with the comparative backwardness of British civilization, it must also be remembered that the organization of Britain under Roman rule was military.[3] One religious custom indeed, of which Caesar himself witnessed examples, proves that Mars, however inferior he may have been to Mercury, had still many fervent worshippers in Gaul. When the warriors of a Gallic tribe had made a successful raid, they used to sacrifice to Mars a portion of the cattle which they had captured; the rest of their booty they erected in piles on consecrated ground. It rarely happened that any one dared to keep back part of the spoil; and the wretch who defrauded the god was punished, like Achan, by a terrible death.[4] Another British epithet of Mars, Toutates,[5] appears with Esus and Taranis in a famous passage of Lucan,[6] where they stand out as representative deities, in whose honour dreadful rites were performed. None of the three, save Esus,[7] is mentioned in Gallic inscriptions, whereas Epona,

[1] J. Rhys, *Celtic Heathendom*, pp. 39, 41–2.

[2] M. Camille Jullian (*Rev. des études anc.*, iv, 1902, p. 109, n. 1) points out that in vol. vii [p. 331] of the *Corpus inscr. Lat.* there are sixty-one inscriptions in honour of Mars [of which, however, eight are uncertain], and only eight in honour of Mercury; and the greater popularity of Mars is also apparent in the supplements published in *Ephemeris epigraphica* (iii, 1877, pp. 125, 128; iv, 1881, p. 196; vii, 1892, pp. 289, 299, 313, 324, 332, 334, 352). But no account should be taken of those inscriptions in which the name of Mars is not coupled with that of a Celtic deity, though even with this reservation the ascendancy of Mars remains unaffected.

[3] See *Rev. des études anc.*, iv, 1902, p. 109, n. 1. Even in Gaul the cult of Mars appears to have preponderated among the Aquitani (*ib.*, pp. 106–7, and *Corpus inscr. Lat.*, xiii, 87, 108–17, 209–13).

[4] *B. G.*, vi, 17, §§ 3–5. Cf. J. Rhys, *Celtic Heathendom*, pp. 49–50.

[5] *Corpus inscr. Lat.*, vii, 84.

[6] *Pharsalia*, i, 445–6.

[7] There is no trace of the worship of Esus in the British Isles, unless M. d'Arbois

the goddess of equitation, a minor deity, whose statues, representing a woman riding upon a mare, or seated between foals, have been found both in France and Britain,[1] appears ten times; and accordingly a distinguished French archaeologist concludes that they were insignificant objects of local worship.[2] But it is not credible that the devotee who composed his inscription to Toutates should have unwittingly ascribed to a mere local god the qualities of Mars. Again, if Taranis was not one of the greater gods, it is surprising to find in Britain an inscription in honour of Jupiter Tanarus,[3]—Jove the Thunderer. Nor is it likely that Lucan should have learned the names of the trinity whom he made famous unless their worship had been national.[4] But it does not follow that Tanarus was the Jupiter of the independent Celts. Tanarus, being the Thunderer, was assimilated to the Roman Jupiter; and perhaps the Jupiter Tanarus whose inscription was found at Chester may have been an outcome of the Roman Jupiter and of a Gallic divinity who is known as the god of the wheel.[5] Statues have been discovered in France, representing a god with a wheel on his shoulder, in

de Jubainville (*Les Celtes*, p. 63) is right in thinking that Esus was a god whose surname was Smertullos, and that Smertullos, the Celtic Pollux, is to be identified with the Irish Cuchulainn (see also *Fragm. hist. Graec.*, ed. Didot, i, 1841, p. 194, fr. 6; Diodorus Siculus, iv, 56, § 4; *Corpus inscr. Lat.*, xiii, 3026 *c*; and H. d'A. de Jubainville, *Principaux auteurs à consulter sur l'hist. des Celtes*, p. 88). Esus is depicted as a woodman in the act of felling a tree on No. 2 of four altars which were discovered at Paris in 1710; while Smertullos appears on the right of No. 3, threatening a serpent with a club. M. d'Arbois is a little rash in concluding (*La civilisation des Celtes*, 1899, p. 173) that because there was a Briton called Esunectus, who may have been an immigrant from Gaul, Esus was worshipped in Britain. The name AESV occurs on a coin of the Iceni; but its meaning is uncertain (J. Evans, *Coins of the Anc. Britons*, p. 386). The scholiasts of Lucan identified Esus with Mercury; but their authority on such a matter is worthless (see *Rev. celt.*, xviii, 1897, p. 117). Prof. Rhys, however, has recently examined an inscription (*Celtic Inscr. in France and Italy*, 1907, p. 56), which leads him to give a qualified support to the identification.

[1] *Corpus inscr. Lat.*, vii, 747, 1114*d*; H. Gaidoz, *Esquisse de la rel. des Gaulois*, p. 12; W. H. Roscher, *Lex. der griech. und röm. Mythol.*, i, 1884–6, col. 1286–93; *Rev. arch.*, 3e sér., xxvi, 1895, pp. 309, 317; 4e sér., ii, 1903, pp. 348–50; *Rev. des études anc.*, vii, 1905, pp. 234–8.

[2] *Rev. celt.*, xviii, 1897, pp. 140–1. [3] *Corpus inscr. Lat.*, vii, 168.

[4] My criticism of M. S. Reinach's theory is supported, I am glad to see, by M. Jullian (*Rev. des études anc.*, v, 1903, pp. 217–9).

[5] H. Gaidoz, *Études de mythologie gaul.*,—*Le dieu gaul. du soleil*, &c., pp. 96–7.

his hand, or at his feet; and this god was assimilated in imperial times to Jupiter. Altars on which wheels are represented have also been found in the north of England; and miniature wheels of gold, silver, bronze, and lead—alone, or forming parts of ornaments or helmets, or stamped on coins—have been met with in scores both in France and England. Probably they had a religious meaning; and it has been supposed that they are symbolical of sun-worship, and that the god with the wheel was the god of the sun.[1] Traces of sun-worship are still discernible in the May and midsummer festivals which are kept up in our own island and in many European lands.[2]

Of the other great deities Minerva appears in Irish legend under the name of Brigit[3], possibly the same goddess as Brigantia, in whose honour several inscriptions were erected in Britain,[4] although in Gaul, unless perhaps in the name of the town Brigantium, there is no trace of her worship;[5] while Apollo was assimilated by Roman or Romano-British devotees sometimes to Maponus, whose name survives in the familiar Welsh Mabon[6], sometimes to Grannos, in whose honour an inscription was set up near Edinburgh.[7] There are also vestiges of the cult of a god who resembled Neptune. At Lydney, on the western bank of the Severn, in the country

[1] H. Gaidoz, *Études de mythologie gaul.*, — *Le dieu gaul. du soleil*, &c., pp. 7, 61-3, 66, 92, 96; *Corpus inscr. Lat.*, vii, 879, 882; J. Rhys, *Celtic Heathendom*, pp. 55–6; *Class. Rev.*, xvii, 1903, p. 420; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 60, 136, 152; *Rev. des études anc.*, vii, 1905, pp. 156–7; *Folk-Lore*, xvi, 1905, p. 272, n. 9. The supposition that the wheels were money is no longer admitted by competent antiquaries (A. Blanchet, *Traité des monn. gaul.*, pp. 27–8).

[2] J. G. Frazer, *Golden Bough*, iii, 1900, p. 326.

[3] J. Rhys, *Celtic Heathendom*, pp. 74–5.

[4] *Corpus inscr. Lat.*, vii, 200, 203, 875, 1062. Cf. W. H. Roscher, *Lex. der griech. und röm. Myth.*, i, 1884–6, col. 819, and H. d'A. de Jubainville, *Les Celtes*, p. 35. [5] *Ib.*, p. 33. Cf. J. Rhys, *Celtic Inscr. in France and Italy*, p. 11.

[6] *Corpus inscr. Lat.*, vii, 1345; *Trans. Cumberland and Westmorland Ant. and Archaeol. Soc.*, xv, 1899, p. 463.

[7] *Corpus inscr. Lat.*, vii, 1082. 'On se tromperait grandement,' says M. d'A. de Jubainville (*Les Druides*, 1906, p. 68), 'si l'on croyait qu'il y eut entre le dieu gaulois *Belenus* . . . et les dieux gaulois *Grannos* et *Borvo* [all of whom were assimilated to Apollo] . . une analogie quelconque . . . Le dieu *Maponus*, "jeune fils", n'avait probablement de commun avec Apollon que la jeunesse éternelle.'

of the Silures, a temple was built in Roman times to Nodons, whose name reappears in Welsh legend as *Lludd* and again in our Ludgate Hill. The marine scenes which are depicted in mosaic on the floor seem to show that he was a god of the sea ;[1] while the structure of his temple may justify the conjecture that he was likewise a Jupiter, even as the Italian Jupiter was god of sea as well as of storm and sky.[2] In Gaul he was unknown ; and an eminent Celticist has assumed that he was peculiar to the Goidelic Celts.[3] On the other hand, Toutates, Taranis, Epona, and Belisama were apparently unknown on Goidelic soil.[4] But it profits little to dispute about names. It does not follow that the Goidels did not recognize somewhat similar deities akin to these ; and Belisama was simply the goddess who in Roman Gaul was identified with Minerva.[5]

Caesar, in a familiar passage,[6] tells us that the Gauls regarded themselves as descendants of Dis Pater, who was conspicuous in the old Latin pantheon as the god of the dead, although in Caesar's time he had been dethroned by the Pluto who was imported from Greece.[7] Several Gallo-Roman images, the best known of which is on an altar discovered at Sarrebourg,[8] represent a god with a hammer :

[1] Prof. Rhys (*Celtic Heathendom*, p. 126) says that ' most of the remains of antiquity connected with his temple make him a sort of Jupiter ', but adds (*ib.*, p. 130) that he ' was not simply a Neptune . . . he was also a Mars, as the inscriptions at Lydney testify '. But the testimony of the inscriptions (*Corpus inscr. Lat.*, vii, 138–40) consists simply in the letter M ; and Hübner, to whom the professor appeals, queries his own suggestion that M stands for *Marti*. [I learn from one of Mr. A. B. Cook's articles in *Folk-Lore* (xvii, 1906, p. 39, n. 1) that Hübner (*Jahrbuch des Vereins von Alterthumsfreunden im Rheinlande*, Heft lxvi, 1879, pp. 29–46) corrected and supplemented the account of Nodons which he had given in the *Corpus*, and interpreted *D. M. NODONTI* as d(eo) *m(agno)*—' the great god '—a reading which would authorize us to regard him, with Mr. Cook, as ' a Jupiter and a Neptune rolled into one '.]

[2] *Folk-Lore*, xvii, 1906, pp. 30, 39.

[3] H. d'A. de Jubainville, *Les Celtes*, pp. 33–5.

[4] *Ib.*, pp. 54–6.

[5] J. Rhys, *Celtic Inscr. in France and Italy*, p. 14.

[6] *B. G.*, vi, 18, § 1. Cf. Tacitus, *Germ.*, 2.

[7] C. Jullian in Daremberg and Saglio, *Dict. des ant. grecques et rom.*, ii, 1892, p. 280. Cf. *Bull. de l'Acad. des inscr.*, 1887, p. 443, and *Rev. arch.*, xx, 1892, pp. 208, 213.

[8] *Rev. celt.*, xvii, 1896, pp. 45–59. Cf. G. Dottin, *La rel. des Celtes*, pp. 21–2. The Celtic name of the god on the altar at Sarrebourg was Sucellos.

a bronze statue of the same deity has been found in England;[1] and eminent French archaeologists believe that this was no other than Dis Pater.[2]

But we must not imagine that these gods had always been distinct, or even that in Caesar's time their physiognomies were sharply outlined. When we see that the Germans whom he encountered worshipped Sun, Moon, and Fire,[3] and that those whom Tacitus described had their Mars and Mercury,[4] we may be inclined to suspect that Celtic ideas, under classical influence, had undergone a like transformation.[5] In polytheism divers attributes of deity tend to become separate deities.[6] Jupiter, Mars, and Quirinus were, it would seem, only specialized forms of the same god;[7] and some of the Celtic epithets which are attached to Minerva, Mars, and the rest may mean that they were assimilated by this or that tribe to topical divinities.[8] Dis Pater was certainly near of kin to Saturn,—that old Italian chthonian divinity;[9] and Dis Pater and Toutates, 'the god of the people,' who was perhaps primarily conceived as a kind of Saturn,[10] may once have been one; indeed there seem to be

[1] C. de Clarac, *Musée de sculpture ant. et mod.*,—Planches, t. iii, 1832–4, pl. 398 [670]; *Comptes rendus . . . de l'Acad. des inscr.*, 4e sér., xv, 1887, p. 444.

[2] S. Reinach, *Antiquités nat.*,—*Descr. raisonnée du musée de St. Germain-en-Laye*, pp. 137, 156–68; H. Gaidoz, *Le grand dieu gaul. chez les Allobroges*, 1902, p. vi. Cf. J. Rhys, *Celtic Heathendom*, p. 81, and *Folk-Lore*, xvi, 1905, p. 273. Dis Pater is identified by Professor Rhys and M. G. Bloch (E. Lavisse, *Hist. de France*, i, 51–2) with Cernunnos (see p. 284, *infra*). Cf. W. Warde Fowler, *The Roman Festivals*, p. 286.

M. H. Gaidoz (*Rev. arch.*, 3e sér., xx, 1892, p. 213) says that the worship of Dis Pater in Britain is attested—it hardly needs attestation—by two inscriptions (*Corpus inscr. Lat.*, vii, 154, 250). The former is not worth quoting. The latter—one of many inscriptions addressed to the *Di Manes* which are contained in the *Corpus* and in *Ephemeris epigraphica* (vols. iii and vii) contains the words *Secreti Manes qui regna Acherusia Ditis incolitis.*

[3] *B. G.*, vi, 21, § 2.

[4] *Germ.*, 9.

[5] *Rev. des études anc.*, iv, 1902, p. 228; v, 1903, p. 106.

[6] See G. Boissier, *La rel. rom.*, i, 6.

[7] *Class. Rev.*, xviii, 1904, pp. 361, 367–72, 375; *Folk-Lore*, xv, 1904, p. 264; xvi, 1905, p. 321; xvii, 1906, p. 30.

[8] *Rev. des études anc.*, iv, 1902, p. 221.

[9] *Ib.*, v, 1903, p. 110.

[10] *Ib.*, vi, 1904, pp. 111 n. 1, 134 n. 4; A. Holder, *Alt-celtischer Sprachschatz*, ii, 1805–6.

indications that from one point of view Dis Pater was Jupiter,—a Jupiter of the nether world.[1] Again, if Toutates in Britain remained Mars, while in Gaul the Romanized Celts seem to have hesitated whether to identify him with Mars or Mercury, one is tempted to conjecture that he may have been the common ancestor of both.[2]

No deities were nearer to the hearts of Celtic peasants than those who were known as *deae matres*,—the mother goddesses. Once they were thought to belong to Germans and Celts alone;[3] but their statues have been found in numbers at Capua; and, slightly modified, they survived into the Middle Age. Generally figured in groups of three—a mystic number[4]—their aspect was that of gentle serious motherly women, holding new-born infants in their hands, or bearing fruits and flowers in their laps; and many offerings were made to them by country folk in gratitude for their care of farm and flock and home.[5]

Besides the gods whose cult was common to all the Celtic peoples or to one or the other of the two great stocks were local deities innumerable. We know that the Gallic cities, Bibracte[6] and Lugudunum,[7] had their divine patrons; and it is probable that every British town had its eponymous hero.[8] The deities, however, from whom towns derived their names were doubtless often worshipped near the site long before the first foundations were laid: the goddess Bibracte was originally the spirit of a spring reverenced by the peasants of the mountain upon which the famous Aeduan town was built.[9] Perhaps we shall not err if we also suppose that the heads of his slain enemies, which the Celtic brave religiously treasured and fastened upon the walls of his

[1] *Folk-Lore*, xvii, 1906, pp. 59, 71.
[2] *Rev. des études anc.*, iv, 1902, pp. 110–4.
[3] G. Dottin, *Manuel pour servir à l'étude de l'ant. celt.*, pp. 234–5.
[4] See *Rev. celt.*, xxv, 1904, pp. 130–1.
[5] *Corpus inscr. Lat.*, vii, 168 *a*, 221, 348, 559; *Ephemeris epigr.*, iii, 1877, p. 120; iv, 1881, p. 198 *a*; *Rev. des études anc.*, viii, 1906, pp. 53–8.
[6] *Rev. celt.*, i, 1870–2, pp. 306–19.
[7] *Corpus inscr. Lat.*, xiii, pars i, fasc. i, p. 249.
[8] J. Rhys, *Celtic Heathendom*, p. 99.
[9] *Rev. celt.*, i, 1870–2, pp. 306–19.

cottage, were offered to his household gods or to the spirits of his ancestors.[1]

The worship of animals, to those who have not felt the fascination of anthropology, appears merely unintelligible and absurd. Animals were worshipped because they were formidable or wonderful; because men fancied that they were incarnations of deity; because they might be tenanted by the souls of heroic forefathers;[2] and animal-worship, or a relic of animal-worship, which may perhaps, in some cases, have been a survival of totemism, has left vestiges in Celtic art. The boar was especially sacred. Bronze figures of boars have been found alone and on the crests of helmets: the Witham shield, as we have seen, was decorated with the figure of a boar; and so are numerous coins, both Gallic and British.[3] Like the Romans, the Gauls and doubtless also the Britons had military standards: like the Romans also, they carried not a flag but the figure of an animal, and with them this animal was always the boar.[4] A reminiscence of animal-worship is probably also discernible in the horned head of Cernunnos, a god who is figured on one of the well-known altars of Paris, and in Tarvos Trigaranus—'the bull with the three cranes'—which fills the back of another.[5]

But votive altars, statues, and temples, although they embodied older beliefs, belong, as we have seen, to the period when the Celts had fallen under the dominion of

[1] Diodorus Siculus, v, 29, § 4; *Rev. celt.*, viii, 1887, pp. 47, 59, n. 13; H. d'A. de Jubainville, *La civilisation des Celtes*, pp. 374–5; *Rev. des études anc.*, v, 1903, p. 252.

[2] See E. B. Tylor, *Prim. Culture*, ii, 1903, pp. 229–34.

[3] J. Evans, *Coins of the Ancient Britons*, p. 121, Suppl., p. 477; Cf. *Rev. celt.*, xxi, 1900, pp. 297–9.

[4] *B. G.*, vii, 88, § 4; E. Desjardins, *Géogr. de la Gaule rom.*, iii, 1890, pl. xii; S. Reinach, *Répertoire de la statuaire grecque et rom.*, ii, 746–7; H. d'A. de Jubainville, *La civilisation des Celtes*, 1899, pp. 390–1; *Rev. des études anc.* vi, 1904, p. 48.

[5] *Corpus inscr. Lat.*, xiii, 3026*b*, *c*. Cf. G. Dottin, *La rel. des Celtes*, pp. 20–1, 28, and *Rev. celt.*, xxvi, 1905, p. 199. M. d'Arbois de Jubainville (*ib.*, p. 195) thinks that the original Epona was the mare deified, and that the woman in the statues was a Greek addition. Cf. A. Lang, *Custom and Myth*, 1885, pp. 118–20, and Sir A. Lyall's *Asiatic Studies*, i, 1899, p. 18.

Rome. The Cisalpine Gauls, if Livy[1] and Polybius[2] are to be believed, worshipped in temples: but the holy places of the Western Celts were groves,[3] and perhaps stone circles which they inherited from the people of the Bronze Age. Such simplicity was of course not peculiar to the Celts and the Germans.[4] The Pelasgian Zeus had no temple: the oldest sanctuary of Jupiter on the Alban Mount was a grove of oaks.[5] Not a single statue of pre-Roman date has ever been found in Britain; not one in Gaul later than the close of the Palaeolithic Age. Caesar indeed says that the Gallic Mercury was represented by numerous *simulacra*; but if these were statues, it is inexplicable that none of them has ever come to light; and perhaps we may accept the suggestion that Caesar was thinking of menhirs, which had been erected long before the first Celt set foot in Gaul,[6] but which, like the formless stones that the Greeks venerated as figures of Hermes,[7] were, he supposed, regarded as possessed by the spirit of the great national deity. On the menhir of Kernuz in Finistère a rude Mercury was sculptured in Roman times.[8] The conjecture may be well founded that the

[1] xxii, 57, § 10; xxiii, 24, § 11.

[2] ii, 32, § 6.

[3] *B. G.*, vi, 13, § 10, 17, § 5; Tac., *Ann.*, xiv, 30; Dion Cassius, lxii, 7, § 3. Cf. G. Dottin, *La rel. des Celtes*, p. 30. Strabo (iv, 4, § 6), Diodorus Siculus (v, 27, § 4), Plutarch (*Caesar*, 26), and Suetonius (*Divus Iulius*, 54) speak of temples in Transalpine Gaul; but all archaeologists would admit that the words which they used—*τέμενος*, *ἱερόν*, *fanum*, and *templum*—did not denote roofed edifices. I think, however, that Livy (xxii, 57, § 10, xxiii, 24, § 11) had such buildings in mind. Whether he was well informed is another question. Cf. *Rev. des études anc.*, iv, 1902, pp. 279–80.

[4] Tacitus, *Germ.*, 9.

[5] Livy, i, 31, § 3. Cf. W. Warde Fowler, *The Roman Festivals*, pp. 338–9, and J. G. Frazer, *Early Hist. of the Kingship*, pp. 210–1.

[6] *Rev. celt.*, xiii, 1892, pp. 190–3. Cf. vol. xi, 1890, p. 225. M. d'A. de Jubainville (*Rev. arch.*, 4e sér., viii, 1906, p. 146) says that 'la vie de Saint Samson désigne par le mot *simulacrum* une pierre levée, *lapis stans*, qui était l'objet d'un culte en Grande-Bretagne au milieu du vie siècle', &c.

[7] Pausanias, vii, 22, § 4.

[8] M. Jullian (*Rev. des études anc.*, iv, 1902, pp. 284 n. 6, 285 n. 1), referring to the passage in which Lucan (iii, 412–3) describes the Druids' grove near Massilia,—

simulacraque maesta deorum
Arte carent caesisque exstant informia truncis,

and interpreting it differently from M. Reinach, argues that Caesar's *simulacra*

Druids, like the priests of Israel, were opposed to anthropomorphism;[1] but it is not needed to explain the lack of native statues of Celtic gods.[2] The Romans, according to Varro, had for many years no sacred images:[3] like the Celts, like the Germans, who also, even in the time of Tacitus,[4] deemed it derogatory to the majesty of the gods to ascribe to them human form, they were content to recognize manifestations of divine will; and even when their temples were being crowded with the works of Greek art, their ancient Vesta remained shrouded in awful mystery.[5] But, while the Druids may have been as hostile as Israel to Gentile abominations, the Celts in general were as receptive as the Romans, and readily accepted the services of foreign sculptors.

Sepulchral usages.

The evidence of interments, from which we tried to glean some information as to the religion of the Bronze Age, remains much the same during the later period; and the noticeable changes do not seem to have much significance. British customs differed somewhat from those of Gaul. Inhumation, which had almost entirely ceased in that country in the second century before Christ, continued everywhere in Britain except in the territory of the Belgae; and even there cremation was not universal.[6] In the more

'ne peut signifier que des objets ayant déjà vaguement l'aspect de forme humaine'. In regard to the 'statues—menhirs', which the abbé Hermet (*Congrès internat. d'anthr. et d'archéol. préhist.*, 1900 [1902], pp. 335–8) regards as figures of divinities, see p. 200, *supra*, and cf. E. B. Tylor, *Prim. Culture*, ii, 1903, p. 168.

[1] *Rev. celt.*, xiii, 1892, p. 199.

[2] M. d'A. de Jubainville (*ib.*, xxvii, 1906, p. 122) argues that the absence of pre-Roman Gallic statues is due not to Druidical influence but to the fact that the Gauls built their houses not of stone but of wood, and were therefore ignorant of the art of sculpture! But houses built of stone have been found at Bibracte. See *Congrès internat. d'anthr. et d'archéol. préhist.*, 1900 (1902), pp. 418–9.

[3] Augustine, *De civ. Dei*, iv, 31. [4] *Germ.*, 9.

[5] G. Boissier, *La rel. rom.*, 1892, pp. 8, 35. Cf. Ovid, *Fasti*, vi, 295.

[6] See *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 115.

M. Camille Jullian (*Rev. des études anc.*, v, 1903, p. 251, n. 1) maintains that Caesar (*B. G.*, vi, 19, § 4) does not say that the rich were cremated, but only their slaves. M. Jullian's interpretation of this well-known passage is, I believe, unique; anyhow, the statement in the text rests upon certain archaeological evidence. See *Rev. celt.*, xx, 1899, pp. 119–20; *Rev. de synthèse hist.*, 1901, p. 50; and *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 84.

southern districts nearly all the interments which have been explored were unmarked by any tumulus; while in the cemetery of Aylesford the urns which contained the cremated remains were placed in small cylindrical pits set in what has been described as a family circle.[1] When barrows were erected their form was still circular: but they were generally

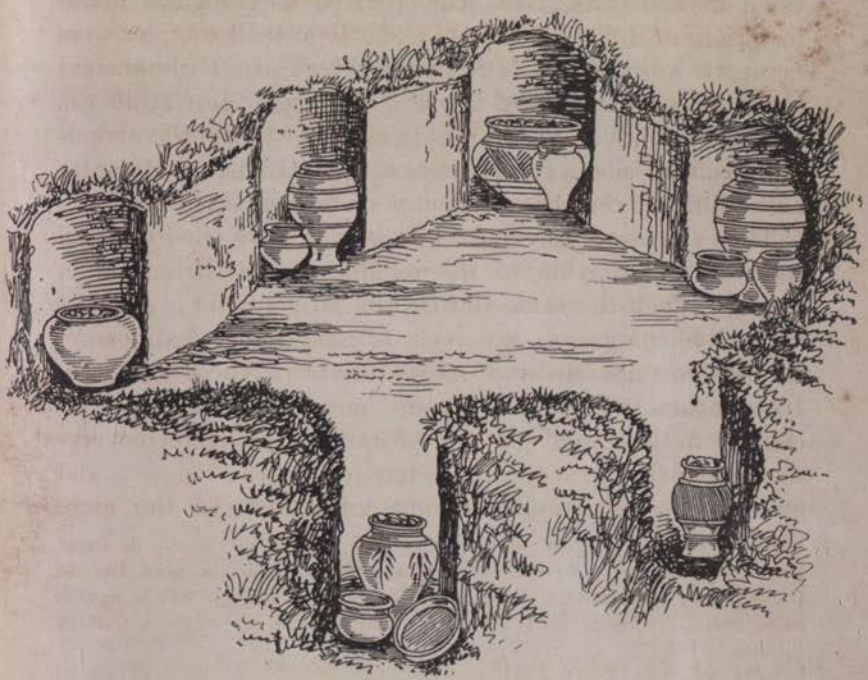

FIG. 44.

much smaller than those of the Bronze Age: they were grouped in much greater numbers;[2] and they were never more than structureless heaps of earth or stone.[3] Although the contracted position was still common, skeletons have been found extended in this country, as generally in Gaul;[4] and, as in Wiltshire in the Bronze Age, the head generally

[1] *Archaeologia*, lii, 1890, pp. 320, 322, 325.

[2] J. R. Mortimer, *Forty Years' Researches*, p. 357.

[3] J. Anderson, *Scotland in Pagan Times,—the Bronze and Stone Ages*, p. 229.

[4] *Crania Britannica*, ii, pl. 6 and 7, pp. 1–3; *Archaeol. Journal*, xliv, 1887, p. 271; *Archaeol. Cant.*, xxvi, 1904, pp. 11–2; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 109.

pointed towards the north.[1] On the other hand, ornaments and weapons were placed in graves more frequently than before:[2] animals were still occasionally interred;[3] and flint chips and stones were still sometimes deposited in or along with urns.[4] But rites which in the Bronze Age could only be inferred are attested in the Iron Age by eye-witnesses. We learn from Caesar[5] that it was a custom of the Gauls to immolate the dead man's cherished possessions, even his favourite animals, on the funeral pyre; and that not long before the time of his oldest contemporaries slaves and retainers had been sacrificed.

The most remarkable perhaps of the sepulchral discoveries that illustrate this period appears to show that old persisted along with new. Hard by the family circles of the Aylesford cemetery, Dr. Arthur Evans opened three cists, each containing a contracted skeleton, the upper slab of one being pierced with a hole which may perhaps have been intended to let the ghost escape;[6] while almost side by side with elegant Late Celtic vases he picked up fragments of the old-fashioned finger-dented ware, including a drinking-cup and a cinerary urn.[7]

[1] *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 106–7, 110–1. Cf. *Crania Britannica*, ii, pl. 6 and 7, p. 6. Mr. Reginald Smith (*Guide*, &c., p. 112) remarks, in regard to the 'Danes' Graves' near Driffield, in the East Riding of Yorkshire, that 'the bodies lay indifferently on the right or left side, though the majority had the head at the north end of the grave: there was thus', he adds, 'no tendency to face the sun, as in the Bronze period'. Since the bodies, on whichever side they lay, would have faced either the morning or the afternoon sun, Mr. Smith's observation apparently assumes that in the Bronze period corpses were laid so as to face the morning sun, which was far from being an invariable rule. See pp. 188–9, *supra*, and the authorities there cited; also *Wilts Archaeol. and Nat. Hist. Mag.*, x, 1866, p. 101. Unhappily Sir R. C. Hoare, from whom we learn that in Wiltshire corpses were generally laid with their heads pointing northward, omits to say whether they were laid on the right or the left side. [See Addenda.]

[2] J. Romilly Allen, *Celtic Art*, pp. 63–71; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 104–20.

[3] *Ib.*, p. 112.

[4] *Ib.*, p. 122; W. Greenwell, *Brit. Barrows*, pp. 208–12.

[5] *B. G.*, vi, 19, § 4.

[6] Or, as Dr. Evans, who mentions both alternatives, suggests (*Archaeologia*, lii, 1890, p. 326), for the introduction of food. See pp. 115–6, *supra*.

[7] *Archaeologia*, lii, 1890, pp. 324–7. Cf. *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 82–3, and see also W. C. Borlase, *Nenia Cornubiae*, pp. 247–51.

The Druids.

It would be interesting to learn whether any Celtic prophet, like the great preachers of India and Palestine, taught that mercy is better than sacrifice. If we may trust Diogenes Laertius,[1] the Druids bade their disciples not only to fear the gods, but to do no wrong and to quit themselves like men. At all events the study of Celtic religion is inseparable from that of Druidism.

Where did Druidism originate? Caesar, in a well-known passage, remarks that it was believed to have arisen in Britain and to have been imported thence into Gaul;[2] and some scholars accept this tradition as literally true. The earliest extant mention of Druids[3] was made about the commencement of the second century before Christ,—not long after the Belgic conquest of Britain began; and it has been supposed that the conquerors found Druidism flourishing there, and made it known in the land from which they had set out. But the Belgae were not the first Celtic conquerors of Britain; and it is reasonable to suppose that if Druidism was of British origin, it would have been imported into Gaul long before. The common view is that on both sides of the Channel it originated among the neolithic population; and Caesar's words are sometimes explained in the sense that in his time it was more vigorous in Britain than in Gaul, and that Gallic Druids therefore travelled to Britain in order to be initiated into its mysteries. At all events it is not unreasonable to believe that the Celts learned it from some non-Aryan people; for there is nothing to show that the Gauls whom the Romans first encountered had ever heard of it. The Germans, with whom the Celts were long in contact in Central Europe and to whom they were ethnically akin, had no Druids;[4] and although it may be true that the intense devotion to religious observances which Caesar remarked among the mixed population of Gaul[5] did not exceed that of other barbarians,[6] it appeared to him to contrast sharply

[1] *Vitae phil.*, ed. Didot, p. 2, ll. 22–3.
[2] *B. G.*, vi, 13, § 11.
[3] Diogenes Laertius, ed. Didot, p. 1, l. 11.
[4] *B. G.*, vi, 21, § 1.
[5] *Ib.*, 16, § 1.
[6] See *Rev. des études anc.*, iv, 1902, p. 102.

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with the temper of the peoples beyond the Rhine.[1] This spirit led them to connect religion with every act of life: in the chase,[2] in all the operations of war, after victory or defeat, before undertaking an expedition, in selecting the site of a town, the gods were regularly invoked:[3] there was no distinction between the sacred and the profane; or rather, nothing was profane. The contrast which Caesar observed supports the theory of the non-Aryan origin of Druidism.

But was Druidism in Britain universal? The leading Celtic scholar of this country insists that there is no evidence that Druidism was ever the religion of any Brythonic people;[4] and since he assigns almost the whole of Britain south of the firths of Forth and Clyde to the Brythons, he appears to restrict the area of Druidism to a narrow western fringe. This hardly accords with Caesar's statement that Britain was the stronghold of Druidism. Moreover, when Caesar tells us that the Druids were the religious aristocracy of the Gauls, he plainly gives us to understand that Druidism was common to all the peoples who lived between the Seine and the Garonne; and it is certain that among many if not most of these peoples the Gallo-Brythonic element was predominant. Indeed, although it is commonly assumed that the Belgae had no Druids, there is absolutely no ground for the assumption. Caesar often used the word *Galli* in a wider sense, including the Belgae; and it is not improbable that when he was describing the manners and customs of the Gauls and Druidism, which was their most remarkable institution, he intended his description to apply to the Belgae as well.[5] Moreover, the very writer who denies that

[1] *B. G.*, vi, 21, § 1.

[2] Arrian, *De venatione*, 34, §§ 1–3.

[3] See *Rev. des études anc.*, vi, 1904, pp. 47–8, 53, 55, 59–60.

[4] See p. 291, n. 2, *infra*.

[5] 'The political condition of the people of Brythonic Britain,' says Prof. Rhys (*Celtic Britain*, 3rd ed., 1904, pp. 57, 61), 'towards the end of the Early Iron Age and the close of their independence, is best studied in connection with that of Gaul as described by Caesar ... The state of things, politically speaking, which existed in Gaul, existed also most likely among the Belgic tribes in Britain.' That is to say, the professor accepts the political part of Caesar's description as applying to the Belgic and the other Brythonic tribes of both

the Brythons had Druids tells us that Druidism was the religion of the British aborigines and was borrowed from them by the British Goidels; and it is certain that both the aborigines and the Goidels (if they had already reached Britain) survived in considerable numbers in the territory which the Brythons conquered.[1] It is clear therefore that Druidism persisted within the Brythonic area; and that the Brythons held aloof from it is a groundless guess.[2]

Gaul and Britain. Yet he insists that that part of the same description which deals with Druidism, and which is indissolubly connected with the political part, has nothing to do either with the Belgae or the other Brythons.

[1] Professor Rhys virtually admits this when he says that the Brythonic dialect was largely influenced by the language of the aborigines. See p. 452, n. 8, *infra*.

[2] The problem of the origin of Druidism is interesting as an example of the divergence which exists among Celtic scholars upon almost every important question of Celtic religion, and also because it once more illustrates the working of that powerful but erratic engine,—the mind of Professor Rhys. The first known mention of Druidism, the substance of which is reproduced in Diogenes Laertius's *Lives of the Philosophers*, occurred in a work by Sotion of Alexandria, who lived about 200 B.C. From this, M. d'Arbois de Jubainville (*Principaux auteurs de l'ant. à consulter sur l'hist. des Celtes*, 1902, pp. 187–8) infers that the Belgic invaders of Britain found Druidism flourishing there about that date, and transplanted it into the country which they had left, but with which they kept up a constant intercourse. M. d'Arbois has consistently maintained this view for many years; and under his influence Professor Rhys affirmed in 1879 (*Lectures on Welsh Philology*, 2nd ed., pp. 83–4) that Druidism reached Gaul 'undoubtedly through the Belgae who had settled in Britain'. Now, however, the professor rightly holds that the Belgae were preceded in Britain by other Brythons (*Celtic Britain*, 1904, p. 4); and it would seem therefore that the date of the first mention of Druidism gives no clue as to the place where it originated. Moreover, Professor Rhys has long been of opinion that there is 'no proof that any Belgic or Brythonic people ever had Druids' (*ib.*, 2nd ed., 1884, p. 69; 3rd ed., 1904, p. 69; *Report of . . . the Brit. Association*, 1900, p. 894). In 1901, accordingly, he argued (*Celtic Folk-lore*, ii, 623, 685) that the Goidelic invaders of Britain (whose existence, I must remind the reader, is denied by some Celtic scholars) 'got their magic and druidism' from 'the [imaginary] dwarf race of the *sids*' (see p. 391, *infra*). But in 1900 (*The Welsh People*, p. 83) and again in 1902 (*ib.*, 3rd ed.) he affirmed that Druidism had been 'evolved by the Continental Goidels, or rather accepted by them from the Aborigines'. Presumably, then, they already had Druids when they invaded Britain, and had no need to borrow them from the *sids*. By 1904, however, the professor appears to have concluded that Druidism originated independently among the aborigines both of Gaul and of Britain, and that with both it was an inheritance from common ancestors; for, after telling us (*Celtic Britain*, 3rd ed., p. 69) that Druidism 'may be surmised to have had its origin' among 'the non-Celtic natives' of Britain, he goes on to say that it 'possessed certain characteristics which enabled it to make terms with the Celtic conqueror, both in Gaul and in the British islands';

But concerning Druidism as it existed in Britain we have no special information, except the passage in which Tacitus [1] speaks of the cruel rites practised by the Druids of Anglesey. Caesar described Druidism once for all; [2] and since he says that British Druidism was the model and the standard of the Gallic Druids, we can only infer that his description applied in many respects to Britain as well as to Gaul. There the Druids formed a corporation, admission to which was eagerly sought: they jealously guarded the secrecy of their lore; and full membership was only obtainable after a long novitiate. They were ruled by a pope, who held office for life; and sometimes the succession to this dignity was disputed by force of arms. They were exempt from taxation and from service in war. They had, as the priests of a rude society always have, a monopoly of learning. The

while on page 73 he remarks that 'it is hard to accept the belief . . . that druidism originated here', and concludes that 'the Celts found it both here and there [in Gaul] the common religion of some of the aboriginal inhabitants'. But the weary student who hopes to be allowed to acquiesce in this conclusion is distracted by finding that on page 4 of this very book, in which the professor insists that 'there is no proof that any . . . Brythonic people ever had Druids', he affirms that 'traces of [the Goidels] are difficult to discover on the Continent' (*Celtic Britain*, p. 4). This time the conclusion would seem to be that the Gauls, whose Druids Caesar described, were neither Goidels nor Brythons! It is hardly necessary to add that the professor has since satisfied himself (see p. 410, *infra*) that traces of Continental Goidels are abundant.

As we have already seen (p. 114, *supra*), M. S. Reinach (*Acad. des inscr. et belles-lettres*,—comptes-rendus de l'année 1892, 4e sér., xx, 6-7) attributes the megalithic monuments of Gaul to Druidical influence, arguing that their construction is inexplicable except on the hypothesis of 'une aristocratie religieuse exerçant un empire presque absolu sur une nombreuse population' (*Rev. celt.*, xiii, 1892, p. 194). Certainly: but if it is a fair conclusion that this hierarchy was composed of Druids, might it not be argued that Druidism was a world-wide institution, or at least co-extensive with rude stone monuments? On the other hand, Professor J. von Pflugk-Harttung (*Trans. Roy. Hist. Soc.*, N. S., vii, 1893, p. 57) can see no reason for supposing that Druidism was originally non-Aryan.

M. Camille Jullian (*Rev. des études anc.*, vi, 1904, p. 260) seems inclined to believe that the priests (*sacerdotes*) of the Cisalpine Boii (Livy, xxiii, 24, § 12) were Druids; and I admit that it is impossible to prove that they were not.

[M. d'A. de Jubainville, in his latest volume (*Les Druides*, p. 13), infers from Caesar's statement, that Druidism originated in Britain, that it was of Goidelic [why not pre-Goidelic ?] origin, and holds (pp. 22-3) that it was imposed by the Goidels upon their Gallo-Brythonic conquerors.]

[1] *Ann.*, xiv, 30. Cf. Pliny, *Nat. Hist.*, xxx, 1 (4), § 13.

[2] *B. G.* vi, 13-4, 16.

ignorance and superstition of the populace, their own organization and submission to one head, gave them a tremendous power. The doctrine which they most strenuously inculcated (if Caesar was not misinformed) was the transmigration of souls. 'This doctrine,' he said, 'they regard as the most potent incentive to valour, because it inspires a contempt for death.'[1] They claimed the right of deciding questions of peace and war. Among the Aedui, if not among other peoples, at all events in certain circumstances, they exercised the right of appointing the chief magistrate.[2] They laid hands on criminals and, in their default, even on the innocent, imprisoned them in monstrous idols of wickerwork, and burned them alive as an offering to the gods. They immolated captives in order to discover the divine will in the flow of their blood or their palpitating entrails;[3] they lent their ministrations to men prostrated by sickness or going forth to battle, who trusted that heaven would spare their lives if human victims were offered in their stead; and one form of human sacrifice which they appear to have countenanced—the slaughter of a child at the foundation of a monument, a fortress, or a bridge—has left many traces in European folk-lore and been practised in Africa, Asia, and Polynesia in modern times.[4] They prac-

[1] Hoc maxime ad virtutem excitari putant metu mortis neglecto (*ib.*, 14, § 5). See p. 295, *infra.*

[2] This statement is, I admit, open to dispute. Caesar (*B. G.*, vii, 33, § 4) does not expressly say that Druids exercised the right in question, but priests (*sacerdotes*); and it has been argued that those priests may not have been Druids (see my *Caesar's Conquest of Gaul*, 1899, p. 534, n. 3, and G. Dottin, *La rel. des Celtes*, p. 41). But, so far as we know, the only other name that designated a priest in Gaul was *gutuater*, which occurs in two Gallo-Roman inscriptions (*ib*, and *Caesar's Conquest of Gaul*, 1899, p. 818); and I doubt whether it is possible to prove that in pre-Roman times the *gutuater* was not a Druid. Anyhow, considering the terms in which Caesar describes the Druids, considering what he says of their power, political and legal as well as spiritual (*fere de omnibus controversiis publicis privatisque constituunt*), I find it difficult to believe that they would have permitted any priest who was not one of themselves to exercise the very important function which he describes in *B. G.*, vii, 33. [For confirmation of the statement in the text see H. d'A. de Jubainville, *Les Druides*, p. 159, who, however (pp. 2–6), insists that *gutuatri* were distinct f om Druids.]

[3] Tacitus, *Ann.*, xiv, 30; Diodorus Siculus, v, 31, § 3.

[4] *Chronica minora*, ed. Th. Mommsen, iii, 1898, p. 182, ll. 14–7. Cf. E. B.

tically monopolized both the civil and the criminal jurisdiction ;[1] and if this jurisdiction was irregular, if they had no legal power of enforcing their judgements, they were none the less obeyed. Primitive states did not originally take cognizance of offences committed against individuals, which were avenged by their kin ; and when they began to intervene they did so at the request of the injured party or his surviving relatives. What was peculiar to the Celts was that this intervention was exercised by the priests ;[2] and doubtless the outlaws who, as Caesar says,[3] abounded in Gaul were criminals whom they had banished. Every year they met to dispense civil justice in the great plain above which now soar the spires of Chartres cathedral.[4] Those who disregarded their decrees were excommunicated ; and excommunication meant exclusion from the civil community as well as from communion in religious rites.

Did the Druids owe their conception of immortality, as Diodorus Siculus[5] and Timagenes[6] imply, to the influence of Pythagoras ? The testimony of these writers has been contemptuously rejected :[7] but it seems not improbable that Druidism may have absorbed tenets of Pythagorean origin through the medium of the Greeks of Massilia ;[8] and this conjecture gains some support from numismatic evidence.

Tylor, *Prim. Culture*, i, 1903, pp. 104–8 ; *Rev. celt.*, xxvi, 1905, p. 289 ; and Sir A. Lyall, *Asiatic Studies*, ii, 1899, pp. 312–3. In regard to the Druidical practice of human sacrifice see N. Fréret, *Œuvres complètes*, xviii, 1796, pp. 264–72 ; *Nouvelle rev. hist. du droit français et étranger*, 1898, pp. 289–300 ; Rice Holmes, *Caesar's Conquest of Gaul*, 1899, p. 533 ; and J. G. Frazer, *The Golden Bough*, iii, 1900, pp. 319–23, 326.

[1] M. G. Bloch (*Rev. internat. de l'enseignement*, Août, 1895, p. 151), referring to Caesar (*B. G.*, vi, 13, § 5), argues that the suitors who appealed to Druids probably all belonged to the upper class (*equites*), who, having unlimited rights over their dependents (*ib.*, § 3), doubtless decided their disputes.

The meaning of the 'awards and penalties' (*praemia poenasque*) which the Druidical judges fixed is uncertain. See H. d'A. de Jubainville, *Études sur le droit celt.*, i, 80–1 ; G. Dottin, *Manuel pour servir à l'étude de l'ant. celt.*, p. 190 ; and Sir H. Maine, *Early Hist. of Inst.*, 1875, p. 136.

[2] *Rev. internat. de l'enseignement*, Août, 1895, pp. 149–50.

[3] *B. G.*, v, 55, § 3. Cf. viii, 30, § 1. [4] *Ib.*, vi, 13, § 10.

[5] *Bibl. hist.*, v, 28, § 6.

[6] Ammianus Marcellinus, xv, 9, § 8.

[7] *E.g.* by Fustel de Coulanges (*Rev. celt.*, iv, 1879–80, p. 53).

[8] H. Gaidoz, *Esquisse de la rel. des Gaulois*, p. 18.

A British uninscribed gold coin, found at Reculver, bears on its reverse side the figure, formed by five interlacing lines, which is known as the pentagram and was a well-known Pythagorean symbol.[1] It would seem, however, that if metempsychosis was really a Druidical doctrine, it had no firm hold upon the Celts in general; and their sepulchral customs were not consistent with it. Their notion of a future life, like that of the Bronze Age, was a form of the 'Continuance Theory', which has had so many adherents both in primitive and modern tribes.[2] They believed that there was an Elysium somewhere in the west, where they were to live again, feasting, carousing, and duelling, a life like that which they had lived before, but free from care.[3] If the Druids, as Caesar said, taught that souls passed 'from one person to another', they meant perhaps that after death the soul entered a new body,—the ethereal counterpart of that which it had left behind. The immortality of the soul

[1] J. Evans, *Coins of the Anc. Britons*, p. 98. The pentagram, which, says Professor Tylor (*Ency. Brit.*, xv, 1883, p. 203), is 'an interesting proof of tradition from the Pythagoreans', has also been found on a more recently discovered British coin (J. Evans, *Coins, &c.*,—Suppl., p. 573); on a bucket in Carnarvonshire (*Archaeol. Cambr.*, 6th ser., v, 1905, p. 256); on a pebble in a broch at Burrian, Orkney (*ib.*); and on Gallic coins of the Carnutes, Senones, Suessiones, and Remi (A. Blanchet, *Traité des monn. gaul.*, pp. 331, 360, 378, 385–6).

M. d'Arbois de Jubainville (*Études sur le droit celt.*, i, 1895, p. 5), referring to Mela, iii, 2, § 19 (*unum ex his quae praecipiunt in vulgus efflυxit, videlicet ut forent ad bella meliores, aeternas esse animas vitamque alteram ad manes. Itaque cum mortuis cremant ac defodiunt apta viventibus*), asserts that the teaching of the Druids differed from that of Pythagoras: they did not inculcate metempsychosis, but merely the immortality of the soul. He also insists, quoting Valerius Maximus, ii, 6, § 10, that, in the belief of the Gauls, the life to come was analogous to life upon earth (cf. N. Fréret, *Œuvres complètes*, xviii, 1796, pp. 182-8). But it is not proved that Caesar, whose authority is higher than that of Mela, and whose testimony is not really contradicted by him, was misinformed when he said that the Druids taught *non interire animas, sed ab aliis post mortem transire ad alios*; and Valerius Maximus himself remarks that the belief of the Gauls was identical with that of Pythagoras. Many Christians, who believe in the immortality of the soul, also believe, or fancy that they believe, in the transmigration of souls. Still, as I have suggested in the text, it is quite possible that even the Druids did not preach the Pythagorean doctrine of metempsychosis, and that Caesar did not intend to convey that they did.

[2] E. B. Tylor, *Prim. Culture*, ii, 1903, pp. 75–83.

[3] *Ib.*, pp. 63–5, 77; *Rev. de l'hist. des rel.*, xiv, 1886, p. 61; G. Dottin, *La rel. des Celtes*, pp. 35–7.

was an idea, more or less vague, common to many peoples: for the Celts the Druids made it an article of faith. Nor indeed are we precluded from supposing that some of them may have conceived or borrowed from a classic source the doctrine of future retribution. But what that theory was which, as Caesar says,[1] the Druids inculcated in regard to the origin of the universe and the nature and motion of the heavenly bodies, it is useless to inquire.[2] We only know that, as they traced the descent of the Gauls back to Dis Pater, they regarded night as older than day, and reckoned time by nights; and that, in common with all the peoples of antiquity, they computed their years by the revolutions of the moon.[3] The statements of Caesar and Pliny are supplemented by a calendar, engraved on bronze, which was discovered towards the end of the last century at Coligny in the department of the Ain.[4] It has its lucky and unlucky days; certain days would be regarded as suitable for sacrifices as well as for other functions;[5] and the regulation of these important matters would certainly have been retained by the Druids. It has been said, perhaps in reliance upon a mistranslation of the word *dryas* or *druias*, that Druidesses taught side by side with Druids:[6] at all events Boadicea

[1] *B. G.*, vi, 14, § 6.

[2] See *Rev. celt.*, iv, 1879–80, pp. 51–2.

[3] *B. G.*, vi, 18, § 2; Pliny, *Nat. Hist.*, xvi, 43 (95), § 250. The Germans had the same notion as the Gauls about night and day (Tacitus, *Germ.*, 11). Cf. N. Fréret, *Œuvres complètes*, xviii, 1796, p. 222, and *Rev. internat. de l'enseignement*, Août, 1895, p. 159.

[4] See *Rev. des études anc.*, v, 1903, p. 127; *Rev. celt.*, xxv, 1904, pp. 115, 118, 121, 131–2, 160; and J. Rhys, *Celtae and Galli*, 1905, pp. 1–4, 8, 21, 35, 46. Of course we have no right to assume that the calendar of Coligny, which was not earlier than the first century of our era, was identical with that of the Britons; but this caution does not invalidate the statements in the text.

The language of the calendar is a subject of dispute. Prof. Rhys and M. Camille Jullian (*Rev. des études anc.*, v, 1903, p. 127) unhesitatingly treat it as Celtic: M. d'Arbois de Jubainville regards the association of *qu* with *p* (see pp. 227–8, *supra*) as proof of its being Ligurian. [Prof. Rhys's latest view (*Celtic Inscr. of France and Italy*, p. 99) is that 'it becomes more and more a question of names, whether it is to be called Celtic or Ligurian'. But the fact remains that history and physical anthropology tend to show that the Ligurians were utterly different from the people among whom the Celtic language came into being. See my *Caesar's Conquest of Gaul*, 1899, pp. 275–81.]

[5] Cf. W. Warde Fowler, *The Roman Festivals*, p. 3.

[6] Lampridius, *Alexander Severus*, 60; Vopiscus, *Aurelianus*, 44, *Numerianus*,

sought to divine the issue of her campaign by observing the movements of a hare, besought the gods to bless her enterprise, and after her success offered female captives to Andate, the goddess of victory;[1] and her joint exercise of royal and priestly functions seems to give colour to the suggestion that in primitive times Celtic kings may also have been priests.[2] Cicero[3] indeed relates that the Galatian King, Deiotarus, was the most skilful augur of his country. But the facts of historical import which stand out as certain are these. Like the Brahmans, who, so long as their authority is acknowledged, recognize, but regulate, the Protean manifestations of Hindu religious fancy,[4] the Druids kept control over the manifold forms of aboriginal and Celtic worship. Being a sacerdotal caste, not, like the priests of Rome, popularly elected, but self-constituted and self-contained, they were naturally opposed to all innovation. It has been said that ancient writers regarded as peculiar to the Druids beliefs and practices which were common to them and other priests of antiquity. Certainly human

14. Cf. *Rev. des études anc.*, vi, 1904, p. 258, n. 6; and J. G. Frazer, *Early Hist. of the Kingship*, p. 224, note

M. d'Arbois de Jubainville (*Rev. celt.*, xxvi, 1905, p. 359) holds that Lampridius and Vopiscus were mistaken in designating as *dryades* women who were mere fortune-tellers, and who should be classed among the μάντεις or soothsayers, mentioned by Diodorus Siculus (v, 31, § 3) or the οὐάτεις who, according to Strabo (iv, 4, § 4), were specially concerned with sacrifices. Μάντεις and οὐάτεις, however, who were doubtless identical, would seem to have been merely Druids of inferior rank (G. Dottin, *Manuel pour servir à l'étude de l'ant. celt.*, pp. 263–4, 267). M. Toutain (*Mélanges Boissier*, 1903, pp. 439–42), who also regards the *dryades* as fortune-tellers, denies that there is any authority for translating the word by 'Druidesses', and insists that if Druidesses had existed, they would not have been mentioned for the first time by writers of the 3rd century. A. Holder (*Alt-celtischer Sprachschatz*, i, 1326, 1329), who prints the quotations from Lampridius and Vopiscus under the heading *Druida*, remarks that in Lucan, i, 451, instead of *druidae* there is a various reading *dryadae*.

[1] Dion Cassius, lxii, 6, § 1; 7, §§ 2–3. Cf. *Rev. des études anc.*, iv, 1902, pp. 224–5.

[2] *Ib.* vi, 1904, pp. 261–2. Cf. J. G. Frazer, *Early Hist. of the Kingship*, p. 31.

[3] *De div.*, i, 15, § 26; ii, 36, § 76. Pliny (*Nat. Hist.*, xvi, 43 [95], § 249) says that the Gallic Druids of his time were *magi*, which is commonly translated by 'magicians' (cf. H. Gaidoz, *Esquisse*, &c., pp. 15–6). But might not the word have been applied to any one who practised augury and divination (Cicero, *de div.*, i, 41, § 90)?

[4] Sir A. Lyall, *Asiatic Studies*, i, 1899, pp. 2, 26, 135, 161.

sacrifice was not peculiar to the Celts: the ceremony of cutting the mysterious mistletoe was German as well as Druidical;[1] and as the Druid sacrificed white bulls before he ascended the sacred oak,[2] so did the Latin priest in the grove which was the holy place of Jupiter.[3] But while every ancient people had its priests, the Druids alone were a veritable clergy.[4] Celtic religion, in so far as it had the same ancestry as that of Rome, would easily harmonize with it; but Druidism, with its more definite theology, might be expected to counteract this tendency, and would therefore be a danger to Roman dominion.[5] And it was British Druidism that supported and renovated the Druidism of Gaul, and formed one of the bonds of union between the two Celtic lands.[6]

[1] See J. G. Frazer, *Golden Bough*, iii, 1900, pp. 328 note, 343–4.

[2] Pliny, *Nat. Hist.*, xvi, 44 (95), § 250.

[3] J. G. Frazer, *Early Hist. of the Kingship*, pp. 212–3.

[4] M. G. Dottin (*La rel. des Celtes*, p. 41), remarking that Druids were also politicians, and that the Druid, Diviciacus, led a life which differed little from that of his brother Dumnorix, who was not a Druid, concludes that 'il ne s'agit donc pas d'une classe sacerdotale, à plus forte raison, comme on l'a dit, d'un clergé gaulois'. Were there then no clergy in England in the days of Becket, or of Wolsey, or in France when Richelieu and Mazarin were supreme?

[5] See H. d'A. de Jubainville, *Les Druides*, pp. 60, 64, which has come into my hands since I wrote this chapter.

[6] The question may be asked, If there were Druids in South-Eastern Britain, why does Caesar not tell us what part they took, or whether they took any part, in the campaign of Cassivellaunus? As all readers of his memoirs have remarked, he is equally silent in regard to the political activity or the political apathy of the Druids of Gaul. 'A singularly powerful priesthood,' says Prof. Haverfield (*Eng. Hist. Rev.*, xviii, 1903, p. 336), 'numbering political leaders, like Divitiacus, among its ranks, might be expected in a national crisis to take some definite line, requiring notice in the *Commentaries*. Yet omit two chapters, and so far as the *Commentaries* go, the Druids might never have existed.' M. Camille Jullian (*Vercingétorix*, 1902, pp. 107–11) argues that they did take an active part in the rebellion of Vercingetorix, but that Caesar chose to ignore the fact: Caesar '*a laïcisé à outrance l'esprit et l'histoire de la Gaule . . . Nul ne croira que la Gaule n'ait pas appelé prêtres et dieux à son secours*'. Prof. Haverfield, who naturally asks 'What motive had Caesar for this?' suggests that an analogy 'to these powerful non-political priests . . . is provided by various priestly *collegia* at Rome, which include political leaders, but which in their augural or other capacity take no political action', and maintains that the Druids, 'as Druids, uttered no word against Caesar or for him'. But if so, why, at a time when their power had certainly diminished, did they aid and abet the insurrection of Civilis (Tacitus, *Hist.*, iv, 54)? I would suggest that Caesar may have bought over the Arch-Druid (*B. G.*, vi,

For, if their material culture was somewhat less advanced, the Britons, at least those of the south-eastern districts, naturally remained connected by the closest ties with the Gauls, and particularly with the Belgae. The Britons of Kent were little less civilized than the Gauls;[1] and Belgic kings, like William the Conqueror and his descendants, ruled on both sides of the Channel.[2] Not many years before the period of the Gallic wars, Diviciacus, king of the Suessiones, who governed directly the country round Soissons, had established supremacy not only over a large part of the surrounding Belgic territory but also over Britain;[3] and during a period which may have coincided with his reign gold coins of certain types were used indifferently in the Belgic districts of Britain and of Gaul, and were doubtless struck for rulers who had possessions in both.[4] But the power of Diviciacus had ended with him;[5] and when Caesar came

Ties between Britons and Gauls.

13, § 8)—and his use of secret-service money is one of the matters which he did not mention—and that if individual Druids did take part in a crusade, he may not have thought their action sufficiently important (if he was aware of it) to be worth recording.

[1] *B. G.*, v, 14, § 1.

[2] *Archaeol. Oxon.*, 1892–5 (1895), p. 159.

[3] The word *Britanniae* (*B. G.*, ii, 4, § 7) is of course used loosely.

[4] J. Evans, *Coins of the Anc. Britons*, p. 83; Suppl., p. 483. See also pp. 51, 63, 65, 90, and 94 of the earlier volume.

[5] Professor Rhys (*The Welsh People*, 1902, pp. 88–90) remarks that 'since no hint as to a revolution is vouchsafed [in Caesar's narrative (*B. G.*, ii, 4, § 7)], the probability is that the empire of Diviciacos in this country subsisted under his successors in Caesar's time. But,' he continues, 'Diviciacos's people were the Suessiones and the Remi; so we should expect to find both of them represented in Britain, though their names have not been detected. Now we know from a couple of inscriptions that a god of the Remi was Camulos.' The professor goes on to observe that Camulodunum 'was near Colchester, in the country of the Trinovantes, in whom we are accordingly prepared to find the Remi we are seeking'; and, he says, 'The next neighbours of the Trinovantes were the Catuvellauni, in whom we probably have our insular Suessiones. At any rate the name of the Catuvellauni was also that which, shortened into *Catelauni* . . . eventually became . . . Chalons, the name of a town . . . in a district usually assigned to the Remi . . . the Catuvellauni and the Trinovantes between them may be regarded as the upholders of the empire of Diviciacos,' &c. But in Caesar's time the Catuvellauni were the bitter enemies of the Trinovantes: Camulos was worshipped by many other tribes besides the Remi; and although it is probable that the Gallic Catuvellauni were clients of the Remi in the time of Caesar (Rice Holmes, *Caesar's Conquest of Gaul*, 1899, pp. 476–7), it is not unlikely that they were one of the tribes which placed themselves under the protection of the Remi in consequence of the

to Gaul, the tribes of South-Eastern Britain were divided into antagonistic groups, headed respectively by the Catuvellauni and the Trinovantes. Cassivellaunus, the king of the Catuvellauni, was the ablest and most aggressive of the British princes of his time; but his opponents were supported, it would seem, by the influence of Commius, a chieftain of the Belgic Atrebates, whose territory comprised adjacent districts of the departments of Pas-de-Calais and Nord, and who were connected with the British tribe of the same name.

How the Britons were affected by Caesar's campaigns in Gaul.

But, if anything could induce the Britons to forget their differences, it was the news which reached them of Caesar's movements in Gaul. The events of the first year of his proconsulship—the overthrow of the Helvetii, who had migrated into Gaul from Switzerland, and the defeat of the German invader, Ariovistus—might not affect their interests: but in the following year, when the Belgae banded together against the Roman conqueror, it was time for them to be on the alert. British adventurers crossed the Straits to assist their kinsmen; and when Caesar shattered the forces of the coalition, the leaders of at least one Belgic tribe fled over sea to escape his vengeance. Late in the autumn of that year or early in the following spring rumours reached the ports of the Channel that Caesar purposed to invade Britain.

favour shown to the latter by Caesar (*B. G.*, vi, 12, § 7). The passage in which Caesar mentions Diviciacus leaves upon my mind the impression that his empire, like that of Celtillus, the father of Vercingetorix, was short-lived. At all events there is no evidence for asserting its continuance.

CHAPTER VI

CAESAR'S FIRST INVASION OF BRITAIN

Caesar obliged to secure his rear before invading Britain.

BEFORE Caesar could venture to undertake so difficult an enterprise as the invasion of Britain, it was necessary for him to secure the country in his rear. His first two campaigns had been directed against enemies who were as dangerous to Gaul as to Rome. Cavalry levied from friendly Gallic tribes fought side by side with the Roman legions against the Helvetii and against Ariovistus: after the defeat of the Helvetii envoys came from all the tribes of Central and Eastern Gaul to congratulate the victor; and after the defeat of Ariovistus the legions took up their quarters for the winter in Gallic territory without resistance. There was probably not a single tribe in which Caesar had not opponents: but the prestige of Rome and of his own victories, the factious spirit and the intertribal jealousies of the Gauls, and above all the sagacity with which he played off party against party, and selected the chiefs who, for their own purposes, were able and willing to serve him, prevented open opposition. Thus, although the seeds of future troubles were even then germinating, he could safely use Celtican Gaul as his base of operations when he crossed the Marne in the following year to encounter the Belgae. The series of victories which he gained in this campaign intimidated his opponents for the time and increased his renown, but had little effect upon the remote maritime tribe of the Morini, on whose coast was the harbour from which he must sail.

He contemplated invasion as early as 56 B.C.

Caesar's first mention of Britain occurs in the chapter that immediately follows his narrative of the operations by which he destroyed the invading hordes of the Usipetes and Tencteri, crossed the Rhine, and chastised the tribe which had given an asylum to their fugitives:—'Only a small part of the summer remained; and in these parts, the whole of Gaul having a northerly trend, winter sets in early: nevertheless

56 B.C. Caesar made active preparations for an expedition to Britain; for he knew that in almost all the operations in Gaul our enemies had been reinforced from that country.'[1] But even if we had not Strabo's explicit statement, it would be unnecessary to argue that Caesar could not have undertaken so momentous an enterprise upon the spur of the moment. Strabo says that the Veneti, who in 56 B.C. formed a coalition of the maritime tribes of North-Western and Northern Gaul against Caesar, made war upon him because they were determined to prevent him from invading Britain, the trade with which was in their hands.[2] The statement is intrinsically probable, and is supported by facts for which we have the authority of Caesar himself. The alliance which the Veneti headed included almost all the maritime tribes between the Loire and the Rhine; and auxiliaries actually came from Britain to join them. It is not credible that the Britons would have crossed the widest part of the Channel, or that the Morini, whose country lay between the Somme and the Scheldt, and the Menapii, whose seaboard reached the Rhine, would have supported the remote Veneti, if they had not had reason to believe that their own interests were imperilled. Moreover, Caesar tells us that among the ships which he assembled for the invasion of Britain were galleys which he had used in the naval action with the Veneti. This action took place off the coast of the Morbihan, the nearest harbour to which was in the estuary of the Loire;[3] and it is needless to argue that the galleys were not there when Caesar sent for them. If only 'a small part of the summer' remained when he began to prepare for the invasion, there was no time for his messengers to travel from the

[1] Exigua parte aestatis reliqua Caesar, etsi in his locis, quod omnis Gallia ad septentriones vergit, maturae sunt hiemes, tamen in Britanniam proficisci contendit, quod omnibus fere Gallicis bellis hostibus nostris inde subministrata auxilia intellegebat, &c. *B. G.*, iv, 20, § 1.

[2] ... Οὐενετοὶ μέν εἰσιν οἱ ναυμαχήσαντες πρὸς Καίσαρα· ἕτοιμοι γὰρ ἦσαν κωλύειν τὸν εἰς τὴν Βρεττανικὴν πλοῦν, χρώμενοι τῷ ἐμπορίῳ (*Geogr.*, iv, 4, § 1).

[3] The estuary of the Loire was the nearest considerable harbour to the scene of the naval battle. It is not likely that Caesar would have sent his fleet to any of the smaller ports in the country of the Veneti (see my *Caesar's Conquest of Gaul*, 1899, p. 665); but supposing that he did so, my argument would hardly be affected.

neighbourhood of Coblenz, where he had crossed and recrossed the Rhine, to the mouth of the Loire, or for the galleys to make the voyage of six hundred miles from the Loire to the north-eastern coast of Gaul. When Caesar's messengers set out, the galleys must have been within a short distance of the port from which he set sail,—probably in the mouth of the Seine or of the Somme. The war which he waged against the Veneti was a necessary prelude to the invasion of Britain. For he could not safely embark his army unless he had command of the Channel; and at the time when he planned the invasion the masters of the Channel were the Veneti. They had a powerful fleet of large vessels, the model of which had, we may suppose, been originally borrowed from that of the merchantmen of the Carthaginians, whose commerce in the Atlantic and in British waters they had inherited. This fleet enabled them to close the ports not only of their own territory in Western Brittany, but also of the western seaboard at least of Northern Gaul; and no one was permitted to use those ports except on condition of paying them toll.

56 B.C. Campaign against the Veneti necessary in order to secure command of the Channel.

But Caesar attempted to gain his object without fighting. After his campaign against the Belgae he sent the 7th legion under Publius Crassus, the younger son of the wealthy triumvir, to winter in the valley of the lower Loire; and all the tribes of Brittany submitted to him and gave him hostages. It was probably about this time that Crassus made his celebrated voyage to the tin-producing districts of Cornwall;[1] and it seems not unreasonable to conjecture that it was the news of his mission which gave the alarm to the Veneti. They arrested two officers whom he had sent to make a requisition of corn: the other maritime tribes of Brittany and Normandy threw in their lot with them; and an embassy was sent to Crassus to demand the restoration of the hostages. Messengers were promptly dispatched to inform Caesar, who had gone to Illyricum. He sent orders to Crassus to have a fleet of war-galleys built in the estuary of the Loire, to summon oarsmen from the Roman Province of Southern Gaul, and to impress seamen and pilots. Mean-

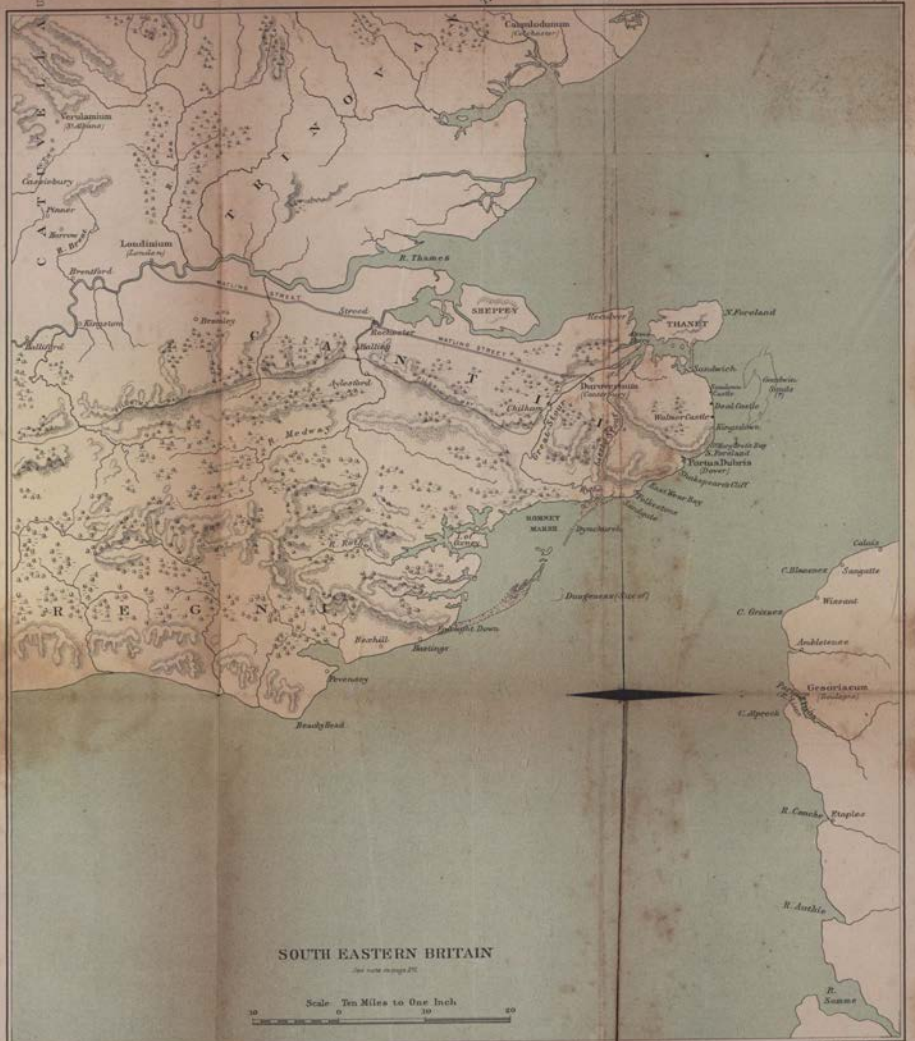
57 B.C.

56 B.C.

[1] See pp. 494–7, *infra*

56 B.C. while the Veneti were engaging fresh allies, and reinforcements were hastening from Britain to join them. The allied fleet was speedily assembled on the coast of the Morbihan. Caesar hurried back to join his army, and on his arrival made all the necessary dispositions for preventing the spread of the insurrectionary movement. Crassus was dispatched southward into the country of the Aquitani, from whom, it is true, little danger was to be expected: another general, Titurius Sabinus, was sent northward into the peninsula of the Cotentin, to prevent the tribes of Northern Brittany and Western Normandy from joining the Veneti; and Labienus, Caesar's most capable lieutenant, marched eastward through the heart of Gaul to the neighbourhood of Treves, with orders to watch the Belgae and repel the German tribes, who were believed to be in communication with the Gauls, in case they attempted to cross the Rhine. Labienus appears to have had little trouble; but Crassus and Sabinus encountered and defeated their respective enemies. Caesar himself invaded Venetia, and entrusted Decimus Brutus with the command of his fleet. During a great part of the summer Brutus was detained in the mouth of the Loire by stormy weather; and Caesar spent the time in endeavouring to reduce the strongholds on the Venetian coast. These operations were fruitless; but on the first fine day the struggle was brought to an issue. The decisive battle was fought in Quiberon Bay.[1] The allied fleet numbered two hundred and twenty sail, while the Roman galleys were reinforced by ships lent by friendly tribes who inhabited the maritime districts south of the Loire. The ships of the Veneti and their allies were so heavy and so stoutly built that it would have been useless for the galleys to attempt to ram them; and they stood so high out of the water that the legionaries were unable to throw missiles with effect. But the Roman engineers came to the rescue as they had done in the First Punic War. Long poles had been prepared, armed at one end with sharp-edged hooks. The galleys were swifter and more mobile than the Gallic ships, which

[1] See my *Caesar's Conquest of Gaul*, 1899, pp. 663–74.



Camulodunum
(Colchester)
Verulamium
(St. Albans)
Cassiobury
Pinner
Harrow
R. Brent
Londinium
(London)
Brentford
R. Thames
WATLING STREET
Streed
SHEPPEY
Kingston
Bromley
Rochester
Halling
Halliford
Aylesford
Chilham
Great Stour
Rochester
THANET
N. Foreland
Sandwich
Sandwich Castle
Goodwin Sands (?)
Deal Castle
Walmer Castle
Kingsdown
St. Mary Bay
S. Foreland
Portus Dubris
(Dover)
Shakespeare's Cliff
East Wear Bay
Folkestone
Sandgate
Dyncheurch
ROMNEY MARSH
R. Rother
Lot Gency
Dungeness (Size of)
Eastbright Down
Hastings
Hoxhill
Pevensey
Beachy Head
R. Medway
Calais
C. Blanenez
Sangatte
Wissant
C. Grisnez
Ambletenae
Gesoriacum
(Boulogne)
C. Alprech
R. Canche
Etaples
R. Authie
R. Somme
SOUTH EASTERN BRITAIN
Scale Ten Miles to One Inch
10 0 10 20

had no oars.[1] When the fleets approached each other, two or more galleys ran alongside one of the enemy's ships; and the halyards were seized by the hooks. Instantly the rowers pulled away: the halyards snapped, and yards and sails fell down, leaving the helpless hulk to be boarded by the legionaries. 'Thenceforward,' wrote Caesar, 'the fight turned upon valour, in which our soldiers easily had the advantage.'[2] When several ships had been captured, the Veneti abandoned the fight and made haste to escape. But their ships had hardly been put before the wind when they were becalmed; and the galleys, running swiftly in and out among them, captured them one after another, all but a few which contrived to reach land when darkness fell. 56 B.C.

The Veneti surrendered unconditionally. Caesar was determined to teach the Gauls that 'the rights of envoys'[3] must be respected in future. The Venetian senate were put to death; and all the tribesmen who failed to escape were sold into slavery.

Campaign against the Morini.

It remained only to subdue the Morini, who had never yet acknowledged the supremacy of Rome. Caesar marched against them: but the season was too far advanced; and he found it impossible to strike a decisive blow. The Morini would not risk a battle, and took refuge in their forests. Caesar allowed himself to be surprised on the outskirts and lost a few men, though he succeeded in punishing his assailants; and after the legions had spent some days in cutting down trees, capturing baggage, and driving off cattle, stormy weather set in, and rain fell so heavily and continuously that they could no longer live safely in tents, and were forced to abandon the campaign. Owing to this failure, which Caesar hardly atoned for by ravaging the cultivated lands as he retreated, the base of operations for the expedition which was to take place in the following year was still insecure. On the other hand, the maritime tribes between the Somme and the Pyrenees were effec-

Its failure leaves Caesar's base not quite secure.

[1] Except perhaps sweeps, which they may have used occasionally to help them in tacking.

[2] Reliquum erat certamen positum in virtute, qua nostri milites facile superabant. *B. G.*, iii, 14, § 8.

[3] ius legatorum. *Ib.*, 16, § 4.

tually subdued; and Caesar was absolute master of the sea.

55 B.C. Caesar determines to sail from the Portus Itius (Boulogne.)

When the campaign of the following year against the Germans was over, Caesar marched westward into the country of the Morini, 'because,' as he tells us, 'the shortest passage to Britain was from their coast.'[1] Probably he had already ascertained what was the best port to sail from; but any competent cavalry officer could have procured the information in a couple of days. Between the Scheldt and the Somme there was only one harbour which would satisfy all his requirements. Calais did not then exist: Sangatte, on the east of Cape Blancnez, was at best a mere roadstead; and the sandy waste between Cape Blancnez and Cape Grisnez, from which the village of Wissant derives its name, though it possessed two tiny creeks formed by rivulets, offered no shelter for a fleet and no facilities for building or repairing ships, or for provisioning an army. The Canche, the Authie, and the Somme, if at that time they were used as harbours, were too far from Britain. But the estuary of the Liane, on whose right bank stood Gesoriacum, the village whose site is now covered by Boulogne, combined every advantage. Caesar, Latinizing its Celtic name—the port of Icht, or 'the Channel harbour'—called it the Portus Itius. Gallic merchants sailed from it to the ports of Kent: from the time of Augustus it was the Roman port of embarkation for Britain, and at a later period the naval station of the Roman Channel Fleet. The estuary, longer, wider, and deeper than it is now, was protected from every gale by the bold bluff of land which on the west throws out the promontory of Alprech, and which then projected northward considerably beyond its present limit.[2] Vessels of light draught could enter the harbour at low tide. Shipyards lined its banks. Roads connected it with the interior; and timber in abundance could be floated down the river from the forest of Boulogne. The heights that look down from the east upon the harbour, about half a mile south of the column which commemorates the assemblage of Napoleon's 'Grand

[1] quod inde erat brevissimus in Britanniam traiectus. *B. G.*, iv, 21, § 3.

[2] See pp. 552–95, *infra*.

Army', offered an excellent site for the encampment of the force that was destined to protect the communications; and perhaps a detachment may have been posted on the opposite bank of the river.[1] If the distance in a straight line to Britain was a little longer than from the creeks of Wissant, the passage, owing to the set of the tidal streams and the prevalence of south-westerly winds, was more convenient. Caesar therefore gave orders that vessels should be collected from the adjacent coasts, and assemble, along with the galleys which had been docked after the war with the Veneti, in the Portus Itius. 55 B.C.

He attempts to obtain information about Britain from Gallic traders.

The summer was now far advanced; and Caesar saw that his first expedition must be a mere reconnaissance: but, as he tells us, 'he thought that it would be well worth his while merely to visit the island, see what the people were like, and make himself acquainted with the features of the country, the harbours, and the landing-places.'[2] Though on a clear day he could see beyond the straits those 'astonishing masses of cliff' which haunted the imagination of Cicero,[3] he was about to venture into an unknown land. The Italians of that time knew hardly anything of the island which they vaguely regarded as the end of the inhabited world, except that it produced tin, some of which found its way to the markets of the Mediterranean.[4] Perhaps Cicero and other cultivated men had read extracts from the journal of Pytheas: but Pytheas was a discredited writer; and, after all, his description of the Britons who lived in the time of Alexander the Great would have been little more useful to Caesar than Bernier's account of the empire of Aurangzeb would be to a traveller who intended to spend a winter in India. Caesar sent for traders from all parts of North-Eastern Gaul, and questioned them about the island:—How large was it? What tribes inhabited it? What were their methods of fighting, their manners and customs?

[1] See A. E. E. Desjardins, *Géogr. de la Gaule rom.*, i, 1876, pl. xv (p. 352), pl. xvii (p. 384), and cf. *Boulogne-sur-mer et la région boulonnaise*, i, 1899, p. 30.

[2] tamen magno sibi usui fore arbitrabatur si modo insulam adisset, genus hominum perspexisset, loca, portus, aditus cognovisset. *B. G.*, iv, 20, § 2.

[3] See p. 329, *infra*.

[4] Dion Cassius, xxxix, 50, §§ 3–4. See p. 509, *infra*.

55 B.C. What ports were capable of accommodating a large fleet? He failed to obtain the information which he required. Many commentators have insisted that the traders could have told him all that he wanted to know; and certainly it seems difficult to understand how they could have professed ignorance of the harbours without manifest contumacy: but at least as regards the other questions, the reason which Caesar assigns for their silence is sufficient:—'even they know nothing of Britain except the coast and the parts opposite the various regions of Gaul.'[1] Moreover, it must be remembered that Caesar asked them what harbours could shelter a large fleet; and as they were only acquainted with the harbours of Kent, none of which would fulfil this requirement, it is quite intelligible that even on this point they should have been unable to enlighten him. Still, they could have given valuable information about the Kentish coast; and the passage in which Strabo accounts for the hostility of the Veneti suggests that they kept silence from interested motives.[2] They could not foresee that Caesar's expeditions would powerfully stimulate British trade.

Gaius Volusenus sent to reconnoitre the opposite coast.

Thrown back upon his own resources, Caesar sent a military tribune, named Gaius Volusenus, in a galley to reconnoitre the opposite coast. Volusenus had distinguished himself in a campaign, conducted by one of Caesar's generals, against the mountaineers of the upper Rhône: he possessed, as his later history proved, not merely a keen eye for the features of a country, but daring of that kind which characterized the sons of Zeruiah; and how highly Caesar thought of him is evident from the fact that he was the only military tribune whose name is mentioned with honour in the *Commentaries*.[3]

Envoys from British tribes sent to

All this time trade was going on as usual between Gaul and Britain; and Gallic merchants had informed their clients in Kent that the long-expected invasion was about

[1] neque his ipsis quicquam praeter oram maritimam atque eas regiones quae sunt contra Gallias notum est. *B. G.*, iv, 20, § 3.

[2] See *Journ. Roy. Soc. Ant. Ireland*, 5th ser., v, 1895, p. 26.

[3] C. Volusenus, tribunus militum, vir et consilii magni et virtutis. *B. G.*, iii, 5, § 2.

to take place. While Volusenus was cruising in the Straits of Dover a ship with envoys from various British tribes on board sailed into the Liane. Presenting themselves in Caesar's camp, they announced that their principals were prepared to submit to the Roman People and to give hostages. Caesar received them courteously, exhorted them to adhere to their resolve, and dismissed them. But they were not to return alone. Two years before, during the campaign against the Belgae, Caesar had gained over Commius, whose connexion with Britain [1] he had perhaps already ascertained, and, in accordance with the policy which he often followed, had established him as king over the Atrebates. He had doubtless learned much from him about British politics, and had concluded that, just as in Gaul he had taken advantage of tribal disputes and had found it politic to support the Aedui and the Remi against their rivals, so in Britain his best course would be to side with the Trinovantes against the aggressive Catuvellauni. He had formed a high opinion of the energy and judgement of Commius, and believed him to be thoroughly loyal. Accordingly he charged him to approach all the British chieftains with whom he had any influence, engage them on the side of Rome, and give them notice that he himself would shortly visit the island. Commius took with him a troop of cavalry, composed of thirty of his retainers.

55 B.C.

Caesar to promise submission.

He commissions Commius to return with them and gain over tribes.

Meanwhile Volusenus had been carrying out Caesar's instructions. His galley, manned by trained oarsmen, not only made him comparatively independent of wind and tide, but, owing to her superior speed, would enable him to keep clear of any ships which Gauls or Britons might send against him. We do not know what part of the coast he reconnoitred first: but it is probable that his coasting voyage did not extend beyond Lympne, or, at the furthest, Rye on one side and the North Foreland on the other; for within those limits the port and the alternative landing-place of which he was in search were to be found. The port was indeed too small for such a vast armada as would be required to transport the grand army with which Caesar purposed eventually

Volusenus's voyage of reconnaissance.

[1] See p. 300, *supra*.

55 B.C. to invade Britain, but not for the comparatively small fleet that had been collected for the preliminary expedition: if Volusenus had sailed westward in quest of the great harbour which he could not have found until he had reached the coast of Sussex,[1] he would have turned back when he saw the inhospitable forest of the Weald, or the Fairlight Down; and, moreover, he knew that Caesar intended to cross the Channel in its narrowest part. While he was still some miles from the British coast he could see the low but precipitous chalk cliffs, backed by a commanding range of heights, that hem in the rock-strewn shore of East Wear Bay: the inlet of Folkestone was plainly too small to accommodate the Roman fleet; and the first sight of the hills that guarded the coast from Folkestone to Hythe and of the wooded uplands that overlooked the tide-washed flat which is now Romney Marsh,[2] must have warned him not to advise the great captain whom he served to land beneath them. It was a maxim of ancient warfare, never disregarded without urgent necessity, to avoid engaging an enemy who had the advantage of higher ground; and there was not a foot of land in the whole extent of coast between Shakespeare's Cliff and Lympne which a Roman soldier would not have described as a most unfavourable position. The hills behind Hythe were, indeed, pierced by three valleys: but it was evident that they ascended to high, broken, and wooded ground, where cavalry would be useless, and an invading army would be encompassed by manifold perils;[3] and for such disadvantages the narrow pool harbour which extended opposite Hythe, between the hills and the long bank of shingle, through a gap in which it might be entered at high tide,[4] promised no compensation. Eastward of Shakespeare's Cliff Volusenus saw that he must look for the place of disembarkation. There, sheltered in the valley between the cliffs, was old Dover harbour, in which we may suppose that Gallic merchants used to discharge their freight.[5] But even this haven would be useless if the landing

[1] See *Journ. Brit. Archaeol. Association*, xxxvii, 1881, p. 272.
[2] See pp. 532–52, *infra*.
[3] See pp. 629, 635–6, *infra*.
[4] See pp. 547–8, 632–3, *infra*.
[5] See pp. 530–1, *infra*.

were to be opposed; and it was necessary to look for some broad expanse of open beach which would give easy access to the interior. None such was yet visible. The galley ran on under the Castle Cliff, round the Foreland and past the coomb within which lies St. Margaret's Bay, past the cliffs, still precipitous but diminishing in height, which end at Kingsdown. About a hundred yards further on the ground was seen rising again; and the tribune observed a low rampart of cliff extending and gradually sinking towards the north till it finally terminated just south of the spot where Walmer Castle rises amid embowering trees. Stretching northward for several miles from this spot he saw the open beach for which he had been looking. Not a sign of high ground was visible. Once the legions had succeeded in forcing their way on to dry land, they would find no difficulty in following up their advantage; and the cavalry would be able to ride down the beaten enemy. The slope upon which Walmer Church now stands would afford a suitable site for the camp. But it was of course impossible to see far inland; and, as Volusenus could not venture to disembark and run the risk of falling into the hands of the natives, he was unable to find out all that he wished to know. The nature of the inner country, the comparative density of the population, the water-supply,—of all these things he remained ignorant. But Caesar had chosen him because he was the fittest man that he could find; and we may assume that he did not neglect precautions which any competent officer would have taken, and that he did not overlook what no observant man could have failed to perceive. He spent three entire days in British waters; and his time must have been fully occupied. We may be sure that he bore in mind that the beach was of shingle; that he took soundings all along the coast between Walmer and Deal as close inshore as he could venture to go, and tested the character of the anchorage; and that he noted the phenomena which twice daily obtruded themselves upon his attention,—the rise and fall of the tide, and the movement up and down the Channel of the tidal stream. Perhaps indeed he went as far north as Sandwich, and concluded that a landing might still more

55 B.C.

55 B.C. advantageously be effected between that point and Sandown, where, even in those days, the beach must have shelved more gently than at Walmer or Deal.[1] One other feature, if it then existed, cannot have escaped his scrutiny,—the Goodwin Sands, perhaps only half-formed, or the long low bank of London Clay, which, as some geologists believe, may then have occupied their place.[2] On the fourth day following that of his departure he returned to the Portus Itius, and presented his report to Caesar.

Kentishmen prepare for resistance.

The Kentishmen, on their part, knew what they had to expect. The Roman galley had of course been watched; and though Caesar was coming professedly to receive them under the protection of Rome, his visit would portend the loss of their independence. If they chose to resist, they would not be embarrassed by having a long line of coast to defend. The movements of the galley indicated where the fleet of which she was the forerunner would probably arrive; and, moreover, those who lived by the sea were aware that the invaders could not attempt to land except at a few points within a strictly limited range. War-chariots would be helpful in checking them when they attempted to advance through the surf: accordingly the horses were exercised on the beach until they became accustomed to enter the waves.

Certain clans of the Morini spontaneously promise to submit.

The Portus Itius was thronged with shipping, and the preparations for the expedition were nearly complete; but the base of operations was still insecure. The Morini had hardly felt the weight of Caesar's hand, and might give trouble to the garrison which he intended to leave for the protection of his communications: but the end of August was approaching; he was anxious to set sail; and he had no time to reduce the tribe to submission. Fortune, however, as usual, befriended him. The various communities of the Morini were accustomed to act independently. Envoys from some of them appeared in Caesar's camp, and excused themselves for having resisted the Romans in the two previous years. He of course accepted their excuses, and ordered them to give him a large number of hostages, who were promptly brought to the camp.

[1] See pp. 595–6, 651, 664–5, *infra*.

[2] See pp. 525–8, 657–9, *infra*.

THE SMALL DOWNS
THE DOWNS
Great Stour
Little Stour
Portus Dubris
EAST KENT
Scale- Two Miles to One Inch

55 B.C. Caesar's expeditionary force.

And now all was ready. The expeditionary force consisted of two legions—the 10th, which had gained renown on many fields and was regarded by Caesar with special favour, and the 7th, which had played a conspicuous part in the famous battle with the Nervii—besides about five hundred cavalry, raised from various tribes of Gaul, slingers from the Balearic Isles, and Numidian and Cretan archers. The entire army numbered about ten thousand men. A small squadron of galleys and about eighty transports were assembled in the harbour; and on the 25th of August[1] the legionaries embarked on the transports, while the galleys were assigned to the archers, slingers, and artillerymen. The catapults which they carried would be worked, in case they were required, under the protection of movable turrets, which could be erected, at short notice, on their decks.[2] Caesar omitted to mention the class of 'long ships' to which they belonged: but his narrative shows that they were shallow; and it may be doubted whether any of them had more than one bank of oars.[3] The transports had of course been carefully selected, and were all excellent sea-boats: but they had not been designed for disembarking troops on an enemy's coast; and in case it should prove necessary to land on an open beach, the troops whom they carried would find themselves, on entering the water, almost out of their depth. They were probably sailed by their native crews; and the galleys, which were severally placed under the command of the quaestor, the two generals who commanded the legions, and the auxiliary officers, were doubtless handled by the seamen and Provincial oarsmen who had manned them in the preceding year. The fleet included some small fast-sailing vessels of light draught, which were commonly used for reconnoitring, and would now be called scouts. Eighteen other transports were lying in the little harbour of Ambleteuse, between five and six miles to the north,[4] having been

[1] See pp. 600–3, *infra*.

[2] *B. G.*, iii, 14, § 4. Cf. C. Torr, *Ancient Ships*, 1894, p. 59.

[3] M. le Contre-Amiral Serre, *Les marines de guerre de l'ant.*, 1885, p. 36. *Naves longae* were not necessarily even decked (*B. C.*, i, 56, § 1; iii, 7, § 2).

[4] See pp. 587–8, *infra*.

55 B.C. prevented by contrary winds from reaching the Liane; and, as the wind was now favourable for the voyage to Britain, and Caesar could not afford to wait, he sent his cavalry by road with orders to embark on these vessels and follow him. As the expedition was to be of such short duration, no heavy baggage was taken, and only sufficient supplies to last for a few days. A general named Sulpicius Rufus remained with an adequate force to guard the camp and the harbour; while Titurius Sabinus, who had commanded a division in the war of the previous year, and Aurunculeius Cotta, who had served with distinction in the campaign against the Belgae, were directed to march with the remaining legions against those clans of the Morini which had not submitted, and their neighbours, the Menapii.

Sabinus and Cotta sent to punish the recalcitrant Morini and the Menapii.

Caesar's voyage.

It was just five days before the full moon;[1] and high tide that evening was about six o'clock. About midnight the moon set, and we may suppose that, like the ships of William when he sailed to encounter Harold, each vessel carried a lantern.[2] Soon afterwards the signal was given to weigh anchor,[3] and the ships stood out to sea and steered against the ebb tide, which, however, was moving at less than one knot an hour,[4] for Dover harbour.[5] As they passed Ambleteuse, there was no sign that the cavalry transports had yet got under way. About half an hour before sunrise the stream turned eastward; and by that time Cape Grisnez had been left behind. But at some period of the voyage the wind must have shifted to an unfavourable quarter,[6] for it was not until the fourth hour of the day, or about nine

His cavalry transports fail to put to sea in time.

Aug. 26.

[1] See pp. 600–3, *infra*.

[2] E. A. Freeman, *Norman Conquest*, iii, 1875, p. 399. The minute details which Mr. F. H. Appach (*C. J. Caesar's Brit. Expeditions*, 1868, pp. 57–8, 99, 107–8) gives as to Caesar's order of sailing both in 55 and 54 B.C. are imaginary: in saying this I have the support of Capt. Iron, the harbour-master of Dover. Moreover, if, as Mr. Appach conjectures, the transports had been drawn up for the disembarkation in 55 B.C. in two lines, one behind the other, the men, in attempting to disembark from the rear line, would have been drowned. See *B. G.*, iv, 24, § 2, 25, § 3, and p. 673, *infra*.

[3] In regard to Caesar's expression—(III. fere vigilia) *solvit* (*B. G.*, iv, 23, § 1), see Prof. R. Y. Tyrrell's *Correspondence of Cicero*, i, 1885, p. 193, with which cf. *B. C.*, iii, 102, § 7.

[4] See *Tidal Streams,—English and Irish Channels.*

[5] See pp. 634–5, 644–6, *infra*.

[6] See pp. 615–6, *infra*.

o'clock in the morning,[1] that the galleys approached the Dover cliffs; and at that time the transports, which were slower sailers and had no oars, were far behind. Above the white precipices, ranged on the undulating downs behind, Caesar descried an armed host of the enemy. 'The formation of the ground,' he observed, 'was peculiar, the sea being so closely walled in by abrupt heights that it was possible to throw a missile from the ground above on to the shore.'[2] To attempt a landing in the harbour or below the cliffs on either side of it was of course out of the question; and Caesar determined to remain at anchor until the rest of the fleet should arrive. The reader who is familiar with the *Commentaries*, and can comprehend their implied meaning, will perceive that the vessels must have been grouped in the bay somewhere between the Castle Cliff and the South Foreland, the one on the extreme right being about a mile westward of the latter.[3] Caesar summoned his generals and tribunes to come on board, communicated to them the substance of the report which he had received from Volusenus, and instructed them how to handle their ships and troops when the landing-place should be reached, warning them above all to bear in mind that rapid and irregular movements were of the essence of seamanship, and to be prepared to obey orders on the instant. When he was satisfied that all understood what was required of them, he sent them back to their ships. Between three and four in the afternoon the infantry transports arrived; and although Caesar does not expressly say so, it seems reasonable to assume that he communicated with their officers as well.[4] Between four and five the stream, which, for about six hours, had been running down the Channel, turned towards the east, and, as the wind was now blowing from a favourable quarter, Caesar gave the signal to weigh anchor.[5] A few minutes

55 B.C.

He anchors off the Dover cliffs.

[1] See p. 615, *infra*.

[2] Cuius loci haec erat natura atque ita montibus angustis mare continebatur, uti ex locis superioribus in litus telum adigi posset. *B. G.*, iv, 23, § 3.

[3] See pp. 652–3, *infra*.

[4] See pp. 648–9, *infra*.

[5] See pp. 610–1, 647–9, *infra*. Strictly speaking, the true (not magnetic) direction of the stream, west of the South Foreland, would have lain between about ENE. and NE. by E. See *Archaeologia*, xxxix, 1863, pp. 291–3.

55 B.C. later galleys, transports, and smaller craft, with all sail set, were running in an extended line past the Foreland, while the British chariots and cavalry, followed by their infantry, were hurrying across country to intercept them. In about an hour the armada was off the coast between Walmer and Deal, heading straight for the shore; and, while the galleys were held ready for emergencies, the transports were run aground.

Late in the afternoon he sails on to Walmer—Deal.

The landing vigorously resisted.

Caesar now saw crowding upon him the troubles that were due to his lack of preparation. All along the beach a multitude of painted warriors,[1] with long moustaches and hair streaming over their shoulders, were drawn up ready for action. The transports were immovable in water so deep that the men, crowding in the bows, shrank from plunging in; and when some of them overcame their hesitation, they found themselves staggering and slipping, over-weighted by their armour and encumbered by the shields on their left arms and the javelins which they grasped in their right hands; while the Britons, standing securely on the beach, and the charioteers, driving their trained horses into the sea, harassed them with missiles to which they could not reply. Old soldiers as they were, they felt unnerved by difficulties which they had never encountered before. Caesar promptly sent the galleys to the rescue. Driven through the water at their utmost speed, they were ranged on the right flank of the enemy, who, alarmed by the long low rakish hulls, the like of which they had never seen, and distracted by the measured stroke of the oars, suddenly found themselves assailed by slingers and archers, and enfiladed by strange artillery. Unable to use their shields unless they changed front, they ceased to press their attack, stood still, and presently began to give ground. But few of the legionaries had yet ventured to enter the water; and the rest still hesitated to take advantage of the respite. Then the standard-bearer of the 10th legion, calling upon the gods for aid, turned to his comrades, and cried, 'Leap down, men, unless you wish to abandon the eagle to the

[1] Cf. *B. G.*, v, 14, § 2, with *Journ. Brit. Archaeol. Association*, N. S., ix, 1903, pp. 95–6.

enemy. I, at all events, shall have done my duty to my country and my general.' Springing overboard, he advanced alone, holding the eagle above his head. The men plucked up courage, and, calling upon one another not to bring the legion to shame, leaped all together from the bows. Encouraged by their example, the men in the nearest vessels followed, and the fight became general. 55 B.C.

But the advantage was still with the defenders. The galleys could not be everywhere at once. The Romans, though they could not get firm foothold, tried hard to keep their ranks and follow their respective standard-bearers; but they soon lost all formation. As men entered the sea from one ship or another, they attached themselves in bewilderment to any standard they came across; and the enemy on the shore, whenever they saw a few legionaries dropping one by one into the water, drove their horses in, and surrounded and attacked them before they could join their comrades; while others planted themselves on the exposed flank of a disordered unsupported group,[1] and showered missiles into their midst. Jarring with the shouts of the disciplined soldiers, resounded the harsh Celtic yell,[2] the clangour of the Celtic trumpet,[3] and invocations uttered in strange language to strange gods.[4] Caesar now manned his scouts and the boats belonging to the galleys, and sent them in different directions to assist all who were overmatched. Gradually the foremost bodies of legionaries fought their way on to the beach: the rest followed quickly in support; and now, closing their ranks and drawing their swords, they charged the enemy with exultant cries, and put them to flight. Want of cavalry, however, made it impossible to complete the victory.

Caesar's victory indecisive owing to want of cavalry.

It was now near sunset. The site which Volusenus had noted for the camp was close to the sea; and while fatigue-parties were sent out to cut wood and the outposts took up

The Romans encamp.

[1] *B. G.*, iv, 25, § 5. See the notes in Kraner—Dittenberger's edition and in that of C. E. C. Schneider.

[2] *B. G.*, v, 37, § 3.

[3] Diodorus Siculus, v, 30, § 3; J. Evans, *Coins of the Anc. Britons*, pp. 192, 232, *Anc. Bronze Implements*, p. 363.

[4] *Rev. des études anc.*, vi, 1904, pp. 53, n. 6, 54.

55 B.C. their appointed places, the rest of the troops fell to work with pick and shovel along the lines which had been marked out for them. The galleys were hauled up on the beach; but the transports were necessarily left at anchor. Until the cavalry should arrive it would not be prudent to venture into the interior; and we may suppose that a galley was sent back to the port of Ambleteuse, to inform their captains about the landing-place for which they were to steer.

British chiefs sue for peace.

It would seem that the resistance which the Britons had opposed to the disembarkation was purely local, and that no defensive league had been organized. The men of East Kent were disheartened by failure, and on the next day sent envoys to sue for peace. Some days before, when Commius had just landed and was formally communicating Caesar's mandate to the chiefs, he had been arrested and imprisoned. The envoys, who brought him with them, begged Caesar to pardon this outrage, for which, they said, the ignorant rabble were responsible. He replied that their countrymen had made an unprovoked attack upon his army although they had spontaneously sent an embassy to Gaul to proffer submission; but he promised to accept their excuses on condition of their giving hostages. Part of the required number were handed over there and then, the envoys promising that the rest, who would have to be fetched from considerable distances, should be brought within a few days. The Britons who had fought at Walmer were ordered by their leaders to return home; and within the next few days tribal chiefs arrived from various districts, and formally surrendered.

The cavalry transports dispersed by a gale.

On the morning of the 30th of August the long-looked-for cavalry transports were descried in the offing. They had sailed from Ambleteuse with a light breeze; but as they were approaching the British coast a sudden gale prevented them from keeping on their course. 'Some,' wrote Caesar, 'were carried back to the point from which they had started, while the others were swept down in great peril to the lower and more westerly part of the island. They anchored notwithstanding; but, as they were becoming waterlogged, they were forced to stand out to sea in the face of night, and

make for the Continent.'[1] The brief sentences tell a tale which cannot be mistaken. The ships which were swept down past the Foreland and the Dover cliffs scudded before the north-easterly gale ;[2] and, although they were evidently in no danger of being driven ashore, they were in great peril because only the most watchful steering could prevent them from broaching to : if a heavy sea struck the stern, it might swing the vessel round, and in a moment she would be overset and founder. The ships which were carried back to the point from which they had started were of course handled differently. A sailing-vessel, caught by a gale, must either run before the wind or lie to. With these vessels the latter course was adopted. Carrying only just enough sail to keep them steady, they were laid to on the port tack ; and once they had drifted past Cape Grisnez into comparatively sheltered water, they were able to stand in for the shore and make the port of Ambleteuse.[3] Not one of the eighteen vessels, not a single man among their crews, was lost ; and this fact, which Caesar was careful to record, bears witness to the skilful seamanship of the Gauls. 55 B.C.

Caesar's fleet partially wrecked.

But on the shores of East Kent the gale was still raging ; and the moon that shone out that night through the fleeting clouds was at the full. Caesar's officers and, it would seem, Caesar himself were ignorant of the connexion between tide and moon ; but if he had ever had leisure to study the writings of Pytheas or of Posidonius,[4] he would have known what he might expect. His Gallic pilots indeed could certainly have enlightened him ; and there will always remain a doubt whether he did not know more than he chose to admit. It was high water about an hour before midnight ;

[1] aliae eodem unde erant profectae referrentur, aliae ad inferiorem partem insulae, quae est propius solis occasum, magno suo cum periculo deicerentur ; quae tamen ancoris iactis cum fluctibus complerentur, necessario adversa nocte in altum provectae continentem petierunt. *B. G.*, iv, 28, §§ 2–3. See p. 598, n. 2, *infra*.

[2] A gale blowing from the north-east on the eastern coast of Kent would be diverted on the south coast to ENE. This, or possibly NE. by E., may be assumed to have been the direction of the wind when the transports were scudding before it. If it had blown from a point nearer north they would have found shelter under the lee of the southern cliffs. See p. 582, *infra*.

[3] See pp. 582, 651, *infra*.

[4] See p. 219, n. 4, *supra*.

55 B.C. and the seas that came rushing over the shingle before the north-east wind rose as high as a spring tide. The galleys which had been hauled up, as Caesar supposed, above high-water mark, were swept by the waves; the transports were driven ashore. Soldiers and crews could only look helplessly on. Several vessels were totally wrecked; and the rest lost their anchors, cables, and other tackle. No provision had been made against the chance of such a disaster; and the tools and materials that were needed for repairs were on the other side of the Channel. The whole army was seized with panic. Men asked one another how they were to subsist when they had no grain, and how they were to get back to Gaul when there were no ships to carry them.

The British chiefs prepare to renew hostilities.

The British chiefs who were still in the camp saw their opportunity. The coincidence of the shipwreck with the full moon was a good omen.[1] They knew that Caesar had no supplies; and although they did not know exactly the strength of his force, they saw that his camp was very small, and concluded that his troops were correspondingly few. Besides, his want of cavalry would place him at a disadvantage. Accordingly, they determined to recall their tribesmen, to prevent the Romans from getting supplies, and to harass them by an irregular warfare, in the hope that they would be able to starve them out, or at any rate prevent them from re-embarking until wintry weather should have set in. One by one they moved away from the camp without attracting observation.

Caesar labours to retrieve the disaster.

Meanwhile Caesar was doing his best to retrieve the disaster; and, although the chiefs managed to keep their plans secret, he suspected that they meant mischief. Moreover, the hostages who were still due did not arrive. The crops were ripe; and troops were detailed every day to get corn. A galley was sent back to Gaul to fetch everything that was required for repairing the ships. Twelve of them were so badly damaged that it was impossible to patch them up even for one voyage; but their timbers and bronze were utilized for the repair of the rest. All the legionaries who had any knowledge of carpentry or metal-working were employed

[1] *B. G.*, i, 50, §§ 4–5.

as shipwrights, and worked with such good will that within a few days the fleet had been made tolerably seaworthy. 55 B.C.

All this time natives were daily passing in and out of the camp; and no one in the Roman army suspected that trouble was brewing. At a considerable distance from Walmer there was a wood, close to which was a field of standing corn. Everywhere else the crops had been already cut; and to this spot the 7th legion was dispatched. The officer who commanded it neglected to send out scouts; and the troops laid aside their arms, and went to work securely with their reaping hooks. It is true that the only cavalry were Commius's thirty retainers; but they might have done good service. It would seem that even the ordinary precaution of keeping some of the cohorts under arms was neglected.[1] Suddenly the enemy's chariots and cavalry emerged from the wood, and swept down upon the unarmed and scattered reapers. The chariots careered at full gallop all over the field, the warriors who stood beside the drivers hurling javelins[2] or slinging stones at the legionaries as they were running to seize their arms, and intimidating them, as Caesar said, 'by the mere terror inspired by their horses and the clatter of the wheels:' presently the drivers passed into the intervals between the troops of their supporting cavalry; horsemen and charioteers charged together;[3] and while the warriors leaped from their chariots and fought as infantry, the drivers moved off to a safe distance, ready to receive them in case they were hard pressed. Meanwhile two cohorts were on guard as usual outside the gates of the camp;[4] and some of their number reported to Caesar that an

The 7th legion surprised and attacked while cutting corn.

[1] My view, which is based upon *B. G.*, iv, 32—not § 5 only—is supported by Turpin de Crissé (*Comm. de César*, i, 1785, p. 294), but differs from that of von Göler (*Gall. Krieg*, 1880, p. 136, n. 3).

[2] See *Rev. celt.*, xxv, 1904, pp. 229–31.

[3] See pp. 676–7, *infra*.

[4] It is impossible to decide whether the cohort or half-cohort which reported to Caesar (*B. G.*, iv, 32, § 1) was an outlying piquet, as von Göler thinks (*Gall. Krieg*, 1880, p. 136, n. 2), or a guard stationed just outside one of the gates. The latter appears to have been the usual position. (*B. G.*, vi, 37, §§ 3–4; *B. C.*, i, 75, § 3; iii, 94, § 6). Von Göler's opinion is based upon a mistranslation of the word *longius* (*B. G.*, iv, 32, § 3).

55 B.C. unusual amount of dust was rising in the direction in which the 7th had gone. His suspicions were aroused; and, ordering the two cohorts[1] to accompany him, two others to take their places, and the remaining cohorts of the 10th legion to leave their work, arm, and follow him immediately, he marched towards the corn-field. He had advanced some little distance before he came in sight of the legionaries, who were evidently unable to hold their own. Huddled together in a small space, with ranks disordered, they were surrounded by cavalry and charioteers, missiles flying into them from every side. Caesar was just in time. When the enemy saw reinforcements approaching they suspended their attack, and the 7th recovered from their panic. But if the enemy had no mind to renew the combat, Caesar did not feel able, without cavalry and with only two legions, one of which had just been so roughly handled, to strike an effective blow. 'The moment,' he afterwards explained, 'was not favourable for challenging the enemy and forcing on a battle.'[2] Accordingly he contented himself with maintaining his ground, and, after a short interval, withdrew both legions into camp.

Military operations suspended owing to bad weather.

The tribesmen who had not yet rejoined their chiefs were on the way: but during the next few days stormy weather prevented the Romans from going out of camp and the enemy from attacking them. Such was Caesar's statement; and it is not difficult to fathom his meaning. He would not attack a mobile enemy whom it was difficult to bring to action, but preferred to wait until they should attack him on his own ground, before his impregnable camp: on the other hand, the ground was so miry that for the time their chariots could not act. The Kentish chiefs, however, were

[1] C. Schneider (*Comm. de bellis C. I. Caesaris*, i, 407) in a note on *B. G.*, iv, 32, § 1, infers from vi, 37, §§ 3–4, that one cohort was on guard in front of each of the four gates of the camp. But there is no proof that in *B. G.*, vi, 37, § 3, the word *cohors* means an entire cohort, and not details thereof, or even if it does, that any hard-and-fast rule prescribed that, without regard to circumstances, one entire cohort, no more and no less, should invariably guard each of the four gates.

[2] . . . nostri se ex timore receperunt. Quo facto ad lacessendum hostem et committendum proelium alienum esse tempus arbitratus, suo se loco continuit, &c. *B. G.*, iv, 34, §§ 1–2.

not idle. Messengers scoured the country, assured all who still remained passive that the Roman army was contemptible, and urged them to seize the opportunity of plundering their camp and securing their own independence for ever. A large body of horse and foot speedily assembled, and advanced towards the coast. If they had been commanded by one skilful leader, and had adhered to the simple plan of harassing the Romans when they were endeavouring to embark, they might have achieved something. But they were a mere aggregate of tribal levies under tribal chiefs; and greed and impatience worked their ruin. The one thought that troubled Caesar was that their speed would enable them to escape the consequences of defeat. They made a wild attack upon the camp, and the legions, which were drawn up outside, of course scattered them. Commius's horse were of some slight service in the pursuit; and the legionaries, who exerted themselves to the utmost, killed many of the fugitives, and burned all the buildings which they had time to reach.

55 B.C.

The Britons, attempting to rush Caesar's camp, are defeated with heavy loss.

This success came just in time to enable Caesar to leave Britain with some show of credit. His departure could not be postponed. It was about the middle of September: the dreaded equinox was near; and, with his unsound ships, he would need a fine night for the voyage. He must therefore have been relieved when, on the very day of their defeat, the chiefs sent envoys to sue for peace. He ordered them to find twice as many hostages as he had demanded before; and, as he could not wait for them, the chiefs were to send them in their own or the merchants' vessels to Gaul. Before he embarked he may have personally reconnoitred the coast north of Walmer: anyhow he decided that, when he returned in the following year, his best landing-place would be the sandy flats between Sandown and Sandwich, where, as we have seen, the seaward slope was gentler than that of the Walmer shingle.[1] But otherwise the objects for which he had undertaken the expedition had not been attained. The time for preparation had been too short.

Caesar compelled by the approach of the equinox to return to Gaul.

Causes of his partial failure.

[1] See pp. 311-2, *supra*.

55 B.C. Owing to the excessive draught of the transports, the disembarkation had entailed unnecessary loss: by neglecting to bring over supplies Caesar had exposed the 7th legion to the risk of a defeat which would have been calamitous; while the unfortunate absence of the cavalry had made it impossible to obtain any information about the nature of the country, and had weakened the effect of the final victory. The troops were embarked without opposition, and, taking advantage of a fair breeze, Caesar set sail just after midnight. The fleet reached the opposite coast safely; but two of the transports, which perhaps were in worse condition than the rest, kept a little too far out to sea, and, failing to make the mouth of the Liane, drifted a few miles further down the coast and reached land somewhere north of the mouth of the Canche. The soldiers who had disembarked from them, numbering about three hundred, were marching northward to join their comrades when they were intercepted and attacked by a band of the Morini, who belonged to one of the clans which had submitted a few weeks before. As the Romans were considerably outnumbered, they were obliged to form in a square; and, hearing the shouts of the combatants, large numbers flocked to join in the attack. The three hundred defended themselves with vigour; and four hours later, when Caesar's cavalry came to the rescue, they were still unbeaten. The assailants speedily dispersed; but next day Labienus marched against them with the two legions which had just returned from Britain, and almost all were taken prisoners. Titurius and Cotta, with the other legions, had been punishing the Menapii. Finding that they had taken refuge in their forests, they mercilessly ravaged the open country, cutting the corn and burning the hamlets. Thus, when the legions went into winter-quarters in the country of the Belgae, Caesar might feel that in the ensuing summer his base of operations would be secure. 'Thither,' he wrote dryly, 'two British tribes and no more sent hostages: the rest neglected to do so.'[1]

Two transports fail to make the Portus Itius: the troops whom they carried attacked by the Morini.

Punishment of the Morini and Menapii.

[1] Eo duae omnino civitates ex Britannia obsides miserunt, reliquae neglexerunt. *B. G.*, iv, 38, § 4.

When Caesar's dispatches reached the Senate, they ordered a thanksgiving service of twenty days to be held in honour of his exploits. No one who is versed in Roman literature and gifted with historical imagination will regard the decree as ironical. For Caesar's countrymen may well have felt that he had opened the way for the conquest of a new world.

55 B.C. Thanksgiving service at Rome for Caesar's success.

CHAPTER VII

CAESAR'S SECOND INVASION OF BRITAIN

55 B.C.

Caesar builds a fleet for a second expedition.

CAESAR had learned the lessons which failure had taught him. In the winter he was obliged, as usual, to go to Cisalpine Gaul, partly in order to discharge judicial and administrative business, partly to safeguard his own political interests in Italy. Before he left Belgium he ordered his generals to employ the legions in repairing the old ships and building a new fleet for the second expedition. He drew up minute instructions for their guidance. Two thousand cavalry horses, besides transport cattle, were to be conveyed across the Channel; and, as the campaign would probably be protracted, it would be impossible to leave all the heavy baggage behind, and imprudent to trust again for supplies to the resources of the country.[1] The ships were to be somewhat shallower than those which were commonly used in the Mediterranean, in order to facilitate the work of loading and to enable them to be hauled up on the shore: on the other hand, to make room for troops and freight, they were to be rather broader in the beam. Their low freeboard would admit of their being constructed for rowing as well as sailing;[2] and Caesar, who had noticed that the waves in the Channel were comparatively small, thought that it would involve no danger. But this shallowness, combined with unusual breadth, entailed a disadvantage which he had perhaps not foreseen: it would cause the vessels, unless the wind were right aft or on the quarter, to make a great deal of leeway.[3] It was of course impossible to build such a large

[1] Cf. *B. G.*, v, 8, § 4.

[2] Cf. Daremberg and Saglio, *Dict. des ant. grecques et rom.*, i, 59–60 (ACTUARIAE NAVES).

[3] I infer from Caesar's narrative (see p. 334, *infra*) that his vessels were not provided with lee-boards, in regard to which see E. F. Knight, *Sailing*, 1900, pp. 16, 25

flotilla in one port. Some of the ships were to be constructed in the mouth of the Seine: others doubtless in the Portus Itius itself; others probably in the Canche, the Authie, and the Somme, possibly even on the Marne, far from the sea-coast.[1] The legionaries were ill provided with appliances for ship-building: but they might be trusted to do their best; and the tackle necessary for rigging and equipping the fleet was to be imported from Spain. The cost of the expedition would be very heavy: but Caesar was amassing wealth for himself and his lieutenants by plundering Gaul; and he certainly hoped to do more in Britain than recover his expenses.[2] 55-54 B.C.

News of these preparations must of course have flown swiftly across the Channel; but it is hardly surprising that the British chieftains did not take advantage of the time that was given them to mature a scheme of defence. Cassivellaunus was still intent on self-aggrandisement; and in the struggle with his neighbours, the Trinovantes, he slew their king, whose son, Mandubracius, contrived to escape, took ship for Gaul, and presented himself—the first of a series of British exiles who invited Roman interference—in Caesar's camp. The exact date of his flight cannot be given: it is sufficient to know that he was with Caesar when the time arrived for the Roman army to embark.

Mandubracius flees from Britain and takes refuge with Caesar.

Caesar did not start for Italy until the middle of November,[3] and after he had fulfilled the civil duties which awaited him in Cisalpine Gaul he was obliged to travel to the further shore of the Adriatic in order to punish a tribe which had been making devastating raids upon Illyricum. In the early spring he was again in Cisalpine Gaul, clearing off arrears of work, and preparing to recross the Alps. Cicero, for whom he had an unfeigned admiration, and whom he was always endeavouring to conciliate, was now upon the best of terms with him; and his correspondence throws a ray of light upon the hopes which had been awakened in Italy by the preparations for a fresh expedition to Britain. Caesar was of

Caesar winters in Cisalpine Gaul and Illyricum.

His correspondence with Cicero

[1] See p. 331, n. 2, *infra*.

[2] See Suetonius, *Divus Iulius*, 47, and also p. 350, *infra*.

[3] Cf. *B. G.*, v, 1, § 1 with p. 726, *infra*.

54 B.C course beset with letters of recommendation written by public men on behalf of friends who hoped to acquire riches in Gaul or Britain; and Cicero wrote one, as he alone knew how to write, begging him to do something for a young lawyer, named Trebatius, who was destined to achieve distinction as a jurist. Caesar, however pressed with business he might be, received all such applications, when they came from men whom he cared to conciliate, with good humour. 'Just as I was speaking,' wrote Cicero, 'to our friend Balbus at my house, a letter from you was handed to me, at the end of which you say: "Rufus, whom you recommend to me, I will make King of Gaul . . . Send me some one else to provide for!" . . . I therefore send you Trebatius.'[1] The confiding lawyer wanted to make a fortune without having to work for it: but Cicero banteringly told him to moderate his expectations. 'I hear,' he wrote, 'there is no gold or silver in Britain. If so, I advise you to capture a war-chariot and come back in it as soon as you can.'[2] He ended his letter by telling Trebatius that if he wished to cultivate Caesar's friendship, he must take the trouble to make himself useful. Caesar bestowed upon him the rank of tribune, exempting him from military duty, for which he was manifestly unfit; but, after a short experience of camp life, he made up his mind that the expedition would involve more hardship than profit, and preferred to remain in Gaul. But Caesar had gained another adherent who turned out a real soldier. Quintus Cicero, the orator's younger brother, had consented to serve on his staff as a *legatus*, or general of division;[3] and a few words from a letter in which this consent is alluded to illustrate the gracious tact which helped Caesar to gain adherents. 'Caesar,' writes Marcus Cicero to his brother, 'has written to Balbus that the little bundle of letters in which mine and Balbus's were packed was so

[1] *Mescinium Rufum*, quem mihi commendas regem Galliae faciam . . . Tu ad me alium mitte quem ornem . . . Mitto igitur ad te Trebatium, &c. *Fam.*, vii, 5, § 2. See R. Y. Tyrrell, *Correspondence of Cicero*, ii, 1886, p. 112, note.

[2] In Britannia nihil esse audio neque auri neque argenti. Id si ita est, essedum aliquod capias suadeo et ad nos quam primum recurras. *Ib.*, vii, 7, § 1.

[3] See my *Caesar's Conquest of Gaul*, 1899, pp. 568–9.

saturated with rain when it was delivered to him that he was not even aware that there was one from me. However, he had made out a few words of Balbus's, to which he replied as follows:—"I see you have written something about [Quintus] Cicero, which I have not deciphered: but as far as I could guess, it was of a kind that I might wish, but hardly hope to be true."[1] On the 30th of April Quintus was with Caesar at Blandeno, a small town near Placentia. Marcus knew of course that Quintus was to accompany the expedition to Britain; and he indulged the fancy that Caesar's exploits would furnish him with a theme for a heroic poem. 'Only give me Britain,' he wrote to Quintus, 'to paint in colours supplied by you, but with my own brush.'[2] But he must have soon received discouraging news; for early in June[3] he wrote to Atticus:—'The result of the British expedition is a source of anxiety. For it is notorious that the approaches to the island are ramparted by astonishing masses of cliff; and, besides, it is now known that there isn't a pennyweight of silver in the island, nor any hope of loot except from slaves; and I don't suppose you expect any of them to be a scholar or a musician.'[4]

54 B.C.

Cicero's hopes and fears about the second British expedition.

By this time Caesar and his new lieutenant, having posted across Gaul at the rate of fifty miles a day or more,[5] must have reached the country of the Belgae; and there is no more conclusive proof of the hold which he had already obtained upon the Gallic tribes than the fact that he was

Caesar returns to Gaul.

[1] Sed ille scripsit ad Balbum fasciculum illum epistolarum in quo fuerat mea et Balbi totum sibi aqua madidum redditum esse, ut ne illud quidem sciat, meam fuisse aliquam epistolam. Sed ex Balbi epistola pauca verba intellexerat, ad quae rescripsit his verbis: 'De Cicerone te video quiddam scripsisse, quod ego non intellexi: quantum autem coniectura consequebar, id erat eius modi ut magis optandum quam sperandum putarem'. *Q. fr.*, ii, 10, § 4. Cf. R. Y. Tyrrell, *Correspondence of Cicero*, ii, 1886, p. 110, note.

[2] Modo mihi date Britanniam, quam pingam coloribus tuis, penicillo meo. *Q. fr.*, ii, 13, § 2.

[3] See p. 667, *infra*, and *Hermes*, xl., 1905, pp. 17–8.

[4] Britannici belli exitus exspectatur. Constat enim aditus insulae esse muratos mirificis molibus. Etiam illud iam cognitum est, neque argenti scripulum esse ullum in illa insula neque ullam spem praedae nisi ex mancipiis, ex quibus nullos puto te litteris aut musicis eruditos exspectare. *Att.*, iv, 16, § 7. See pp. 666–7, *infra*.

[5] See p. 727, *infra*.

54 B.C. able to count, as securely as in Italy, upon finding horses ready for each successive stage. He immediately proceeded to inspect the various shipyards, near which the troops were encamped, and was well satisfied with the manner in which his instructions had been executed. 'Thanks,' he wrote, 'to the extraordinary energy of the troops, and in spite of the extreme deficiency of resources, about six hundred vessels of the class specified and twenty-eight ships of war had been built, and would probably be ready for launching in a few days.'[1] Caesar, who knew the stimulating power of discriminative praise, bestowed hearty commendation upon officers and men, and gave orders that the ships, as soon as they were ready for sea, should all assemble in the Portus Itius. For this purpose he detached an adequate number of troops. Meanwhile his presence was urgently required in the country of the Treveri, a powerful tribe who inhabited parts of Luxembourg and Rhenish Prussia, and whose name survives in that of the modern Trèves. A squadron of cavalry furnished by this people had served on his side in the battle with the Nervii, and had deserted in a body at a moment when it seemed that he was doomed to defeat. Since that day the Treveri had refused to send representatives to attend the councils of Gallic magnates which he periodically convened; and he was now informed that they were making overtures to the Germans. Unless he recalled them to obedience, it was more than probable that while he was absent in Britain, Gauls and Germans would raise a rebellion in his rear. Accordingly, he marched against the malcontents with four lightly equipped legions and eight hundred cavalry. Fortunately for him the Treveri were not unanimous. Two rival leaders, Indutiomarus and Cingetorix, were struggling for supremacy. Cingetorix at once threw in his lot with Caesar, and gave him full information of all that was going on. Indutiomarus began to raise levies, and prepared to resist; but, finding that most of his fellow chieftains were going over to the

He is obliged to march to the country of the Treveri.

[1] singulari militum studio in summa omnium rerum inopia circiter DC eius generis cuius supra demonstravimus naves et longas XXVIII invenit instructas neque multum abesse ab eo quin paucis diebus deduci possint. *B. G.*, v, 2, § 2.

stronger side, he sent envoys to Caesar, and endeavoured to explain away his conduct. Unwilling to lose time, Caesar feigned to accept his excuses, and contented himself with taking hostages for his good behaviour. 54 B.C.

It was near the middle of June when he returned to the camp on the Liane. More than eight hundred vessels of all sorts were in the harbour, including numerous small craft, constructed by rich officers who desired to make the voyage in comfort, by merchants who had dealings with the troops, or by adventurers who, we may suppose, had been attracted by stories of the wealth of Britain;[1] but sixty of Caesar's ships had encountered contrary winds, and failed to arrive.[2] The entire Roman army, comprising eight legions, perhaps about thirty-five thousand men, besides slingers, archers, and four thousand Gallic cavalry, were assembled on the spot. The notables from all the tribes had also repaired thither in obedience to Caesar's summons. He was aware that there was much smouldering discontent among them, and he intended to take all but the few on whose fidelity he could depend, as hostages across the Channel. Among these was one whose name, as written by Caesar, was Dumnorix, and whose coins, bearing the legend DUBNOREIX,[3] still testify to the authority which he exercised. He was the most powerful chieftain of the Aedui, the most powerful Gallic tribe, whose territories, corresponding with the Nivernais and Western Burgundy, gave access to all parts of Northern and Western Gaul; who, from the time when the legions first entered Transalpine Gaul, had borne the honorary title of 'Friends and Allies of the Roman People'; and whom it had been Caesar's constant policy to treat with special favour. Dumnorix was the leader of the anti-Roman faction which existed in this as in almost every other Gallic tribe. He was a man of boundless ambition, the vehemence of whose character was out of all proportion with his judge-

Returning to the Portus Itius, he finds fleet and army assembled.

He resolves to take Gallic chiefs of doubtful fidelity as hostages to Britain.

[1] See C. E. C. Schneider's note (*Comm. de bellis C. Iulii Caesaris*, ii, 1849, pp. 43-4).

[2] See my *Caesar's Conquest of Gaul*, 1899, pp. 453-6, and H. J. Heller's remarks in *Zeitschr. für allgemeine Erdkunde*, xviii, 1865, pp. 185-6.

[3] See my *Caesar's Conquest of Gaul*, 1899, p. 816.

54 B.C. ment: he had amassed great wealth, which enabled him to maintain an army of retainers; and he had great influence not only with the lower orders in his own country but also with the Gauls of every tribe who wished to rid themselves of the Roman dominion. For the last four years his intrigues had caused anxiety to Caesar. He had been secretly in league with the Helvetian invaders at the time when Caesar marched to encounter them; and in the early part of the campaign his own brother, the famous Druid, Diviciacus, as well as the chief magistrate of his own tribe, had advised Caesar to beware of him. At that time Caesar had not felt sufficiently secure in his new position to punish him; he had simply given him a severe reprimand and a stern warning, but had ever since employed spies to watch his movements. It was now reported that Dumnorix had announced in the Aeduan tribal council that Caesar intended to make him king, and that the announcement had been received with alarm and indignation. There are writers who believe that Caesar had really offered him the throne in order to purchase his support: but it is hardly credible that he would have made such a gross miscalculation; and there is more reason in concluding that Dumnorix had spread a false report in order to estrange the loyal Aeduans from Caesar's side. At all events he was irreconcilable; and he determined that to Britain he would not go. He began by imploring Caesar to allow him to remain behind, pleading that he was not accustomed to the sea, and dreaded it, and insisting that he was debarred by religious obligations from leaving the Continent. Finding Caesar obdurate, he approached his brother chieftains, and adjured them to join him in refusing to go, assuring them that Caesar only wanted to get them out of Gaul in order that he might safely put them to death. Caesar did his utmost to keep him quiet, at the same time informing himself through his agents of all that he said and did. Meanwhile the fleet was lying idle in the harbour. All the preparations were complete: but continuous north-westerly winds made it impossible to sail; and we may safely presume that the troops, who might be required to row the transports, were employed in learning

Dumnorix resolves not to go.

The fleet weather-bound.

to use their oars. The two Ciceros were in constant correspondence; and the elder brother was impatiently waiting for the announcement that the campaign had begun. On the 2nd of July he wrote to Atticus, 'Judging from my brother Quintus's letter, I imagine that by this time he is in Britain. I am anxiously waiting for news of his movements.'[1] The fleet had been weatherbound then for about three weeks; and the chief of Caesar's commissariat, who succeeded in feeding forty thousand men for so long a period in an unfriendly country, must have possessed rare powers of organization. At length the wind shifted; and infantry and cavalry began to embark. Suddenly, while every man in the force had his thoughts concentrated on the work in hand, Caesar received news that Dumnorix and his Aeduan troopers had gone. Instantly he stopped the embarkation; and a strong detachment of cavalry was soon riding in pursuit with orders to bring Dumnorix back, or, if he resisted, to kill him on the spot: for, as Caesar afterwards said, 'he thought that a man who disregarded his authority when he was present would not behave rationally in his absence.'[2] Adjuring his retainers to be true to him, Dumnorix resisted desperately; but he was surrounded and slain, passionately crying with his last breath that he was a free man and a citizen of a free country. 54 B.C. The fate of Dumnorix.

It was about the 6th of July, probably the day after this episode, when the embarkation took place.[3] Commius, still friendly to Rome, was to accompany the expedition, as well as Mandubracius, the Trinovantian prince who had placed himself under Caesar's protection. The slaughter of Dumnorix, following the temporary submission of Indutiomarus, had relieved Caesar from imminent danger: but he knew that to keep a hold on the half-subdued and restless peoples whom he was leaving behind would require all the ability of his ablest lieutenant; and there are indications in Caesar sets sail, leaving Labienus in charge of Gaul.

[1] Ex Quinti fratris litteris suspicor iam eum esse in Britannia. Suspenso animo exspecto quid agat. *Att.*, iv, 15, § 10.

[2] nihil hunc se absente pro sano facturum arbitratus qui praesentis imperium neglexisset. *B. G.*, v, 7, § 7.

[3] See pp. 728-30, *infra*.

54 B.C. his narrative that he hoped, if all went well, to winter in Britain, and thus to find time not merely to deter the Britons from combining with the Gauls, but to conquer the south-eastern part of the country.[1] Labienus therefore remained in charge of the camp and port with three legions and two thousand cavalry. He was to keep the expeditionary force supplied with corn, ascertain all that was passing in Gaul, and act on his own discretion according to circumstances. Among the divisional commanders that accompanied Caesar was Gaius Trebonius, an intimate friend of Marcus Cicero,[2] who, two years before, had proposed, in the interests of the triumvirate, the law by which the province of Syria was assigned to Crassus, and the two provinces of Spain to Pompey. Late in the afternoon all was ready for the start, the flotilla lying moored in the harbour with five legions and two thousand cavalry on board. The ebb stream was running slowly down the coast. Towards sunset the hawsers were cast off,[3] and the ships steered north by west before a light south-westerly wind. The moon was invisible,[4] but at that time of the year there is no real night in these latitudes; and perhaps, as in the preceding year, each vessel hoisted a lantern when the twilight waned. About ten o'clock the stream began to run up the Channel, and for a time the vessels made good progress. By midnight the leading division was not far off the South Foreland, and somewhere near what is now the southern end of the Goodwin Sands; but it is probable that in steering, sufficient allowance had not been made for the current, and that the shallow flat-bottomed vessels had already drifted to leeward away from their true course. And now the wind, which had been gradually dying down, almost entirely dropped, only retaining just sufficient force to give steerage way. Borne along by a rapid flood, the armada drifted into the North Sea; and about a quarter past three, when day broke, Caesar descried the white cliffs of Kingsdown and the South Foreland

The fleet drifts north-eastward out of its course.

[1] See pp. 349, 355, 670, *infra.*

[2] Cicero, *ad Fam.*, xii, 16, §§ 2-3, xv, 21; *ad Q. fr.*, iii, 1, § 9. Cf. R. Y. Tyrrell and L. C. Purser, *Correspondence of Cicero*, iv, 1894, pp. lvii-lviii.

[3] See p. 314, n. 3, *supra.*

[4] See p. 729-30, *infra.*

receding on the port quarter. Right opposite, but hardly discernible, was the low coast on which he had landed in the previous year. We may assume that when he saw where he was drifting he anchored for a time. Presently the stream ceased to run up the Channel, and, after a few minutes' slack water, the ebb set in.[1] The Romans had a system of naval signalling,[2] and either by this means or by oral instructions conveyed from vessel to vessel, the order was given to go about and run down Channel with the stream. The soldiers on board the transports got out their oars. For some time their work was easy; but when, not far from the spot where the South Sand Head Light Vessel is now moored, the ships' heads were turned in the direction of Sandwich, they encountered a cross current setting towards the south-west.[3] Although the transports were heavily laden, they toiled with an energy which earned Caesar's warm admiration, and actually succeeded in keeping up with the galleys. About noon the whole fleet had reached the landing-place; but no enemy was to be seen, and in the course of the day a galley was speeding back across the Channel with one of Caesar's couriers on board, who carried, besides other dispatches, a letter in which Quintus Cicero informed his brother that all was well.[4]

54 B.C.

The landing-place, between Sandown Castle and Sandwich, reached by rowing.

While the troops and baggage were being disembarked, Caesar chose a site for his camp, perhaps on the slight eminence near the village of Worth. Some prisoners were soon brought in by the cavalry and questioned. They stated that their countrymen had assembled in large numbers to oppose the landing, but that, on observing the huge size of the armada, they had abandoned the shore and retreated to higher ground inland. Caesar determined to march against them that very night, and accordingly accepted the risk of not hauling his ships up on shore, an operation which would have consumed valuable time. He had not forgotten the disaster of the previous year; but, as the shore where he now left the ships at anchor was not only perfectly open but

Leaving the fleet at anchor in charge of a brigade, Caesar marches against the Britons,

[1] See p. 658, *infra*.

[2] Polybius, i, 50, §§ 7–8, 51, § 1. Cf. x, 43–7, and *Ency. Brit.*, xxii, 1887, p. 49.

[3] See pp. 655–9, *infra*.

[4] See p. 348, *infra*.

54 B.C. sloped very gently seaward, he felt little anxiety for their safety.[1] He mentioned this fact in his memoirs[2] with an emphasis which suggests that he wished to deprecate professional criticism. Moreover, the storm which had wrought such havoc before had occurred on the night of a full moon: the moon was now new; and it may be doubted whether Caesar had studied the writings of the Greek astronomers, or consulted the pilots, from whom he would have learned that the tides at new and at full moon are virtually identical. Ten cohorts selected from the various legions, or about four thousand men, and three hundred cavalry were left, under the command of an officer named Quintus Atrius, to protect the fleet. Soon after midnight Caesar set out against the enemy. We may presume that he had sent a troop of cavalry in the afternoon to reconnoitre; but he must have trusted to his prisoners for information as to the whereabouts of the British force. It was posted on high ground overlooking Durovernum, the village which stood upon the site of Canterbury, and which the Romans afterwards linked by a system of roads with their settlements at London, Reculver, Richborough, Dover, and Stutfall near West Hythe. The general direction of Caesar's march is indicated by the road which runs across the gently undulating and somewhat featureless country between Sandwich and Canterbury. He had advanced about eleven miles when, in the early morning, he descried the enemy's cavalry and charioteers descending from high ground towards the left bank of the Stour. The spot where he encountered them must have been somewhere between Sturry on the east of Canterbury, and Thanington on the west; and military experts who know the country will probably conclude that it was near the latter.[3] The enemy had doubtless attempted to occupy the whole range of low hills which closes the valley of the Stour between these two points, prepared to oppose the legions wherever they might attempt to cross. It would seem, however,

[1] See pp. 664–5, and 673–4, *infra*

[2] eo minus veritus navibus quod in litore molli atque aperto deligatas ad ancoras relinquebat. *B. G.*, v, 9, § 1.

[3] See pp. 682–5, *infra*.

that their resistance was comparatively feeble, perhaps because they were surprised, and, having needlessly strung out their forces, were unable to concentrate in time. Caesar may have sent a detachment to turn their position: anyhow they were driven from the banks after a combat which he recorded in a single sentence. Retreating to the higher ground, they took up their position in a stronghold situated in the midst of woods,—probably the earthwork, about a mile and a half west of Canterbury, through which runs the Pilgrims' Way, and within which, as we have seen, have been discovered iron implements and weapons of pre-Roman age.[1] The legions, pressing after them, found the entrances blocked by *abatis*; and when they attempted to force their way in, the Britons, issuing from the woods in small groups, assailed them with showers of missiles. It would appear from Caesar's narrative that the rampart, or at least a part of it, extended along the edge of the wood. The 7th legion was selected for the assault. Advancing in a dense column, with shields close-locked over their heads, they shot earth or fascines into the ditch so as to form a causeway flush with the top of the rampart; and it may be conjectured that the work was performed by men who advanced between the files under the protection of their comrades' uplifted shields.[2] In this way the entrenchment, which, like all the British forts that Caesar saw, was weaker than the great strongholds of Western Britain, was speedily captured with small loss; and the Britons were expelled from the woods. The legionaries followed up their success, but Caesar soon stopped the pursuit. He was afraid to run the risk of letting his troops get entangled in a wooded country, of the intricacies of which he was ignorant; and, as it was late in the afternoon, he was obliged to utilize the remaining hours of daylight for the construction of his camp.

54 B.C.

forces the passage of the Stour near Canterbury,

and storms a fort, to which they had retreated.

Early next morning he dispatched his cavalry in three columns, each supported by a strong body of infantry, to hunt down the fugitives. The pursuers had advanced a considerable distance from the camp, the rearguard being

Next morning he sends three columns in pursuit,

[1] See p. 253, *supra*.

[2] See A. von Göler, *Gall. Krieg*, 1880, p. 149, n. 5.

54 B.C. still in sight,[1] when some troopers rode up to Caesar with a note from Atrius. A storm had arisen on the previous night: the ships had parted from their anchors, collided with one another, and almost all been dashed ashore and damaged.

but is forced to recall them by news that many of his ships had been wrecked.

Caesar sent gallopers to recall the pursuing columns, and order them to march back to the coast, defending themselves, if necessary, against a counter-attack, and started in person for the scene of the wreck.[2] When he arrived, he found that Atrius's report was accurate: about forty ships were totally destroyed; but, after inspecting the rest, he saw that it would be possible to repair them. In the course of the day the legions arrived. The men who had enlisted as skilled craftsmen were segregated and set to work; and galleys were sent to Labienus with a letter in which he was ordered to dispatch gangs of shipwrights from his three legions, and to employ the rest of the men in building new vessels. Caesar reluctantly concluded that the only way of preventing another disaster was to have all the ships hauled up on land out of reach of the highest spring tides. They were doubtless moved in the usual way, by capstans over greased logs, which the Romans called *phalangae*;[3] and then, in order to secure them against attack, an earthwork was thrown up round them, and connected with the existing camp. The amount of labour which these operations entailed was enormous: but there were some twenty thousand willing workers; and by employing them in relays all day and all night, Caesar was able to complete the task in about ten days. The repairs of course required a longer time.

He beaches the ships, constructs a naval camp, and repairs damage.

Results of the disaster.

This second shipwreck was a calamity of which the mere loss in ships formed the smallest part. It changed the course of the campaign. Why had not Caesar restrained his eagerness to close with the enemy, and employed every available man in beaching the vessels which he had constructed with that very aim? Granted that it might not have been possible to complete even the mere work of dragging them all out of reach of the waves before the storm began, he would still have done right in not presuming

[1] See pp. 686–7, *infra*. [2] See pp. 685–8, *infra*.
[3] Forcellini, *Totius latinitatis lex.*, iv, 1868, p. 651.

upon the favour of fortune. Nobody knew better how necessary it is, especially in making war upon a half-civilized enemy, to complete all preparations, even at the cost of delay, before opening the campaign, so as to lose not a moment in following up an initial success, and to give fugitives no time to recover from their demoralization. Less than two days after he set foot in Britain he had dealt the enemy a succession of heavy blows, and the game was in his hands,—when all that he had done was undone by his own carelessness. Britons saw Romans in full retreat, and concluded that they were not invincible. 54 B.C.

By the time when the naval camp was finished the season was far advanced. It was near the end of the third week in July when Caesar was able to renew his campaign. The Britons had made good use of their respite. The tribes had suspended their feuds: Cassivellaunus had been called upon by a general assembly of notables[1] to undertake the chief command with full powers; and a large force, composed of contingents from all, or almost all, the cantons of the south-eastern district, had marched to join the men of East Kent. We may doubt whether the Trinovantes had not held aloof; but if they had been forced to join the league, they were half-hearted. It is certain that, before Caesar had been long in the island, they sent envoys, promising submission and begging him to send Mandubracius back to them as their ruler and to protect him against Cassivellaunus. He allowed Mandubracius to depart, only stipulating that the Trinovantes should give him forty hostages and provide grain for his army; and readers who can interpret the *Commentaries* will conclude that the embassy was dispatched before he had advanced far into the interior, and doubtless as soon as he had proved his superiority. He left the same force as before—ten cohorts and three hundred cavalry—to protect the camp, and marched once more in the direction of Canterbury. As he was approaching the valley of the Stour, the enemy's cavalry and charioteers commenced a fierce running fight with his Gallic cavalry; but they were

Caesar again marches towards Canterbury. Cassivellaunus elected commander-in-chief of the Britons.

[1] *B. G.*, v, 11, § 8. Cf. i, 30, § 4; ii, 4, § 4; iii, 8, § 3; vii, 63, §§ 6-7; and my *Caesar's Conquest of Gaul*, 1903, p. 12.

54 B.C. beaten back at all points and driven to take refuge on the wooded heights near the river. The Gallic cavalry, however, over-eager to pursue, and getting entangled in ground which was unknown to them, suffered considerable loss; and soon afterwards, while the legionaries, careless of danger, were engaged in entrenching their camp, the enemy suddenly swooped down upon the cohort on guard and began to overpower it. Caesar had not yet learned due respect for his enemy; otherwise he would have kept a much more powerful force, as he had done on a similar occasion in Gaul, to protect the working-parties. He sent two cohorts, however, to support the struggling guard and cut off the retreat of the assailants. These reinforcements were separated from one another by a narrow interval: the men who composed them, and who had not served in the preceding year, were unnerved by the novel tactics of the charioteers; and the enemy boldly rushed through the interval, and got back to the main body unhurt. Several additional cohorts, accompanied by cavalry, were sent to retrieve the situation. The combat was clearly visible from the camp; and Caesar saw that his troops, who had so often routed their continental enemies, were at a serious disadvantage. The Britons fought not in close order but in small groups, separated by wide intervals; and when these were tired, their places were taken by reserves. Whenever a group was hard pressed by the legionaries, the men who composed it ran away: the Romans, weighted by their heavy armour, were ineffective in pursuit; and, besides, accustomed as they were to fight in compact masses, they and their officers naturally failed to adapt themselves to new conditions. Again, when the Gallic cavalry charged the charioteers, the latter drove rapidly away; and, as soon as they had withdrawn their assailants from the support of the legions, the warriors leaped to the ground, and, supported by their own cavalry, fought as infantry, with the odds in their favour.[1] A tribune named Quintus Laberius Durus was killed; but at length the reinforcements which Caesar sent up succeeded in beating back the Britons, or

The Romans harassed by British charioteers.

[1] See pp. 688–91, *infra*.

at all events deterring them for the moment from renewing their attack. 54 B.C.

All this time Caesar was doubtless fighting to gain the line of the road or trackway by which he would have to march westward into the interior of Britain and assail the dominions of Cassivellaunus. But it was of course out of the question to begin his march until he had inflicted a crushing defeat upon the allies; and, as he saw now, their game was to avoid a general action. On the following day, however, a chance presented itself. In the morning the enemy, who had taken up a position on the heights at some distance from the Roman camp, moved down, as before, in scattered groups, and began to assail the cavalry outposts, but with somewhat diminished vigour. The outposts fell back; and presently the whole of the cavalry were sent out, along with three of the legions, under Gaius Trebonius, on a foraging expedition. Part of the force proceeded to cut grass, while the rest remained drawn up in support. Suddenly the enemy rushed down from all points on the foragers, and, made reckless by success, 'did not even hesitate,' as Caesar wrote, 'to attack the ordered ranks of the legions.'[1] The Romans charged them fiercely, and took ample revenge for the previous day. The Britons were driven from the field, hotly pursued by Trebonius and his men, until the Gallic cavalry, relying upon the support of the legions, which still followed as closely as they could, hunted them in headlong rout, cutting them down in numbers, and never giving them a chance of rallying. Not even the charioteers could get a moment's respite, or dared to dismount and turn upon their pursuers. This defeat was decisive. The tribal levies of foot at once dispersed to their homes; and 'from that time', wrote Caesar, 'the enemy never encountered us in a general action.'[2]

Trebonius routs the Britons.

The British infantry disperse.

Cassivellaunus had learned a lesson which his kinsmen on the other side of the Channel were already taking to heart.

War-chariots *versus* Roman troops.

[1] . . . repente ex omnibus partibus ad pabulatores advolaverunt, sic uti ab signis legionibusque non absisterent. *B. G.*, v, 17, § 2. See p. 692, *infra*.

[2] . . . neque post id tempus umquam summis nobiscum copiis hostes contenderunt. *B. G.*, v, 17, § 5.

54 B.C. His undisciplined foot were evidently powerless to contend against the legions on a fair field, and, except behind works, in a strong position, or in attacking small bodies which had been carelessly isolated, they were of little use. The Celtic infantry of the more warlike tribes were not indeed to be despised. The Helvetii with their allies made a stubborn fight against Caesar: the Parisian confederation under the veteran Camulogenus tested the mettle of Labienus; and the issue of the battle with the Nervii remained long doubtful. But in all these combats the Celts had a great numerical advantage; and in all they were beaten to the verge of annihilation. Cassivellaunus saw that his object was not to be attained by regular warfare. Moreover, it is certain that, during a prolonged campaign, he would have been unable to feed a large army. But he still had four thousand charioteers with the cavalry who supported them;[1] and on them he determined to rely. The success with which he had already used them makes us wonder why the Continental Celts had abandoned the arm which their insular kinsmen wielded with such effect. Less than a century before Caesar crossed the Alps chariots had been generally employed in Eastern and in Central Gaul.[2] Chariots have been found in scores in the great sepulchres of the Iron Age which have been opened in Burgundy and Champagne, while in the British barrows their remains are extremely rare.[3] It is evident to every reader of the *Commentaries* that Caesar was at his wits' end to know how to adapt his organization to this strange form of resistance; and it is equally evident that on his own side of the Channel he never encountered it at all. The most satisfactory explanation is to be found in a passage of the *Commentaries* from which we learn that the Gauls spent large sums in buying well-bred horses.[4] Evidently they discarded chariots for cavalry when they began to import from Southern Europe horses which were powerful enough to carry big men and charge

[1] See p. 675, *infra*.
[2] See G. Dottin, *Manuel pour servir à l'étude de l'ant. celt.*, p. 197.
[3] See p. 676, *infra*.
[4] *B. G.*, iv, 2, § 2.

with effect.[1] The German cavalry, it is true, had only small underbred cattle; but they were virtually mounted infantry.[2] The British may have been well or ill mounted; but for the most part British horses were no bigger than ponies,[3] able to draw a light car but not to gallop fast with heavy riders. Still, whoever calls to mind how in the last Samnite War the Gallic chariots routed the Roman cavalry,[4] will perhaps doubt whether the Gauls did well to abandon chariots altogether in favour of mounted troops. 54 B.C.

Nevertheless the reader who trusts to his first impressions of Caesar's narrative is prone to exaggerate the successes of the British charioteers. Their object was to break up the formation of their opponents; and this they could only do when carelessness gave them an opening. The punishment which they inflicted upon the 7th legion was invited by the almost incredible negligence of its commander: the check which Caesar himself suffered in the following year befell an outpost of inadequate strength. In irregular warfare chariots could cause serious trouble; but the difficulty which Caesar found in dealing with them was partly due to the fact that his army, like all Roman armies, was weak in cavalry,—and in cavalry of the right kind. If he could have taken to Britain one of those German squadrons with their attendant light infantry which so effectively supported him in the war with Vercingetorix, he would have had less trouble in his encounters with the British charioteers.

Caesar marches for the country of Cassivellaunus,

Caesar now marched for the country of Cassivellaunus, who, as he divined, intended thenceforth to wage a guerrilla warfare. The troops must have carried in their wallets rations for several days, drawn from the magazine in the naval camp; for they could not count upon getting supplies from the farms till they reached the territory of the Trinovantes; and we may be sure that Caesar, venturing into an unexplored country and against so troublesome an enemy, dispensed as far as possible with transport. What route he

[1] W. Ridgeway, *Origin and Influence of the Thoroughbred Horse*, pp. 94–5, 102–3.

[2] *B. G.*, i, 48, §§ 4–7; iv, 2, §§ 2–3; vii, 65, § 5.

[3] See p. 676, *infra*.

[4] Livy, x, 28–30.

54 B.C. followed is an interesting but perhaps insoluble question. He dismisses the story of the march, which must have occupied nearly a week, in a single sentence, which contains no clue. We know only that he started from the neighbourhood of Canterbury, and that he crossed the Thames at or not far from Brentford.[1] It is, however, morally certain that he marched either by the trackway on the line of which the Romans of a later period made the great road called Watling Street, which crosses the Medway between Rochester and Strood, or along the southern slope of the chalk escarpment, and across the Medway at Aylesford or Halling. All the antiquities of Roman or pre-Roman age that have been discovered in Kent, west of the maritime tract which is bounded by a line drawn from Reculver through Canterbury and Lympne to Romney, have come from sites clustering alongside these routes.[2] That Caesar makes no mention of the Medway has no significance. He must have crossed it somewhere; and it is certain that he crossed many rivers to which he never alluded unless the passage had some tactical or strategical importance. His narrative shows that his object was to inflict the greatest damage possible upon the enemy's homesteads and farms; and we may reasonably suppose that he followed the route, leading through a fertile and populous country, which his successors selected, diverged from it somewhere near Rochester, and thence advanced by way of Bromley. But the matter is of no great consequence. Caesar demands from his readers not only attention and intelligence, but also expert knowledge; but from those who possess these qualifications he rarely withholds necessary information: when he baffles their curiosity, his silence does not prevent them from understanding what is essential.

whose chariots harass his cavalry.

During a great part of the march Cassivellaunus dogged the Roman column. Caesar's object was to strike terror; and he sent out parties of cavalry to devastate the country and despoil the inhabitants of their chief source of wealth,—their flocks and herds. But Cassivellaunus soon taught him

[1] See pp. 692–8, *infra*.

[2] *Archaeologia*, li, 1888, map facing p. 446.

a lesson of caution. He succeeded in ascertaining what route the Romans intended to pursue, and sent messengers to warn the inhabitants to drive their cattle into the woods and to fly for refuge thither themselves. Knowing every inch of the country, and having the advantage of superior mobility, he would conceal his force in some wooded spot, and when he saw the Roman horsemen diverge from the column and ride forth to plunder, swoop down upon them and inflict heavy loss. Caesar was compelled to keep his cavalry, who were terrorized by these unforeseen attacks, in constant touch with the infantry; while the legions, whose powers of endurance were taxed to the uttermost, moved off the road from time to time, and burned and ravaged whatever they could reach.[1] 54 B.C.

Caesar had ascertained that the Thames, in that part of its course which formed the southern boundary of the territory of Cassivellaunus, was only fordable at one spot; and since the time of Camden it has generally been supposed that this was close to Halliford,—the only place, it is said, between Hurleyford, about two miles west of Great Marlow, and the sea, whose name preserves the memory of an ancient ford.[2] Evidence, however, has lately been adduced which makes it more probable that Caesar was describing Brentford; for, though the name may only have denoted a ford over the Brent, in this part only of the lower Thames have piles been discovered in dredging operations which could reasonably be identified with the obstacles that threatened the passage of the Roman army.[3] When the column descended into the valley, Caesar found that Cassivellaunus had anticipated him. The further bank was fenced by a row of sharp stakes, behind which were massed Cassivellaunus's tribesmen; and Caesar learned from prisoners and deserters that similar stakes, concealed by the water, were planted in the bed of the river. He sent his cavalry behind cover to swim the stream close by; and at the right moment the column of infantry plunged into the water, and advanced to the attack. Caesar had calculated that the British levies Caesar crosses the Thames.

[1] See A. von Göler, *Gall. Krieg*, 1880, p. 157, n. 1.

[2] See pp. 692–6, *infra*.

[3] See p. 697, *infra*.

54 B.C. would be distracted by the onset of the cavalry upon their flanks and rear; but the infantry were determined to have the credit for themselves. We may suppose that, while they were removing the stakes, the slingers and archers harassed the enemy.[1] 'The infantry,' wrote Caesar, 'advanced with such swiftness and dash, though they had only their heads above water, that the enemy, unable to withstand the combined onset of cavalry and infantry, abandoned the bank and fled.'[2]

Cassivellaunus orders the kings of Kent to attack the naval camp.

But Cassivellaunus did not despair. Before Caesar crossed the Thames, he had sent mounted messengers to order the four petty kings of Kent to raise all their tribesmen instantly and make a sudden attack upon the naval camp.[3]

Caesar enters the country of the Trinovantes, who furnish hostages and grain.

Meanwhile Caesar was moving eastward into the country of the Trinovantes. Cassivellaunus haunted his line of march as before, and pursued the same harassing tactics; but the legionaries succeeded in doing considerable damage. When, however, they crossed the frontier of the Trinovantes, Caesar was careful to restrain them from committing any act of violence. The Trinovantes punctually handed over the hostages and delivered the grain which Caesar had required from them; and several other tribes which had joined the defensive league, seeing that they had been rewarded for their submission, sent envoys to announce their surrender. These tribes were the Cenimagni, the Segontiaci, the Ancalites, the Bibroci, and the Cassi. The last three do not reappear in history: they were evidently dependent tribes, and nothing is known about their geographical position except that they lived somewhere in the basin of the Thames, on the west or possibly on the north of the Trinovantian territory in Essex.[4] The territory of the Segontiaci, judging by

Five of the confederate tribes submit.

[1] See pp. 698–9, *infra*.

[2] Sed ea celeritate atque eo impetu milites ierunt, cum capite solo ex aqua extarent, ut hostes impetum legionum atque equitum sustinere non possent ripasque dimitterent ac se fugae mandarent. *B. G.*, v, 18, § 5.

[3] See *Archaeologia*, ii, 1773, p. 166.

[4] See *Vict. Hist. of . . . Norfolk*, i, 284–5. I am inclined to think that the Bibroci, whether their name is connected with that of Berkshire or not, and the other two may have lived on the south of the Thames. Otherwise would they not have been clans either of the Trinovantes or the Catuvellauni? It

coins, may have been conterminous with, and was probably north of that of the Atrebates,[1] who occupied parts of Hampshire and Berkshire.[2] The Cenimagni may have been the people who dwelt in Suffolk and Norfolk,[3] and who, under the name of Eceni or Iceni, rose in revolt under Boadicea, a century later, against the Romans. The envoys told Caesar that the stronghold of Cassivellaunus was not far off, and that a large number of the inhabitants with their flocks and herds had taken refuge in it. Possibly it was Verulamium, near St. Albans,[4] which was in later times the capital of the son and successor of Cassivellaunus, though Caesar seems to imply that there was no permanent settlement within the fortress: at any rate it was not far west of the river Lea, which formed the boundary of the Trinovantes. When Caesar arrived, he found that the stronghold was protected by woods and marshes, and fortified with a rampart and trench: but the legions, advancing on two sides, speedily carried the place by assault: many of the Britons, as they were endeavouring to escape, were caught and killed; and all their cattle were taken. 54 B.C.

Meanwhile the counter-attack which Cassivellaunus had ordered had been delivered. The extent of the naval camp, enclosing as it did several hundred vessels, might appear disproportionate to the slender force to which Caesar had entrusted its defence; but he had made no miscalculation. Probably the entrenchment was protected at intervals by towers like those which he used to strengthen his lines at Alesia, and from which artillery could play upon the flanks Attack on the naval camp repulsed.

seems unlikely that any group included in the latter would have dared in spite of Cassivellaunus to surrender. Dr. Haverfield (R. L. Poole's *Hist. Atlas of Mod. Europe*, 1896, xv,—'Roman Britain') suggests that the Bibroci may have been in Berkshire, and that they and the Segontiaci were clans of the Atrebates.

[1] See J. Evans, *Coins of the Anc. Britons*, pp. 41, 225, 272–5, Suppl., pp. 534, 539–40; and *Vict. Hist. of . . . Hampshire*, i, 273. Sir J. Evans says that the word SEGO on coins of Tasciovanus 'seems plainly to point to the tribe of the Segontiaci'; and as VER. on coins stands for Verulam, so SEGO. may stand for Segontium, the site of which is, however, unknown.

[2] *Vict. Hist. of . . . Hampshire*, i, 273–4.

[3] *Vict. Hist. of . . . Norfolk*, i, 284–5.

[4] See pp. 699–702, *infra*.

54 B.C. of the assailants. A chieftain named Lugotorix was chosen to lead the assault; but the garrison made a sortie, beat off the Britons with considerable loss, and captured their commander.

Caesar's hurried journey to the coast and its significance.

It was perhaps just after this event that Caesar, accompanied by a flying column, made a journey to the coast, of which he omits all mention in the *Commentaries*. His silence, which can hardly have been unintentional, certainly suggests that the news of the attack—perhaps the information that it was about to be delivered—caused him serious anxiety. On the 5th of August (the 1st of September of the unreformed calendar) he wrote a letter from the naval camp to Marcus Cicero. A service of dispatch vessels had been organized, which plied from time to time between the Kentish coast and the Portus Itius. Caesar had found time to write at least once before; and the younger Cicero had sent a long series of letters to his brother, whose allusions to them reveal something of the inner history of the campaign. In the first week of August he replied to the one which had described the safe arrival of the armada :—'How I rejoiced at your letter from Britain. I was nervous about the sea and the coast of that island. I don't underrate what you have still to do; but there is more ground for hope than fear.'[1] On the 1st of September he dispatched a long letter, written in instalments, in which he acknowledged the receipt of four successive letters :—'I gather from yours,' he said, 'that we have no occasion either for fear or exultation.'[2] The letter to which he here alludes—the first of the series—was written before the 16th of July, that is to say, while the construction of the naval camp was still going on. Caesar's first letter was written in a spirit so friendly that it gave him the keenest pleasure, mingled with pain; for he knew that Caesar could not long remain in ignorance of the death of his daughter, Julia, the wife of Pompey. Towards the end of the

[1] O iucundas mihi tuas de Britannia litteras! Timebam Oceanum, timebam litus insulae. Reliqua non equidem contemno, sed plus habent tamen spei quam timoris, &c. *Q. fr.* ii, 15 (16), § 4.

[2] De Britannicis rebus cognovi ex tuis litteris nihil esse nec quod metuamus nec quod gaudeamus. *Ib.*, iii, 1, § 10.

letter of September 1 he says, 'Caesar wrote me a letter on the 5th of August, which reached me on the 31st, satisfactory enough as regards affairs in Britain, in which, to prevent my wondering at not getting one from you, he tells me that you were not with him when he reached the coast.'[1] Caesar did not, it would seem, write again until the 29th of August, after which about a fortnight elapsed before he quitted the island; and it is hardly credible that he should have spent more than five weeks inactive at the sea. The only conclusion is that he had some urgent motive for leaving the main body of his army and undertaking a journey of seventy miles, and that this journey was connected with the attack upon the camp. Perhaps he desired to see for himself that the defences were secure against any future attempt, to reinforce the garrison, and to ascertain what progress had been made in the repair of the fleet.[2] 54 B.C.

Cassivellaunus sues for peace.

But Cassivellaunus had by this time begun to lose heart. His country had been harried without mercy; his people had been dragged off by hundreds to be sold as slaves; and —what he valued most of all—his cattle had been taken away from him. Discredited by reverses, he had not been able to hold his ill-assorted confederates together; their defection left him powerless to retrieve his fortunes; and his last great stroke had failed. He therefore sent envoys to the Roman camp to propose surrender, and requested Commius to negotiate for him.[3] Caesar, on his part, was glad to be able to leave the island with a semblance of success. He had originally intended to winter in Britain and renew the war in the following spring. But Labienus had just warned him that the outlook in Gaul was threatening: the season for campaigning was nearly at an end; and he was aware that Cassivellaunus could still maintain a guerrilla

[1] Ex Britannia Caesar ad me Kal. Sept. dedit litteras, quas ego accepi A.D. IIII. Kal. Octobr., satis commodas de Britannicis rebus, quibus, ne admirer quod a te nullas acceperim, scribit se sine te fuisse, cum ad mare accesserit. *Ib.*, § 25.

[2] See pp. 672, 731–3, *infra*.

[3] Such is Caesar's statement (*B. G.*, v, 22, § 3): but only his perfervid admirers deny that in certain passages of his memoirs he was guilty of misrepresentation; and there are critics who argue that he employed Commius to induce Cassivellaunus for a consideration to negotiate. See pp. 669–71, *infra*.

54 B.C. warfare. He was obliged therefore to content himself with demanding hostages, fixing a sum which the tribes that had belonged to the league were to pay annually as tribute to Rome, and admonishing Cassivellaunus to leave the Trinovantes and their king unmolested.

Caesar and his army return to Gaul.

The hostages were handed over without delay; and Caesar, with his army and his train of captives, marched back to the coast. He found all the ships which it had been possible to repair ready for sea: but the number of those which had been condemned was not inconsiderable; and, as the prisoners were very numerous, he determined to effect the transport in two successive trips. With the first convoy went one of his couriers, bearing letters from him and Quintus to the elder Cicero. Their purport is preserved in one of Cicero's letters to Atticus:—'On the 26th of September I received letters from my brother Quintus and from Caesar, dated from the nearest coasts of Britain on the 29th of August. They had settled affairs in Britain, received hostages, and imposed tribute, though they had got no booty, and were on the point of bringing the army back.'[1] Caesar expected that when the empty transports returned, they would be accompanied by sixty ships, which had just been launched by Labienus; but only a few either of the old or the new vessels arrived, the rest having been driven back by contrary winds. Day after day Caesar waited for them with increasing anxiety; for the equinoctial gales might soon be expected. At length he made up his mind that he could wait no longer. The few available vessels were inconveniently crowded: but the sea was perfectly smooth, and, leaving the Kentish coast between

About Sept. 15.

nine and ten at night, the fleet rowed into the harbour at break of day. In spite of all the perils to which they had been exposed in their numerous voyages, not a man had been lost at sea, not a ship had foundered in either year.

While Caesar was still in Britain he had begun to collect

[1] A Quinto fratre et a Caesare accepi A.D. IX. Kal. Nov. litteras, datas a litoribus Britanniae proximis A.D. VI. Kal. Octobr. Confecta Britannia, obsidibus acceptis, nulla praeda, imperata tamen pecunia, exercitum e Britannia reportabant. *Att.*, iv, 18, § 5. See pp. 712–3, 726, *infra*. Strabo (iv, 5, § 3) says that Caesar got much booty besides slaves.

materials for a description of the island and of the manners and customs of its inhabitants. Partly, indeed, it may have been based upon the account of the Greek historian, Timaeus, who had himself derived material from the journal of Pytheas;[1] but certain sentences embodied the results of his own observation. What specially struck him as he marched through the country was the density of the population and the superiority in material civilization of the people of Kent. 'The population,' he wrote, 'is immense: homesteads, closely resembling those of the Gauls, are met with at every turn; and cattle are very numerous.'[2] His curiosity was excited by the statement, which he had seen in one of his Greek authorities, and the origin of which we have already endeavoured to trace,[3] that in some of the islands off the mainland there was continuous night for a month about the winter solstice. 'Our inquiries,' he tells us, 'could elicit no information on the subject; but by accurate measurements with a water-clock we ascertained that the nights were shorter than on the Continent.'[4] It would be useless to guess from what authority he derived the puzzling statement that groups of ten or twelve men had wives in common, brothers sharing with one another and fathers with their sons;[5] in other words, that one of the British customs was polyandry. Thoughtless commentators have condemned the passage as simply untrue: it has been explained as the outcome of a misunderstanding; and an eminent scholar, with a theory that needed every support, has insisted that it was merely a blundering description of the primitive institution of matriarchy, which he believed to have survived among the Picts of a later time.[6] We can only be sure that neither matriarchy nor polyandry existed among the dominant Celts; but it is permissible to suppose that certain primitive communities in remote districts had some usage which gave

54 B.C. Caesar's description of Britain.

[1] See p. 499, *infra* and Pauly's *Real-Encyclopädie*, iii, part i, 1897, p. 863.

[2] Hominum est infinita multitudo creberrimaque aedificia fere Gallicis consimilia, pecoris magnus numerus. *B. G.*, v, 12, § 3.

[3] See pp. 225-6, *supra*.

[4] Nos nihil de eo percontationibus reperiebamus, nisi certis ex aqua mensuris breviores esse quam in continenti noctes videbamus. *Ib.*, 13, § 4.

[5] *Ib.*, 14, §§ 4-5.

[6] See pp. 414-7, *infra*.

54 B.C. colour to Caesar's statement. But perhaps the most remarkable feature in his description was the approximate accuracy of his estimate of the size of the island. He was told that its circumference was two thousand miles; and this information was certainly not derived either directly or indirectly from Pytheas, whose estimate, if Strabo has reported it correctly, was monstrously exaggerated.[1] On the other hand, Caesar, although, like Pytheas, he placed Ireland in its true position, imagined, in common with other geographers who derided Pytheas's teaching, that the Gallic coast, from the Rhine to the Pyrenees, was roughly parallel with Southern Britain.[2]

Review of Caesar's invasions of Britain.

The story of these invasions is not without interest for students of military history. In Britain Caesar was confronted by tactical problems of an entirely strange kind; and he did not dissemble the difficulty which he had experienced in attempting to solve them. The Roman soldiers had been trained to encounter an enemy who fought in close order; if ever, in the stress of unforeseen circumstances, such as those which beset the foragers of the 7th legion, they found themselves cut off from the standards which they were accustomed to rally round, they felt that they were but the units of a mob.[3] It was not perhaps that they lacked the intelligence which enabled the German soldier in 1870 to adapt himself to new conditions. The coolness with which, in the fearful combat with the Nervii, each legionary shook off the effects of his surprise, and, disentangling himself from the press, 'fell in by the standard he first caught sight of,'[4] and fought as steadily as under his own centurion, shows that in Caesar's soldiers no moral, as no physical, military qualification was wanting. But encompassed by those rushing chariots, assailed by those nimble groups of skirmishers who would not come to sword's point with them, they found themselves helpless. And when they advanced with ranks closed—for the enemy never

[1] See p. 223, *supra*.

[2] See my *Caesar's Conquest of Gaul*, 1899, p. 480.

[3] Cf. Caesar, *B. C.*, i, 44, § 4.

[4] Quae prima signa conspexit, ad haec constitit. *B. G.*, ii, 21, § 6.

succeeded in breaking their formation—the charioteers could easily keep out of the way and concentrate the whole weight of their attack upon the cavalry, which they had lured away from their support. Cassivellaunus handled his levies with commendable skill; and if he did not deserve from Caesar the admiration that makes itself felt in the terse chapters which mirror the tremendous personality of Vercingetorix, he was a leader of no ordinary capacity, raised to his high place by merit alone. For the mistake which gave Trebonius the opportunity of dealing him that staggering blow near the banks of the Stour—the rush of his tribesmen, intoxicated by success, upon the ranks of the legions—not his lack of judgement but their lack of discipline was responsible. And if, instead of disbanding his infantry and following Caesar's march with his chariots, he had then had the hardihood to let Caesar go his way, and, leaving his cattle, his homesteads, and his granaries to their fate, had hurled his entire force, combined with the levies of the Kentish kings, against the little garrison which held the naval camp, it might have gone hardly with Caesar. For, like the weak cohorts with which Galba strove to hold his camp in the Valais against a host of mountaineers, the garrison would have been compelled to defend themselves without respite against assailants whose numbers enabled them to fight and rest by turns; and if, like Galba's men, they had attempted to disperse their enemies by a sortie, they would have been attacked in flank and rear by the charioteers and cavalry. Perhaps, indeed, Cassivellaunus saw what to do, but was not sufficiently master of his countrymen to do it. He who can keep in hand an aggregate of levies, shattered by defeat in a regular combat which they should never have fought, must needs be a king of men. Caesar understood the weaknesses of half-civilized tribes, and knew what risks he might fairly run. Just as Vercingetorix was compelled by his tribesmen to let go his hold upon the country of the Bituriges, where he barred Caesar's advance, and to leave the way open to him by returning to succour their farms, so Cassivellaunus, we may be sure, would not have been able to withstand the clamours that

54 B.C.

54 B.C. would have bidden him go to the rescue of the threatened dominions of the Catuvellauni and their allies, even if, by sacrificing them, he could have cut the invaders' communications, and detained him a prisoner in Britain. One may be allowed perhaps to speculate whether Caesar, if he had himself had much experience of British tactics in his first expedition, would have been able, without sacrificing the advantage of discipline, to train his troops in the intervening winter to adapt their formation to the methods of attack which they had to expect; or whether it would have been possible for him then, as it was two years later, to enlist the invaluable aid of German cavalry: but in his second campaign he speedily corrected the mistakes which his sanguine temperament had led him to make; and in his mode of conducting the war he conformed so closely to the maxims which the foremost British soldier of our time, himself an enthusiastic admirer of the *Commentaries*, has laid down for generals who have to command against uncivilized enemies,[1] that one might almost suppose those maxims to have been derived from a study of the campaign. By marching in the night to seek out his enemy after his disembarkation, he gained the advantage which is the reward of a secretly-planned, sudden, and swift movement against an undisciplined foe. Instantly following up his success, he taught the fugitives that the strongholds which kept their own countrymen at bay were of little avail against Roman soldiers. As soon as he was free to advance into the interior, he demoralized his enemies by rapidity of movement and incessant energy; and by ruthlessly destroying their crops, seizing the stores upon which they depended for subsistence, and driving off the cattle, which were their most valued possession, he succeeded, within a few weeks, in bringing the campaign, which fortune would not permit him to continue, to a successful conclusion.

[1] Lord Wolseley, *The Soldiers' Pocket-Book*, 5th ed., pp. 286, 412–7.

CHAPTER VIII

THE RESULTS OF CAESAR'S INVASIONS OF BRITAIN

54 B.C.—A.D. 43. The importance of Caesar's British expeditions under estimated by his contemporaries and by historians.

CAESAR'S contemporaries and the Roman writers of succeeding generations did not over-estimate the results of his British campaigns. The well-known line of Lucan—

Territa quaesitis ostendit terga Britannis[1]—

is only worth quoting as an instance of the poet's animosity; but the impression left by the various passages which refer to Caesar's expeditions is, that public expectation, having been wrought up to a high pitch, had suffered disappointment.[2] Everybody knew that Caesar had not incurred the vast expense of his second expedition merely for plunder or to deter the Britons from aiding the Gauls: they gathered from his own book that he had aimed at conquest; and they could see no more than that he had failed. Tacitus came nearest to the truth when he said that 'Julius, though by a successful engagement he struck terror into the inhabitants and gained possession of the coast, must be regarded as having indicated rather than transmitted the acquisition to posterity'.[3] But even this judgement was based upon imperfect knowledge; and the tendency of modern historians, including the greatest scholar of them all, has been to underrate the importance of what Caesar had achieved.

For although Caesar had failed to achieve his aim, he had opened a new world to his countrymen; had proved the facility with which it could be conquered; and had done all

[1] *Phars.*, ii, 572.

[2] See the passages quoted on pp. 329, 348, 350, *supra*, and also Diodorus Siculus, v, 21, § 2; Strabo, iv, 5, § 3; Plutarch, *Caesar*, 23; Appian, *De rebus Gall.*, i, 5, *B. C.*, ii, 17; Dion Cassius, lxii, 4, § 1; Tacitus, *Agricola*, 13; and Suetonius, *Divus Iulius*, 25.

[3] . . . divus Iulius cum exercitu Britanniam ingressus, quamquam prospera pugna terruerit incolas ac litore potitus sit, potest videri ostendisse posteris, non tradidisse (*Agricola*, 13).

54 B.C.—A.D. 43. that opportunity permitted to pave the way for the conquest. He directed the course of British history into a new channel. He forced the most civilized peoples of the island to acknowledge the supremacy of Rome, and made it clear to those of them who could read the signs of the times that the enforcement of that supremacy would not be long delayed. He impressed upon them such respect for the Roman power that the avowed object with which he had invaded the country was effectually gained:—the Britons ceased to abet the resistance of their kinsmen on the other side of the Channel. He showed that the key to the conquest was to take advantage of the jealousy between the family of Cassivellaunus and their rivals. In the presence of these facts, the question whether the tribute which he imposed was ever actually paid is merely academical; but the great scholar who required us to believe that 'it is certain that the stipulated tribute was never paid'[1] made an assertion which is not only improbable but is opposed to such evidence as we possess. Mommsen did not fully appreciate the severity of the punishment which had been inflicted upon Cassivellaunus, or the hold over him which Caesar could exert through his hostages. It is probable indeed that Diodorus,[2] when he said that Caesar forced the Britons to pay tribute, was only putting his own construction upon Caesar's words: but what is certain is that the Britons, although in the reign of Augustus they were not required to pay tribute, were obliged to pay duties at the Gallic harbours upon the goods which they exported to and imported from Gaul; and it is not unreasonable to conjecture that these charges may have been imposed as an equivalent for a tribute which could no longer have been collected except by an irresistible army.[3] But the influence which Caesar exercised upon the destinies of Britain was communicated chiefly through Gaul. In the three years

[1] Th. Mommsen, *Hist. of Rome*, v, 1894, p. 66 (*Röm. Gesch.*, iii, 1889, p. 272). Mommsen is, I think, nearer the truth when he says (*The Provinces of the Roman Empire*, i, 1886, p. 171 [*Röm. Gesch.*, v, 155]) that 'the Britons . . . certainly did not long pay—perhaps never paid at all—the tribute,' &c.

[2] *Bibl. Hist.*, v, 21, § 2.

[3] Strabo, ii, 5, § 8; iv, 5, § 3.

which followed his departure the Britons saw the conquest of Gaul completed: while the civil war ran its course they saw that Gaul made no effort to throw off the Roman yoke; and as time passed and the provinces settled down in the grasp of Augustus, they saw that Gaul was incorporated in the Roman Empire. Meanwhile in Britain the history of Gaul was being enacted over again. In the earlier half of the first century before our era Roman traders, settled in Gallic towns, had prepared the way for the legions of Julius: in the later half Roman or Romanized traders who found it profitable to deal with Britain prepared the way for the legions of Claudius. 54 B.C.—A.D. 43.

Development of British commerce.

In Strabo's time the Britons still imported ornaments of various kinds from the Continent, vases of amber and glass, gold necklets, and ivory for the decoration of horse trappings. Among their exports were slaves, which shows that inter-tribal warfare was still rife, and, if Strabo's statement is to be taken literally, corn, cattle, and iron.[1] Representations of horned cattle, sheep, and pigs are found so often upon British coins[2] that we can easily understand how the graziers should have been able to spare of their abundance; but, although ears of corn are figured on some of Cunobeline's coins,[3] it requires more faith to believe that the population by whose density Caesar was amazed grew enough corn to satisfy not only their own requirements, but those of their continental neighbours, and that the Gauls, whose resources were sufficient to enable them to feed Caesar's army, were obliged to import grain. One would have supposed too that the output of the Gallic iron mines, which Caesar mentions, would not have required to be supplemented from Britain; and that the iron-workers of the Weald had enough to do in supplying the wants of their own countrymen. But, though Britain was not as opulent as Gaul, it would seem that some of the chiefs in the southern and eastern districts amassed a considerable amount of wealth.

[1] . . . *φέρει δὲ σῖτον καὶ βοσκήματα καὶ χρυσὸν καὶ ἄργυρον καὶ σίδηρον· ταῦτα δὴ κομίζεται ἐξ αὐτῆς καὶ δέρματα καὶ ἀνδράποδα*, &c. *Geogr.*, iv, 5, §§ 2–3.

[2] J. Evans, *Coins of the Anc. Britons*, pp. 241, 265–6.

[3] *Ib.*, pp. 297–305, 347, 352–3.

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Tacitus[1] tells us that Prasutagus, who was king of the Iceni about 60 A.D., was renowned for his riches; and, like Dumnorix the Aeduan, he may have acquired them in part from tolls. It has been maintained that the tin trade, which had once been so flourishing, and which certainly flourished during the later period of the Roman occupation, ceased about the beginning of the Christian era, and was suspended for the next two hundred years: but the mere absence of ingots of tin bearing the Roman stamp is hardly sufficient to establish a theory which, intrinsically, is so improbable; and it seems more reasonable to conclude that the mines were continuously worked, but not until the third century under Roman control.[2]

The British inscribed coinage and its historical value.

But the notices of Britain which appear in the writings of Strabo and Diodorus are the least important sources of our knowledge: More valuable is the systematic classification of British coins which has been accomplished during the last fifty years. They show how thoroughly Roman ideas had permeated British civilization before the legions returned to the island, and enable us to trace in outline the course of British political history during the century that elapsed between the departure of Julius and the invasion of Claudius Caesar. Soon after the former event the numismatic art of Britain entered upon its second period. Coins of silver, copper, bronze, and tin were now coming into use;[3] and the need that was beginning to be felt for small change testifies to an advance in material civilization. On the site of Verulamium have been found gold coins of two values, silver of one, and bronze of three.[4] Perhaps we must also regard as a sign of progress increased ingenuity in fraud: at all events besides the authorized mints there were forgers, who made a living by passing coins of base metal thinly plated with gold.[5] Uninscribed coins were still struck, especially in the

[1] *Ann.*, xiv, 31. [2] See p. 510, *infra*.

[3] J. Evans, *Coins of the Anc. Britons*, pp. 99, 116, 126.

[4] *Journ. Brit. Archaeol. Association*, xxvi, 1870, p. 198; *Archaeologia*, liii, 1892, pp. 247–8. Silver coins of two values have been found in Sussex (J. Evans, *Coins of the Anc. Britons*, pp. 109–10).

[5] *Ib.*, p. 44.

remoter districts,[1] and remained in circulation in the time of Claudius ;[2] but from about 30 B.C. the greater number of new coins bore the name either of the prince or of the tribe in whose territories they were minted, and in some cases also the name of the town in which the mint was situated. This evidence shows that Verulamium and Camulodunum were the chief political centres of Southern Britain ;[3] and it is remarkable that the name of Londinium, although it may even then have been the chief commercial town, as it certainly was from the very beginning of the Roman occupation,[4] does not appear upon any British coin which has yet come to light.[5] The earliest of the inscribed coins naturally belonged to the south-eastern parts of the country :[6] the northern tribe of the Brigantes were the last to adopt them ;[7] and not a single specimen has been discovered which can be assigned to the Durotriges.[8] Of the course of events in the northern and western regions history tells us nothing, and coins but little : indeed there is no evidence that the tribes of Scotland, Wales, Northumberland, Durham, Cumberland, Westmorland, Shropshire, Devonshire, and Cornwall even now had coins at all ;[9] and it was not until some time after Caesar's departure that the inhabitants of Gloucestershire, northern Wiltshire, and Somersetshire began to use them.[10] Probably the iron bars which have been already described were still current[11] in the midlands and the west; and Solinus affirmed that in his time the people of the Scilly Islands refused money and traded by barter.[12] Coins bearing the simple inscription,

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[1] *Ib.*, pp. 106, 131, 148, 361, 379–80 ; *Numism. Chron.*, 3rd ser., xvi, 1896, pp. 183–4.

[2] A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 240–1.

[3] J. Evans, *Coins of the Anc. Britons*, pp. 246–7, 291.

[4] See *Vict. Hist. of . . . Northampton*, i, 161.

[5] J. Evans, *Coins of the Anc. Britons*, p. 215.

[6] *Ib.*, p. 38. [7] *Ib.*, p. 406. [8] *Ib.*, p. 131.

[9] *Ib.*, pp. 40, 129; Suppl., pp. 433, 549, map facing p. viii, and indices of both vols.

[10] *Ib.*, p. 41. [11] See pp. 250–1, *supra*.

[12] 22, 7 (9). Sir J. Evans (*Coins*, &c., p. 36) says that Solinus made the statement quoted in the text about the Silures of South Wales ; but he speaks of the inhabitants of the island of Silura, which, he says, is separated from the country of the Dumnonii (Cornwall) by a stormy strait (*Siluram insulam ab ora quam gens Britanna Dumnonii tenent turbidum fretum distinguit*).

54 B.C.—A.D. 43. CATTI, which has been assumed to be that of a tribe, have been found in Worcestershire, Monmouthshire, Somersetshire, Devonshire, and Cornwall;[1] and it has been hastily concluded that some of the remoter British tribes, like many of those of Gaul, had expelled their kings.[2] But our most experienced numismatist thinks that the inscription represents the name of the prince by whom the coins were minted;[3] and one would be inclined to believe that the more backward north and west were then, as they were sixteen centuries later, the strongholds of conservatism.[4] The evidence

Mommsen, in his edition of Solinus (p. 113), remarks that as there is no island called Silura, we must either accept the reading of the 'interpolated' MSS., *insulae Sillinae* (Scilly islands), or assume that Solinus based his statement upon a careless perusal of the passage in which Pliny (*Nat. Hist.*, iv, 16 [30], § 103) mentions the Silures (*super eam* [Britanniam] *haec* [Hibernia] *sita abest brevissimo transitu a Silurum gente xxx*). Considering what Solinus says about the Dumnonii, I would adopt the former alternative.

[1] J. Evans, *Coins of the Anc. Britons*, pp. 39, 140–1; Suppl., p. 488 and map facing p. viii.

[2] J. Rhys, *Celtic Britain*, 3rd ed., 1904, pp. 29, 62. Professor Rhys (*ib.*, pp. 40–1) remarks further that on coins of the Parisi 'one Volisios styles himself sometimes Domnocoveros and sometimes Domnoveros, which may possibly have meant the guardian of the state, or the man of the people. At any rate . . . the same term occurs on a coin of Dumnorix, the Æduan, whose great popularity with the common people Caesar dwells upon more than once . . . On another of these northern coins the person who issued it gives himself a title, which, if correctly read Senotigirnios, would literally mean the old lord or monarch, whatever the exact official signification of that may have been among the Parisi. Unfortunately, the relation of these two kinds of coins to one another in point of time is not known; should they turn out to be of the same date, they might be taken to prove the state to have been divided into two parties, the one clinging to the representative of a dynasty, and the other rallying round one who gave himself out as the friend of the people.'

The professor's 'proof', resting as it does upon possibilities and uncertainties, is hardly conclusive. Moreover the reading DOMNOVEROS does not occur at all, and must be replaced by DVMNOVELLAVNOS (J. Evans, *Coins of the Anc. Britons*, Suppl., p. 591)—the same name as that of the prince who sought the aid of Augustus (p. 363, *infra*); while the reading SENOTIGIRNIOS is hopelessly uncertain (J. Evans, *Coins*, &c., pp. 405, 410–1). The term which occurs on the coins of Dumnorix (assuming that Dumnorix and Dubnorex are identical) is DVBNOCOV (E. Muret and M. A. Chabouillet, *Cat. des monnaies gaul. de la Bibl. Nat.*, 1889, Nos. 5026–48).

It is impossible to decide whether the coins to which Professor Rhys alludes belong to the Parisi or to the Brigantes (J. Evans, *Coins*, &c., Suppl., p. 589).

[3] J. Evans, *Coins of the Anc. Britons*, p. 141; Suppl., p. 488.

[4] According to Xiphilinus, the continuator and epitomator of Dion Cassius

which relates to Southern Britain is less flimsy; and it points to the conclusion that the course of events in that part of the country was leading inevitably to the Roman conquest. 54 B.C.—A.D. 43.

The history of Southern Britain in this period, if we disregard Dorsetshire, Devonshire, and Cornwall, is the history of two dynasties,—that of Caesar's old antagonist, Cassivellaunus, and that of his old ally, Commius. Of the later life of Cassivellaunus nothing is known; but it would seem probable that the recollection of the punishment which the legions had inflicted upon him and the knowledge that his hostages were in Caesar's power were sufficient to induce him to obey Caesar's last injunction and to leave the Trinovantes and their king, Mandubracius, unmolested. About 30 B.C., or perhaps a few years earlier, he was succeeded by his son, Tasciovanus.[1] The earlier coins of this prince were purely British in character; but those of later date are adorned with the figures of Pegasus and centaurs, while one of them is imitated from a coin of Augustus, which was first struck in 13 B.C.;[2] and their number and variety are so great that the reign of Tasciovanus must have extended over a long period,—not improbably until about A.D. 5.[3] His dominions, which were perhaps originally confined to the country of the Catuvellauni, in whose capital, Verulamium, most of his coins were struck, ultimately included, it should seem, not only those of the Trinovantes, but also of the Segontiaci and parts of Northamptonshire.[4] He left several sons, among whom were Epaticcus and Cunobeline. The coins of the former, which bear the abbreviated Latin inscription TASC. FIL.—'son of Tasciovanus'—have all been found

The dynasties of Cassivellaunus and Commius.

Tasciovanus.

Epaticcus and Cunobeline.

(lxxvi, 12, § 2), the Caledonians had democratic government (δημοκρατοῦνται δὲ ὡς πλήθει): but Dion wrote in the third century; and he also says (*ib.*, § 1) that they did not till the soil, which, considering that bronze sickles have been found in Perthshire, Aberdeenshire, and Sutherlandshire (J. Evans, *Anc. Bronze Implements*, pp. 199–200), and that ancient Scottish querns are numerous (*Anc. Stone Implements*, 1897, p. 259), is hardly credible.

[1] J. Evans, *Coins of the Anc. Britons*, pp. 223–4.

[2] *Ib.*, pp. 223–6, 239–40

[3] *Ib.*, pp. 222–3.

[4] *Ib.*, pp. 200, 216, 226, 238–9, 274; *Vict. Hist. of . . . Northampton*, i, 154.

54 B.C.—A.D. 43.

either in the western part of Surrey or the east of Wiltshire; and it has been inferred that he either succeeded to the western portion of his father's dominions or conquered territory which had never been subject to him.[1] Epaticcus was, however, completely overshadowed by his brother, who, under the name of Cymbeline, has been immortalized by Shakespeare. There may perhaps be a kernel of truth in the statement of Geoffrey of Monmouth, that he was educated by Augustus:[2] at all events his silver and copper coins bear witness to the growing influence of Roman culture; and many of them must have been designed either by Romans or by artists who had received Roman training. One of his silver coins, in the opinion of the highest authority, is characterized by exquisite workmanship, 'worthy of a Greek artist;'[3] and some of them suggest that not long after the commencement of the Christian era the worship of Hercules had been introduced into Britain.[4] Not one of the coins bearing his name which have so far been discovered was struck at the mint of Verulamium, from which, as we have seen, those of his father had mainly issued: the name of Camulodunum appears upon them all; and the conclusion seems warranted that he inherited the eastern part of his father's dominions, and extended them by subduing the Trinovantes,—the hereditary enemies of his family.[5] It is not improbable that he had begun to reign about 5 B.C., while his father was still alive; and that he conquered the Trinovantes before his father died.[6] The area which was under his immediate rule when he was at the height of his power included perhaps, besides their country and that of the Catuvellauni, a part of that of the Dobuni, who inhabited what is now Gloucestershire;[7] but it would seem

Cunobeline's coins prove growth of Roman influence in Britain.

His conquests.

[1] J. Evans, *Coins of the Anc. Britons*, pp. 200, 226, 279–80.

[2] *Hist. Brit.*, iv, 11. Cf. J. Evans, *Coins*, &c., pp. 288–9.

[3] *Ib.*, pp. 289–90; Suppl., pp. 479, 565.

[4] J. Evans, *Coins*, &c., p. 283. See p. 369, n. 3, *infra*.

[5] *Ib.*, pp. 200–2, 226, 287, 291.

[6] *Ib.*, pp. 226, 287–9.

[7] *Ib.*, pp. 137–8. Sir John Evans is mistaken in identifying the Dobuni with the Boduni, whom Aulus Plautius subdued (Dion Cassius, lx, 20, § 2), and who were certainly a south-eastern tribe. See Th. Mommsen, *Provinces*, i, 175, n. 1 (*Röm. Gesch.*, v, 1885, p. 160, n. 1) and F. Haverfield (R. L. Poole's *Hist. Atlas of Mod. Europe*, xv,—'Roman Britain').

that he also exercised a general supremacy over the whole of the south-eastern part of the island.[1] Suetonius was so impressed by the fame of his power that he described him as *Britannorum rex*,—'King of the Britons.'[2] 54 B.C.—A.D. 43.

Cunobeline's conquest of the Trinovantes appears to have been one of the causes which led to the flight, briefly chronicled by Augustus on the monument of Ancyra,[3] of two British princes who sought for Roman aid. Their names, as recorded on the stone, were DVMNOBELLAV[*nus*], and, if we are to accept the testimony of Chishull,[4] an antiquary of a past generation, TIM . . . The name of the former, as it is spelled on British coins, was Dubnovellaunos. Those of his coins which appear to have been circulated earliest have been found only in Kent, which he probably at one time ruled.[5] His later coins tend to show that he afterwards annexed the territory of the Trinovantes, from which he was in his turn expelled by Cunobeline.[6] But who was the prince who with him undertook the long journey to Rome? The letters TIM, if indeed M was really graven upon the monument, were of course only the first three of another name; and it is possible that Chishull may have mistaken one or perhaps two broken letters for M, or, since M and N were often confused, that the engraver may have been misled by his copy.[7] Be this as it may, there is only one known

Flight of Dubnovellaunus and Tincommius (?), the son of Commius, to Rome.

[1] J. Evans, *Coins of the Anc. Britons*, p. 287.

[2] *C. Caligula*, 44.

[3] *Res gestae divi Augusti* ed. Th. Mommsen, 1883.

[4] *Ib.*, p. 139.

[5] J. Evans, *Coins*, &c., pp. 200-2; Suppl., p. 527. According to Mommsen (*Provinces*, i, 171 [*Röm. Gesch.*, v, 156]), Dubnovellaunus was 'probably the successor of the prince of the Trinovantes confirmed by Caesar'. The only ground for this conjecture is that Dubnovellaunus appears to have had temporary dominion over the country of the Trinovantes, which had previously been annexed by Tasciovanus. But the fragmentary numismatic evidence which is all that we have to go upon seems to show that Dubnovellaunus was originally King of Kent.

[6] Gold coins have been found, struck by a king named Addedomaros, which appear to show that he began to reign earlier than Cunobeline, and that his dominions were in the eastern counties, their centre being Essex; but there is no evidence for defining his relations with Cunobeline or Dubnovellaunus (*Numism. Chron.*, 4th ser., ii, 1902, pp. 12, 16).

[7] 'primas tres' [litteras], says Mommsen, 'in Latino exemplo TIM fuisse

54 B.C.—A.D. 43.

name with which TIM . . . can be identified,—that of Tincommius, who called himself on some of his coins TINCOM[*mios*] COMMI FILI[*us*] REX[1]—'King Tincommius, son of Commius.' In order to understand the history of Tincommius, we must trace the later career of the Commius who was, beyond all reasonable doubt, his father,[2]—the king of the Atrebates who had accompanied Caesar to Britain.

The later adventures of Commius.

Commius had of course been liberally rewarded for his services: but in the great Gallic insurrection of 52 B.C. he had thrown in his lot with Vercingetorix; and he was one of the four generals to whose joint direction was entrusted the command of the Pan-Gallic host which marched to relieve the latter when he was beleaguered in Alesia. 'Caesar,' we read in the seventh *Commentary*, 'had found Commius a loyal and serviceable agent in former years in Britain; and, in acknowledgement of these services, he had granted his tribe immunity from forced contributions, restored to it its rights and laws, and placed the Morini under his authority. Yet so intense was the unanimous determination of the entire Gallic people to vindicate their liberty and recover their ancient military renown, that no favours, no recollection of former friendship, had any influence with them, but all devoted their energies and resources to the prosecution of the war.'[3] Patriotism, however, was not the only motive

Chishullius auctor est, qui unus eas servavit, in Graecis non superest nisi prima T. Comparavit Evansius (l. c. p. 159) nummos inscriptos *Tinc . . . Commi f(ilius)* repertos praesertim in regione Sussex, potestque fortasse defendi in lapide Ancyrano superfuisse TIN et postremam litteram fractam errore pro M acceptam esse.'

[1] J. Evans, *Coins of the Anc. Britons*, Suppl., pp. 499, 515. In his earlier volume (pp. 159–60) Sir John Evans remarks that 'possibly it is his [Tincommius's] name which is preserved in the form of TIM, in company with that of Dubnovellaunus, in the inscription at Ancyra . . . I should, however,' he adds, 'regard Tinc[ommius] as belonging to a rather earlier period than Dubnovellaunus, though both must have been contemporaries of Augustus,' &c.

[2] *Ib.*, pp. 153–4.

[3] Huius opera Commii, ut antea demonstravimus, fideli atque utili superioribus annis erat usus in Britannia Caesar; pro quibus meritis civitatem eius immunem esse iusserat, iura legesque reddiderat atque ipsi Morinos attribuerat. Tanta tamen universae Galliae consensio fuit libertatis vindicandae et pristinae belli laudis recuperandae ut neque beneficiis neque amicitiae memoria moverentur omnesque et animo et opibus in id bellum incumberent. *B. G.*, vii, 76, §§ 1–2.

of Commius : he had a reason for the bitterness of his hostility, which Caesar does not mention, but which we learn from Caesar's friend, Aulus Hirtius, who wrote the last of the *Commentaries on the Gallic War*. In the winter of 53–52 B.C., while Caesar was absent in Cisalpine Gaul, Commius took an active part in forming the nucleus of the coalition of which Vercingetorix was destined to be the leader; and Labienus, who found out his designs, commissioned the tribune Volusenus to assassinate him. Commius escaped with a severe wound; and in the year which followed the overthrow of Vercingetorix he formed, in conjunction with a chief of the Bellovaci, a fresh coalition against Caesar, who was obliged to exert all his strength in order to subdue it. For some time Commius led the life of a brigand chief, and succeeded in capturing several convoys which were on their way to Caesar's winter camp in the country of the Atrebates. He made himself so formidable that Mark Antony sent Volusenus to make a second attempt to kill him; and although he again escaped, he ultimately surrendered on the express condition that he should never again be brought face to face with any Roman. 54 B.C.–A.D. 43.

When and why Commius took up his abode in Britain is not known; but some probability may be claimed for the conjecture that his motive was to check the encroachments of the Catuvellauni.[1] No coins have been found which can with absolute certainty be ascribed to him:[2] but it is admitted that he issued coins before Tasciovanus, who, as we have seen, began to reign at least as early as 30 B.C.;[3] and before his death he became overlord of the maritime tribes of South-Eastern Britain on the right bank of the Thames.[4] He left three sons, Tincommius, Verica, and Eppillus; and almost all their coins have been found in Kent, Sussex, Surrey, and Hampshire.[5] Each of these sons described himself on his coins as REX, and each of them appears to have had a kingdom of

His conquests in Britain.

Tincommius, Verica, and Eppillus.

[1] J. Rhys, *Celtic Britain*, 1904, pp. 30–1.

[2] J. Evans, *Coins*, &c., p. 156. Cf. pp. 83, 157–8; Suppl., p. 499; and *Journ. Brit. Archaeol. Association*, xxvi, 1870, p. 196.

[3] J. Evans, *Coins*, &c., pp. 154–5.

[4] *Ib.*, pp. 153, 155–6.

[5] *Ib.*, p. 155.

54 B.C.—A.D. 43. his own, Tincommius ruling the Regni, who inhabited Sussex, Eppillus the Cantii, and Verica the Atrebates.[1] The dominions of Verica cannot, however, be certainly defined. There is some reason to suppose that he held sway over the Atrebates of Belgium as well as over those of Britain; for certain coins found in the north of France, and closely resembling others that are common in the south-eastern counties of England, are inscribed with a monogram which appears to denote the abbreviation VE.[2] It should seem that Eppillus, at some time, was king of the Atrebates, for some of his coins have the legend CALLEV,—an abbreviated form of Calleva, the chief town of that tribe.[3] Certain coins, however, exist which apparently bear the names of all the three brothers, a fact which can only be explained on the theory that at one time they exercised a joint sovereignty over the dominions which had belonged to their father;[4] while others are inscribed with the names of Verica and Eppillus only.[5] It has been assumed that these coins were not struck until after the death of Tincommius;[6] but another explanation seems possible. Why did Tincommius, alone of the three brothers, solicit the protection of Augustus, and why did he undertake the journey to Rome in conjunction with Dubnovellaunus? Numismatic evidence has led to the belief that Dubnovellaunus had once ruled over the Cantii;[7] and if so, Eppillus, who afterwards acquired dominion over the same tribe, probably dispossessed him. Dubnovellaunus, as we have already seen, appears to have once ruled over the Trinovantes as well, and to have been expelled from their country by Cunobeline. These successive reverses may have been the motive for the journey which he undertook to Rome; and when we consider that certain coins bear the names of Eppillus and Verica, without that of Tincommius, which on others appears side by side with theirs, it seems possible

[1] J. Evans, *Coins of the Anc. Britons*, pp. 155, 158, 171; Suppl., p. 521.

[2] J. Evans, *Coins*, &c., pp. 170–2; Suppl., pp. 508–9.

[3] *Ib.*, pp. 523–4. Is it permissible to suppose that Verica may have ruled the Gallic, and Eppillus the British Atrebates?

[4] See J. Evans, *Coins*, &c., pp. 159, 172.

[5] *Ib.*, p. 172.

[6] *Ib.*, pp. 172–3, 183.

[7] See p. 363, *supra*.

that Tincommius, finding that his brothers were leagued together against him, threw in his lot with another prince who had been as unfortunate as himself. This conjecture is perhaps somewhat strengthened by the fact that one of the coins of Tincommius bears, along with TIN—the abbreviated form of his name—the inscription DV,[1] which has baffled the acumen of numismatists, but which, on the analogy of TC—one of the abbreviations of TINCOMMIOS[2]—may possibly stand for DUBNOVELLAUNOS.[3] 54 B.C.—A.D. 43.

How the fugitives were received we are not told; but it is certain that Augustus did not grant them armed assistance; nor is there any evidence that they ever recovered power. As early as 34 B.C. Augustus had marched into Gaul with the intention, as was generally believed, of invading Britain; but, owing to an insurrection in Dalmatia, he was compelled to abandon his resolve.[4] For several years, however, it was expected that he would sooner or later complete the work which his adoptive father had begun; and this expectation was voiced in the poetry of the time. About the year 30 B.C. Vergil[5] prayed that 'far off Thule' might obey Augustus; and Horace, in odes which seem to have been officially inspired, called upon Fortune to preserve him in his expedition against the Britons, 'remotest inhabitants of the world,'[6] and foretold that when they and the Parthians were brought under the imperial sway he would be hailed a god upon earth.[7] In 27 and again in 26 B.C. Augustus marched into Gaul with the ostensible purpose of invading Britain, but again without result.[8] But the latest of these dates was earlier than the flight of Tincommius and Dubnovellaunus; and thenceforward Augustus abandoned all thought of invading Britain.[9] The cause of his inaction is discernible *Why he* Augustus contemplates an invasion of Britain.

[1] J. Evans, *Coins*, &c., p. 161 and pl. I, no. 12.

[2] *Ib.*, p. 194 and pl. III, no. 14.

[3] As far as I know, I am alone responsible for the conjecture which I have made in the text.

[4] Dion Cassius, xlix, 38, § 2.

[5] *Georg.*, i, 30.

[6] *Carm.*, i, 35, 29.

[7] *Ib.*, iii, 5, 2–4.

[8] Dion Cassius, liii, 22, § 5; 25, § 2.

[9] Professor Rhys (*Celtic Britain*, 1904, p. 32), apparently forgetting the dates (which I have given in the text) of Augustus's preparations for invading

54 B.C.—A.D. 43. abandoned his intention.

in two passages of Strabo's *Geography*,[1] which give the official explanation of the imperial policy. The conquest of Britain would be very costly; and it was unlikely that the revenue would be more than sufficient to defray the expense of the garrison and the administration: the duties levied at the Gallic harbours on goods imported from and exported to Britain were more productive than any tribute; besides, Britain was too weak to be dangerous, and its conquest was therefore unnecessary. Possibly we may gather from the prominence which is given in the monument of Ancyra to the petition of Tincommius and Dubnovellaunus that it was officially interpreted as a sign of the virtual submission of the Britons.

Continued growth of Roman influence in Britain.

This confidence indeed is not difficult to understand. The conjecture that at the courts of Commius, of Tasciovanus, and of Cunobeline Latin was the official speech[2] may perhaps be somewhat rash: but at all events Latin was the language of the mint; and perhaps it is not unreasonable to suppose that, as some Pannonian Celts were versed in Latin literature,[3] a Briton here and there was equally accomplished. Roman silver coins were already eagerly accepted, on account of their purity, in Southern Britain.[4] And if Rufina, the young British wife of a Roman, whose praises Martial sang,[5] could hold her own in Italian society, we may realize that before the Roman conquest Britain had begun to be Romanized.

Cessation of British coinage in certain districts which had belonged to the sons of Commius.

With the sons of Commius the British coinage in the districts which they had ruled, with the sole exception of Kent, came to an end.[6] It may be that the inhabitants had begun, like the Gauls with whom they traded, to use only Roman money; but, as the coinage of Kent continued, the more probable explanation would seem to be that they were

Britain, suggests that 'it may be that it was the representations of the [fugitive British princes] . . . that led him thereto.'

[1] ii, 5, § 8; iv, 5, § 3.

[2] *Archaeologia*, lii, 1890, p. 384.

[3] Velleius Paterculus, ii, 110, § 5.

[4] *Archaeologia*, liv, 1895, pp. 489–94.

[5] xi, 53.

[6] J. Evans, *Coins*, &c., pp. 151, 156.

no longer able to make head against the King of the Catuvellauni.[1] 54 B.C.—A.D. 43.

Tincommius and Dubnovellaunus were not the only British princes who paid their respects to the emperor. 'In our time,' says Strabo, 'various British chieftains gained the friendship of Augustus Caesar by sending embassies and performing services; placed votive offerings in the Capitol; and made almost the whole island familiar to the Romans.'[2] Among them, we can hardly doubt, was Cunobeline, whose coins, like those of his father, testify that Roman mythology had already taken root on British soil,[3] and who, according to Geoffrey of Monmouth,[4] voluntarily paid tribute to Rome. If there is any truth in Geoffrey's statement, the tribute must have been the price paid for moral support. During the reign of Tiberius, who adhered to the conservative and moderate policy of his stepfather, the relations of Cunobeline and of Britain with Rome apparently remained unchanged: history only relates that some soldiers of Germanicus, who had been shipwrecked on the British coast, were sent back by British princes.[5] It can hardly be doubted, however, that the conquest of Britain was contemplated by Roman statesmen as inevitable: to leave independent the Celtic island which was so near the conquered Celtic mainland was unnatural, and could not be permanently safe.[6] The latter part of Cunobeline's reign was clouded by domestic quarrels; and in A.D. 40, when he was an old man, his son Adminius,[7] whom he had driven into exile, threw himself on

Relations of Cunobeline with Rome.

His exiled son, Adminius, takes refuge with Caligula.

[1] See J. Rhys, *Celtic Britain*, 1904, pp. 25–6.

[2] *νυνὶ μέντοι τῶν δυναστῶν τινες τῶν αὐτόθι πρεσβεύσεσι καὶ θεραπείαις κατασκευσάμενοι τὴν πρὸς Καίσαρα τὸν Σεβαστὸν φιλίαν, ἀναθήματά τε ἀνέθηκαν ἐν τῷ Καπετωλίῳ, καὶ οἰκείαν σχεδόν τι παρεσκεύασαν τοῖς Ῥωμαίοις ὅλην τὴν νῆσον* (*Geogr.*, iv, 5, § 3).

[3] J. Evans, *Coins*, &c., pp. 226, 289–90. Sir John observes that Jupiter Ammon, Hercules, Apollo, Diana, Cybele, and other deities are figured on silver and copper coins of Cunobeline, which proves 'how completely Roman mythology had taken root . . . unless we are to suppose that the types were . . . left to the mere fancies of the engravers', who either were Roman or had been trained in Roman workshops.

[4] *Hist. Brit.*, iv, 11.

[5] Tacitus, *Ann.*, ii, 24.

[6] See Mommsen's *Provinces*, i, 173–4 (*Röm. Gesch.*, v, 157–8).

[7] Sir J. Evans (*Coins*, &c., pp. 208–9) gives good reasons for not identifying

54 B.C.—A.D. 43. the mercy of Caligula, who was at the time in Gaul, and offered to surrender his father's kingdom. The feather-pated emperor sent messengers to Rome, who were charged to announce to the Senate in the temple of Mars the submission of the whole island;[1] but the magniloquent and mendacious message testifies not only to his vanity but to the fame of Cunobeline.

Death of Cunobeline. Within the next three years the great king died, leaving, besides Adminius, three other sons who still remained in Britain,—Caratacus, Togodumnus, and, as we may conjecture, one Bericus, who fled over sea. Caratacus, whose name is more familiar under the erroneous form Caractacus, was the prince who in later years opposed a desperate resistance to the Roman conquest of Western Britain. After Cunobeline's death he and Togodumnus assumed royal power, and perhaps combined to exclude Bericus from any share in the inheritance of their father's dominions.[2] It is possible that Bericus had some influence with the Iceni, who were bitterly hostile to the dynasty of Cassivellaunus and his successors, and were prepared to join the Romans if they should invade the island. But another explanation has been proposed. There are late coins of the Iceni which bear the name of a prince named Antedrigus, who later still issued coins which have been found in the territory of the Dobuni. It has been suggested that, like the Treveran Indutiomarus and his enemy Cingetorix,[3] Antedrigus and Bericus were the leaders of rival factions of the Iceni; that Antedrigus prevailed; that Bericus thereupon determined to seek Roman aid; and that Antedrigus, when the Iceni joined the Romans, sought an asylum among the Dobuni.[4] Anyhow Bericus

Unpopularity of his dynasty intensified on the accession of his sons, Caratacus and Togodumnus.

Adminius with the Amminus whose name appears on coins. Cf. J. Rhys, *Celtic Britain*, 1904, pp. 34, 280.

[1] Suetonius, *C. Caligula*, 44. Cf. J. Evans, *Coins*, &c., p. 285.

[2] *Ib.*, pp. 286–7. See also Tacitus, *Ann.*, xii, 35.

[3] See p. 330, *supra*.

[4] This is the theory of Professor Rhys (*Celtic Britain*, 1904, pp. 36–8) and apparently also of Sir John Evans (*Coins of the Anc. Britons*,—Suppl., pp. 489–93, with which cf. p. 584 and pp. 358, 366–7, 381–5, 387–9 of the earlier volume); but it will not bear examination. Bericus was one of the fugitives whose retention at Rome was resented by the two sons of Cunobeline

fled to Rome.[1] It would seem that Caratacus and Togodumnus took offence when he and Adminius were not sent back, and even committed, or threatened to commit, some act of violence against the Roman power;[2] and it may be that their attitude, combined with the information which Bericus gave about the internal politics of his country, was among the motives that induced Claudius to dispatch the force which, under Aulus Plautius, was to begin the Roman conquest of Britain.[3] 54 B.C.—A.D. 43. Invasion of Britain by Aulus Plautius.

Amid many uncertainties the facts of British history which stand out prominently are these. The invasions of Caesar, supported by his conquest of Gaul, stimulated trade between the Britons and the Romanized Gauls, and thereby brought Britain within the sphere of Roman influence; encouraged those British princes who needed protection or support to turn to Rome, and made them all look up to the Emperor as a patron, who might eventually be their sovereign lord. In the island itself Commius and his sons made themselves supreme in the eastern districts south of the Thames; Review of British history from 54 B.C. to A.D. 43.

who remained in Britain. It is admitted, or rather maintained, by Professor Rhys that the Iceni were hostile to the dynasty of Cunobeline. It would seem therefore that if, as the professor suggests, Antedrigus was forced to flee from the Iceni when they joined the Romans, he belonged to a party among the Iceni which was not opposed to the sons of Cunobeline and was perhaps even in sympathy with them. But if he had prevailed over Bericus and forced him to flee, his party was evidently the stronger. Why then should he have been forced to quit the Icenian territory? Are we to assume that the anti-Catuvellaunian party among the Iceni, to which Bericus *ex hypothesi* belonged, was originally the weaker, but on the return of Bericus suddenly became the stronger? May we not rather suppose that Bericus was one of the sons of Cunobeline and was for some reason at variance with his brothers, Caratacus and Togodumnus; that the Iceni, with whom he was in sympathy, were for the most part or as a whole opposed to them; and that Antedrigus was not the leader of a faction but the king of the Iceni, who, like Gallic kings mentioned by Caesar, was unpopular with his nobles and his subjects generally, and was by them forced to flee?

[1] Dion Cassius, lx, 19, § 1.

[2] Suetonius, *Claudius*, 17.

[3] See *Vict. Hist. of . . . Norfolk*, i, 284–5. Professor Haverfield (R. L. Poole's *Hist. Atlas of Mod. Europe*, xv, —'Roman Britain') thinks that Claudius's pretext, as stated by Dion Cassius—the appeal of Bericus—'may well be the real reason for the undertaking'. Mommsen (*Provinces*, i, 174, n. 1 [*Röm. Gesch.*, v, 1885, p. 158, n. 1]) says 'The war was certainly not waged on account of Bericus (Dio, lx, 19)'.

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but their power was overmatched and perhaps finally absorbed by that of the family of Cassivellaunus, who steadily augmented their dominion by conquest until under Cunobeline it extended from the coast of Essex to the estuary of the Severn, and from the Midlands to the English Channel.

The Roman conquest and its results.

But the jealousy and the fear which this ambitious dynasty aroused led directly to the Roman invasion, by which the influences that had already begun were so developed that the upper classes and the townspeople of Britain learned to speak Latin[1] and to adopt Roman customs, and in the end came, like their Gallic neighbours, to regard themselves as Romans; that the Late Celtic art which had flourished for centuries gave way to that of Rome, and even in cottages and remote hamlets Samian pottery and rude hypocausts were to be found;[2] that by the fourth century a British church had been fully developed, which continued to flourish after the Roman administration had ceased, while even in the sixth century the forsaken Britons gloried in the name of *Romani*;[3] and that, in a word, Britain, becoming completely Romanized, received an impress which has not yet wholly faded away.[4]

Permanence in English history of prehistoric and Celtic elements.

But when the Roman had gone, when the Saxon, the Dane, and the Norman had come, the descendants of neolithic aboriginals, of bronze-using immigrants, and of Celts still lived on; and their composite influence has ever since been helping to form the British character and to determine the course of British history. The roads on which we travel, the flocks and herds that feed us, the corn that grows in our fields, the implements which we use,—all our industrial arts are inseparably connected with theirs. Not only do their beliefs still survive, tinging the faith which their successors have been taught, but their spirit has lived again in the men who have done the deeds of which our nation may be proud.

And perhaps the story which this book has told may

[1] F. J. Haverfield, *The Romanization of Roman Britain*, pp. 9–12.

[2] *Ib.*, pp. 14–5; *Vict. Hist. of . . . Northampton*, i, 159–62.

[3] *Eng. Hist. Rev.*, xi, 1896, pp. 417–30.

[4] See *Vict. Hist. of . . . Hampshire*, i, 268–9, and cf. F. W. Maitland, *Domesday Book and Beyond*, 1897, pp. 222, 327–40.

lead a few to become less self-complacent and to think more of those primitive ancestors. In some things we have sunk below their level: in what have we risen? Riches, luxury, the security that tends to make self-reliance weak, the softening of manners, rapidity of communication, the development of engines of destruction, medicine, and surgery—all that appertains to material civilization—herein we have made giant strides. But such improvements hardly enable men to bear up under burdens which are ever increasing. The tourist in a Pulman car is not happier than those who travelled in stage-coach or wagon, and speed deprives him of as much as it bestows; machinery has but substituted fresh evils for those which it destroyed. New superstitions, less gross but not less false, have been engrafted upon the old: but 'pure religion and undefiled,'—how far has it strengthened its hold upon the hearts of men? We have professed indeed to teach inferior races the gospel of love· but in Australasia our mission has been not so much to evangelize as to exterminate. Apart from the extirpation of the coarser forms of inhumanity and from those other civilizing influences which may operate even in a decadent society, the progress of which we may not unreasonably boast has been in knowledge, which to the vast majority is unattainable, and, in this island, unheeded or contemptuously rejected by most of the few who have it within reach. 54 B.C.—A.D. 43.

THE ETHNOLOGY OF ANCIENT BRITAIN

I. INTRODUCTION

THE ethnology of ancient Britain has been studied from many points of view. Writers of a past generation relied simply upon the notices which are to be found in the works of Caesar, Strabo, Tacitus, and other ancient writers. In the last century the science, if it may now be so called, of physical anthropology came into being. The barrows in which our prehistoric ancestors had buried their dead were opened; and the skeletons which had been left in them by earlier explorers were systematically measured. The physical characters of the living population were noted as far as possible in the hope that they might help to solve the problems of the past. Archaeologists collected the pottery, the tools, the weapons, and the ornaments which were found beneath the soil, in the beds of rivers, in barrows, cairns, caves, earthworks, and elsewhere, described them, classified them, and compared them with those of other countries. Philologists studied the forms of the Celtic dialects, and endeavoured to discover in them traces of dialects older still. Finally, folk-lorists formed an association, and joined the army of inquirers. The united efforts of all these seekers after truth have stored up a huge mass of information; and those who may read this article will, I believe, agree with me that there is no reason to expect that any additional facts which may be ascertained will throw much new light upon the questions which we are about to consider:[1] but no serious attempt has yet been made to co-

[1] A few years ago Professor Macalister (*Journ. Anthr. Inst.*, xxiii, 1894, pp. 407–8) propounded a set of questions which, he suggested, might be answered by the help of an 'ethnographic census': —'Have we', he asked, 'any representatives of the pre-Celtic inhabitants? . . . if so, are such people of a pre-Aryan stock, and are they of the same type as the long-headed people in the long barrows? . . . Are these the Silures? . . . Were the Celtic immigrants homogeneous? . . . What relation subsisted between the Cymric and Gaidhelic-speaking peoples?' &c. It will be apparent to any one who reads this article that most of these questions can be answered without the aid of an 'ethnographic census'; and that, if they could not be answered independently, such aid would be insufficient unless it could be supplemented by new archaeological and linguistic information. The unofficial census which has been carried out by Dr. Beddoe, M. H. Muffang, Sir William Turner, and, perhaps in consequence of Professor Macalister's suggestion, by Dr. C. R. Browne, Messrs. Gray and Tocher, and other anthropologists, is of course incomplete; but it may be doubted whether the evidence which they have collected would be seriously modified by further investigation. When Dr. Collignon undertook a similar informal census in France, he compared in each department the mean cephalic index of the whole number of the heads which he had measured with that of the ten which he had measured first; and in every instance the difference was less than 1 per cent. (Rice Holmes, *Caesar's Conquest of Gaul*, 1899, p. 320).

ordinate the materials which exist. To do this is the aim of the present article. If the problems of British ethnology can be solved, history, physical anthropology, archaeology, and philology must combine.

II. THE METHODS OF ANTHROPOLOGY

A lay reader who takes up a treatise on ethnology ought to understand the methods by which anthropologists differentiate the various human types. I may be allowed to reproduce a paragraph which I wrote a few years ago in another volume, and to which I shall have something to add.

'Anthropologists are obliged to make use of technical terms, more or less uncouth; and they are guided in their observations by very precise and minute rules, framed with the object of eliminating, as far as possible, the chance of error. But it is unnecessary for my purpose to trouble the reader with more than a few of these things. What I shall have to say about stature, complexion, hair and eyes, will need no explanation; and in regard to the skull I shall, as a rule, only have to deal with that measurement which fixes the proportion between its length and its breadth. In this measurement the length is represented by 100; and the proportion which the breadth bears to the length is called the cephalic index. Thus, if the breadth is four-fifths of the length, the index is 80. According to the system formulated by the great French anthropologist, Paul Broca,[1] skulls are grouped, according to the cephalic index, in five classes. Skulls whose index exceeds 83·33 are brachycephalic; those whose index falls between 83·33 and 80 are subbrachycephalic; those between 80 and 77·77 mesaticephalic; those between 77·77 and 75 sub-dolichocephalic; and those below 75 dolichocephalic . . . it is necessary to bear in mind that measurements of living heads invariably yield a higher cephalic index—the average difference being as much as 2—than those of skulls[2] [of the same form]. Another important character of the skull or head is *gnathism*, that is to say, the degree of projection of the upper jaw. The word *orthognathous* denotes that this projection is comparatively slight; for absolute orthognathism does not exist. The remaining technical terms which it is necessary for general readers

[1] *Mém. d'anthr.*, iv, 1883, p. 243.

[2] Professor W. Z. Ripley thinks that the difference is nearer 1·5 than 2 (*L'Anthr.*, vii, 1896, pp. 516–9); while Mr. Gray (*Man*, ii, 1902, No. 41, pp. 50–1) regards the method' of subtracting 2 as 'illogical', and would subtract 8 mm. from the breadth and 10 mm. from the length.

Certain minute differences between Broca's system of measuring the skull, which is followed everywhere except in Germany, and that adopted by the German anthropologist, von Ihering, are lucidly explained by Otto Ammon (*L'Anthr.*, vii, 1896, pp. 676–82) and Professor Ripley (*The Races of Europe*, 1900, p. 593), but may, for the purpose of the present inquiry, be safely disregarded.

to understand are those which describe the structure of the nasal skeleton. *Platyrrhinian* means that it is wide, *mesorrhinian* intermediate, and *leptorrhinian* narrow.'[1] I should have added that the orbital index, which is important, denotes the relation of the breadth of the orbit to its length; and, since we are dealing with the ethnology of Britain, it will be convenient to adopt for cephalic indices the notation which is prevalent in this country, and according to which skulls whose indices exceed 80 are called brachycephalic, those between 80 and 75 mesaticephalic, and those under 75 dolichocephalic.

The value of the cephalic index was for many years taken for granted in all ethnological treatises; and many anthropologists still lay great stress upon it.[2] But there has lately been a reaction.[3] Professor Sergi[4] scoffs at 'the old and discredited method of the cephalic index, which only indicates artificial and conventional distinctions', and tells us that 'it is the forms alone that we have to take into consideration',[5] and that 'indices may serve to approximate the most diverse forms and to separate the most homogeneous'.[6] This last remark is unquestionably true; as Huxley said, 'in nine cases out of ten you may diagnose an Australian skull [among other dolichocephalic skulls] with certainty.'[7] Nevertheless the cephalic index, used with discrimination, retains the value which Broca, Beddoe, Collignon, Turner, and other anthropologists ascribe to it; and those who are familiar with Sergi's writings will not be surprised to learn that, when it suits his purpose, he lays great stress upon the distinction between dolichocephalic and brachycephalic skulls.[8] He considerably modifies his view when he affirms the truism that 'we cannot accept the evidence of the cephalic index when that evidence is contradicted by other important facts';[9] but if any one who has a taste for ethnology will spend a few days in

[1] *Caesar's Conquest of Gaul*, 1899, pp. 249–50.

[2] For instance, Prof. Ripley (*The Races of Europe*, p. 37), Dr. Beddoe (*Journ. Anthr. Inst.*, xxx, 1900, No. 93), Sir W. Turner (*Trans. Roy. Soc. Edinburgh*, xl, part iii, 1903, pp. 547–614), and Prof. Symington (*Report of . . . the Brit. Association*, 1903, p. 796). See also *L'Anthr.*, x, 1899, pp. 105–6, and *Journ. Anthr. Inst.*, xxxiv, 1904, pp. 181–206.

[3] See Prof. C. S. Myers's article (*ib.*, xxxiii, 1903, pp. 36–40) and *Man*, iii, 1903, No. 13, pp. 28–32. I confess that I do not believe that for the present inquiry any valuable result would be attained by revising, on what are called 'biometric' lines, the craniological work which has already been done for ancient Britain. See *Nature*, Aug. 30, 1906, p. 458, and *Biometrika passim*.

[4] *The Mediterranean Race*, 1901, p. 102. See also *L'Anthr.*, x, 1899, pp. 105–6; *Report of . . . the Brit. Association*, 1901, p. 778; *Man*, ii, 1902, No. 41, p. 50; and *Nature*, Aug. 30, 1906, p. 458.

[5] *The Mediterranean Race*, p. 104.

[6] *Ib.*, p. 195.

[7] *Proc. Soc. Ant.*, 2nd ser., iii, 1864–7, p. 284. Cf. G. Rolleston (*Brit. Barrows*, p. 646, n. 1).

[8] For instance, on pages 136, 138, 143, 160–2, 189–92, and 238.

For evidence that 'the mesaticephals', Sergi's opinion notwithstanding, are the result of intermarriage between 'dolichocephals' and 'brachycephals', see *Rev. mensuelle de l'École d'anthr.*, iv, 1894, p. 399; v, 1895, p. 413.

[9] *The Mediterranean Race*, pp. 199–200. Professor Rolleston (*Brit. Barrows*,

walking through the department of Jura or the mountainous parts of Auvergne, the contrast between the round heads which he will see everywhere and the totally different type which he has been accustomed to in his own country will convince him that the cephalic index has been 'discredited' in vain.[1] Anthropologists are, however, becoming convinced that the labour which has been spent upon calculating the averages of tables of widely different indices has borne little fruit.[2]

When we consider the cranial forms, apart from measurements, we find the same lack of unanimity. According to Sergi,[3] 'the *norma verticalis*, or view from above,' is 'the most important of all'. According to Rolleston,[4] 'the *norma lateralis*, or profile view of a skull is the most important.' The present tendency, however, of British anthropologists is to follow the Italian professor.

The evidence of skulls will often mislead unless it is used with caution and discernment, reinforced by collateral knowledge. Certain British brachycephalic skulls of the Bronze Age closely resemble in many respects those of the Maoris.[5] Rolleston, remarking on the likeness between a dolichocephalic skull of the Bronze Age from Weaverthorpe and the famous Engis skull, observes that 'resemblances so strong . . . should, as they are also so widely scattered over the globe, make us careful not to speak as to the ethnological affinities of any skulls, until we have a very considerable number of representatives of both objects of comparison to place alongside of each other; and it may be added until we have also succeeded in bringing other lines of evidence to bear upon the question'.[6]

Besides the various characters of the skull and face, and, when they can be ascertained, the complexion, and the colour of the hair and eyes, ethnologists have to take account of stature, because, although it partly depends upon food and social environment,[7] it unquestionably varies in different races. Now the stature of prehistoric men, when their skeletons are found, can only be estimated

1877, p. 568) so far supports Sergi's view that he regards a skull as brachycephalic, even though its index be less than 80, if it has what he regards as the distinguishing characteristic of brachycephaly, which he proceeds to explain in terms that are too technical for the general reader. Ethnological students will remember the passage.

[1] Even Sergi, as Mr. Myers observes (*Journ. Anthr. Inst.*, xxxiii, 1903, p. 37), 'shows signs of yielding the isolated position which he originally took up as to the utter worthlessness of indices.' He has recently affirmed (*Archiv für Anthr.*, N. F., iii, 1904, p. 120) that the long and the short types of European skulls are specifically different,—that the 'Eurafrican' species is dolichocephalic and the 'Eurasiatic' brachycephalic. See also *L'Anthr.*, xiv, 1903, pp. 587–8.

[2] *Journ. Anthr. Inst.*, xxxiii, 1903, pp. 38, 40.

[3] *The Mediterranean Race*, p. 105.

[4] *Brit. Barrows*, p. 568.

[5] See A. de Quatrefages and E. T. Hamy, *Crania Ethnica*, 1876–82, pl. lv, and *Mem. Anthr. Soc.*, i, 1865, p. 150.

[6] *Brit. Barrows*, p. 621. For some valuable remarks on the permanence of cranial types notwithstanding changes of environment, see Mr. J. L. Myres's paper in *Geogr. Journal*, xxviii, 1906, p. 559, with which cf. pp. 555–6.

[7] See my *Caesar's Conquest of Gaul*, 1899, p. 246, n. 1.

by calculating the relations between the lengths of certain bones and the actual height of the individual; and since these relations are obviously variable, the calculation can only lead to approximately true results. The error would no doubt be insignificant if the average relations were certain; but various anthropologists have adopted various methods of calculation, which have led to widely different results.[1] The most satisfactory, for our purpose,[2] appears to be that of Dr. Beddoe,—'I take away from the length of the femur [or thigh-bone] one-quarter of the excess over 13 inches up to 19, and thereafter only one-eighth, and then multiply by four'.[3]

III. EOLITHIC MAN (?)

Much controversy was excited in the last decade of the nineteenth century by the announcement that stone implements, ruder than the rudest of the Palaeolithic Age, had been discovered on the plateau between the Medway and Caterham valleys: but even if it were possible to convince sceptics that some of these flints were wrought by men's hands, the proof would not affect the present inquiry; for we should have no means of ascertaining to what race

[1] A. H. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 1888, pp. 205–6. See also Dr. Beddoe's remarks in *Journ. Anthr. Inst.*, xvii, 1888, pp. 202–9.

[2] I say 'for our purpose' because of many of the skeletons with which we are here concerned the only relevant measurement that exists is that of the thigh-bone. Dr. Garson (*Journ. Anthr. Inst.*, xxii, 1893, p. 9) thinks that the 'most reliable estimate of stature is obtained from the length of the femur and tibia added together' according to the formula

$$\frac{\text{Femur} + \text{Tibia} \times 100}{49.4}$$

[3] *Ib.*, xvii, 1888, p. 204. Dr. Beddoe's arguments are very strong; and his method appears to me better, on the whole, than that of Dr. Topinard, expressed by the formula $\frac{\text{Femur} \times 100}{27.1}$ (*ib.*, xxii, 1893, p. 9), or than that of M. Rollet (*ib.*, p. 19, note), expressed by the formula $\frac{\text{Femur} \times 100}{27.3}$. So far as I know, the most exhaustive discussion of the question is that of M. L. Manouvrier (*Mém. de la Soc. d'anthr.*, 2e sér., iv, 1892, pp. 347–402), who points out defects in the methods of Dr. Beddoe and MM. Topinard and Rollet; but although he has perhaps shown how greater accuracy can be achieved, the more or less approximate results that have been already obtained are sufficient for our purpose: we should not be in a better position for solving the problems of the ethnology of Ancient Britain even if the Britons whose skeletons have been preserved had been measured in their lifetime, and the measurements recorded.

[Since the foregoing note was written I have read a most interesting paper by Dr. Beddoe (*Journ. Roy. Inst. Cornwall*, xv, 1902, pp. 161–78), which confirms my conviction that his is the best method of measurement, although he confesses (p. 165) that it 'probably errs by excess in the higher statures'. Remarking (p. 163) that prehistoric bones 'have lost much of their original substance, and are probably from 1 to 3 millimetres short of their original length', he says, 'Manouvrier does not seem to have made any provision for this reduction; and I apprehend that his computed statures must on an average be a little too low'. See Addenda.]

(supposing that it differed from that of the earlier palaeolithic hunters) those men belonged.[1]

IV. PALAEOLITHIC MAN

1. The people who inhabited this island in the Old Stone Age appear to have been confined to the south; for no palaeolithic implement has yet been found further north than Lincoln, or, as some maintain, the East Riding of Yorkshire.[2] An attempt has indeed been made to prove that such tools were used in Scotland;[3] but the best judges are unanimously of opinion that the contention has not been established.[4]

Little direct evidence exists as to the physical type of the palaeolithic inhabitants of Britain. Only four human skulls have been found in England which can be referred to that period,—one at Galley Hill, near Swanscombe,[5] one at Westley, near Bury St. Edmunds,[6] and two in the Cattedown cave near Plymouth:[7] but it is not certain that the first was contemporaneous with the beds which contained it:[8] of the second only fragments remained from which it was impossible to determine the contour;[9] and the others could not be removed entire. Almost all the older palaeolithic skulls, however, which have been discovered in Western Europe belong apparently to the same race,[10] which may have been represented among the hunters who entered Britain when it still formed part of the Continent. Indeed the Galley Hill skull, whether it belonged to a palaeolithic man or not, has certain characteristics of the most famous representative of the race,—the Neanderthal skull, which was discovered about the middle of the last century in the valley of the Neander in Rhenish Prussia.[11] The skulls of this type are extraordinarily dolichocephalic; and the people to whom they belonged had extremely low and retreating foreheads, heavy and projecting lower jaws, and amazingly prominent brow ridges, and were short, big-boned, and muscular.[12]

[1] See pp. 25–30, *supra*.

[2] See p. 35, *supra*.

[3] *Proc. Philosoph. Soc. Glasgow*, xxx, 1899, pp. 30–8.

[4] *Proc. Soc. Ant. Scot.*, xxxvi, 1902, p. 42.

[5] *Archaeol. Journal*, liii, 1896, pp. 217, 221. See also *Quart. Journ. Geol. Soc.*, li, 1895, pp. 505–27, and especially 516 and 526.

[6] *Journ. Anthr. Inst.*, xiv, 1885, pp. 51–5. Cf. Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 542, 656, 703.

[7] See p. 33, *supra*.

[8] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 607.

[9] *Journ. Anthr. Inst.*, xiv, 1885, p. 51, pl. iv–vi; *Nature*, Nov. 15, 1894, p. 68. A skeleton of palaeolithic age was found two years ago near Luton in Bedfordshire, but has not been preserved (*Man*, vi, 1906, No. 6, pp. 10–1).

[10] *Nature*, Nov. 22, 1894, pp. 90–1; Ph. Salmon, *L'Age de la Pierre*, 1889, p. 62; J. Deniker, *The Races of Man*, 1900, pp. 310–2.

[11] The reader will remember that the age of the Neanderthal skull is uncertain. See p. 34, *supra*. The *norma verticalis* of the Galley Hill skull is different from that of 'the continental forms' (*Quart. Journ. Geol. Soc.*, li, 1895, p. 526); and the profile is not brutal.

[12] *Nature*, Nov. 22, 1894, p. 90.

But what if the Neanderthal skull was not human? If that poor creature had but known how famous he, or it, was to become! His broken cranium has a bibliography of its own. Virchow, who, however, late in life changed his mind, at one time regarded it as abnormal,—pathological. Huxley and Broca vigorously defended its respectability; and at the end of the nineteenth century the most eminent anthropologists of Europe and America accepted it as the type of the most ancient of the known races of men. But in 1901 a German anthropologist, Dr. G. Schwalbe, wrote an article of appalling length,[1] which disturbed settled convictions. Huxley had pronounced the Neanderthal to be the most ape-like of all known human skulls: Schwalbe refused to regard it as human, in the accepted sense, at all. For him it represents a distinct species, intermediate between the *Pithecanthropus* of Java—the famous 'missing link', whose remains were discovered a few years ago by Dr. Dubois—and man himself. In the same class Schwalbe places the skulls of Spy, which have always been grouped along with that of Neanderthal; and he insists that all the human palaeolithic skulls of Europe, however closely they may appear to resemble these, are in reality different.[2] 'In the Neanderthal skull,' says Dr. Laloy, in a lucid summary of Schwalbe's article, which will satisfy all who are not specialists, 'the greatest length coincides with the "inio-glabellar" diameter,' that is to say, the diameter measured from the space between the supraciliary, or brow, ridges and the sinus at the back of the neck: this, he adds, is never the case in man. No, not in man as we know him. But what sense are we to attach to the word 'human'? Was there ever a creature of whom it could be affirmed that he was the first man? [3]

Ten or twelve skulls, which, in dolichocephaly and prominence of the supraciliary ridges, resemble those of the Neanderthal type, but, unlike them, have high foreheads, and are said to have belonged to tall men, have lately been found associated with tools of Mousterian form,[4] at Krapina in Northern Croatia.[5] Fourteen skeletons, which may evidently be assigned to the same group, have been found at Předmost in Moravia,[6] and another at its capital, Bruenn.[7]

But the Palaeolithic Age, in Britain as in other parts of Europe,

[1] Der Neanderthalschädel (*Bonner Jahrbücher*, Heft 106, 1901, pp. 1–72). See also *Globus*, lxxx, 1901, pp. 217–22; lxxxi, 1902, pp. 165–74; the notices of Schwalbe's article in *Man*, ii, 1902, No. 129, pp. 186–9; and *L'Anthr.*, xiii, 1902, pp. 356–8, xvii, 1906, pp. 67–72.

[2] Mr. J. Gray (*Man*, iv, 1904, No. 17, pp. 28–9) summarizes Schwalbe's most recent views (*Die Vorgeschichte des Menschen*).

[3] Professor Johnson Symington (*Report of . . . the Brit. Association*, 1903, p. 798) holds that Schwalbe 'has not sufficiently recognised the significance of the large cranial capacity of the Neanderthal skull . . . or made sufficient allowance for the great variations in form which skulls undoubtedly human may present'; and he affirms that the Neanderthal skull 'was capable of lodging a brain fully equal in volume to that of many existing savage races'.

[4] See p. 40, *supra*.

[5] *L'Anthr.*, xvi, 1905, pp. 17–8.

[6] *Ib.*, p. 395.

[7] *Ib.*, pp. 396–7.

was of such immense duration that it would be absurd to assume that it had no other representatives than men of the Neanderthal type; and the 'artists' of the latest period, whose creations have been discovered in the caves of La Madelaine and Les Eyzies,[1] belonged to a different race, represented by skulls discovered at Laugerie-Basse and Chancelade in the valley of the Lozère. While these skulls are hardly less dolichocephalic than those of the Neanderthal type, they are in other respects strikingly different, being much more capacious, and having high and broad foreheads, and brow ridges which are hardly perceptible.[2] Although no skulls of this kind have been found in our own country, it is not improbable that men of the stock to which they belonged penetrated into Britain; for in one of the caves of Creswell Crags in Derbyshire there has been found a bone engraved with the figure of a horse's head,[3] which reminds one of the spirited designs of the artists of the Dordogne, and was associated with implements of the kind which have been found in the caves of La Madelaine and Les Eyzies and others of the Dordogne basin.[4]

The recent systematic exploration of the Baoussé-Roussé caves near Mentone is of the highest importance because it has demonstrated an intimate connexion between palaeolithic and neolithic races in Southern France. All the interments have been proved to be palaeolithic.[5] The newest skeleton in the Grotte des Enfants approximates to the dolichocephalic type of the Neolithic Age.[6] Beneath it, 5 metres 15 millimetres lower down, lay a gigantic skeleton, closely resembling but far older than that of the famous 'old man' of Cro-Magnon, which is commonly assigned to the earliest neolithic times, but may possibly be as old as the period that in France is recognized as transitional.[7] This skeleton has certain negroid characteristics,[8] which, however, are more pronounced in the two most ancient skeletons of the Grotte des Enfants,

[1] See p. 35, *supra*.

[2] See J. Deniker, *The Races of Man*, pp. 311–2 and fig. 87. Dr. J. G. Garson (*Nature*, Nov. 22, 1894, pp. 90–1) implies, if I do not misunderstand him, that the Laugerie-Basse skeletons belonged to the Neanderthal race. I can only invite the reader to compare the illustrations of the two types, and refer to Deniker, Philippe Salmon, and the French anthropologists generally in support of my view. But when Salmon (*L'Age de la Pierre*, p. 64) remarks that 'le crâne de Laugerie-Basse . . . présente une forme manifeste de transition entre le type des premiers temps quaternaires et ceux de Cro-Magnon' [the oldest of the French neolithic skulls], I am unable to follow him. See *Geogr. Journ.* xxviii, 1906, p. 546. The known skulls of Neanderthal type do not belong to 'les premiers temps quaternaires', and the age of the Neanderthal skull is unknown. See p. 34, *supra*.

[3] See p. 35, *supra*.

[4] I find that Sir John Evans (*Report of . . . the Brit. Association*, 1897 [1898], p. 12) has argued in the same sense.

[5] *L'Anthr.*, xvii, 1906, p. 292.

[6] *Ib.*, and p. 111.

[7] *Ib.*, pp. 110, 292, 297. M. Verneau holds that the skeletons of Laugerie-Basse and Chancelade are the 'arrière-petits-fils' of this inhabitant of the Mentone cave.

[8] *Ib.*, p. 299.

discovered 70 millimetres lower still and associated with the bones of a rhinoceros.[1] M. Verneau argues that the prognathism which appears in certain skeletons of Western Europe of the early Bronze Age was connected by atavism with these primitive denizens of the Riviera.[2]

2. Professor Boyd Dawkins draws a sharp distinction between 'the River-drift men' and 'the Cave-men'. I must remark that the term 'Cave-men' is not happily chosen; for the professor himself assures us that 'the Cave-men did not always use caves', and that 'the habit of camping in the open air must have been the rule . . . because caverns and rock-shelters are only met with in very limited areas'; [3] while on the other hand he points out that 'River-drift men' often lived in caves.[4] By 'the Cave-men' he means those who made implements of what he terms 'the higher types', that is, the types which are called after the caves of Le Moustier, Solutré, and La Madelaine. Observing that there were 'Cave-men' not only in our own country and in France, but also in Belgium, Switzerland, and Germany, he argues that 'from this distribution of the implements it is evident that the Cave-man belongs neither to the southern group of the Pleistocene animals nor to the temperate which found its way over the mountain barriers into Spain, Italy, and Greece. On the other hand,' he continues, 'the River-drift man must be considered as a member either of the temperate or southern fauna of Europe, because his remains are met with in the regions of the Mediterranean, north [and also south] of those mountain barriers.'[5]

Granting that no implements of the higher types have been discovered in caves south of the 'mountain barriers', it is hardly safe to conclude that the 'Cave-men' did not belong either to the southern or the temperate group of mammals.[6] The question is

[1] *L'Anthr.*, xvii, 1906, pp. 292–3. Cf. vol. xvi, 1905, pp. 503–6. M. E. Piette (*Bull. et mém. de la Soc. d anthr.*, 5e sér., iii, 1902, pp. 773–4), if I do not misunderstand him, attributes negroid characters to the Neanderthal race.

[2] *L'Anthr.*, xvii, 1906, pp. 308–9. It has been maintained that another—the so-called steatopygous—race existed in Gaul in late palaeolithic times. If any reader does not know the meaning of 'steatopygous', let him use his dictionary, and he will pardon me for not having translated the word into plain English. The existence of this people is inferred from the discovery of certain 'statuettes' at Brassempouy in the department of the Landes (*L'Anthr.*, vi, 1895, pp. 129–51) and near Mentone. I have not seen them; but when I saw the woodcut of one which was selected for illustration (*Bull. et mém. de la Soc. d'anthr.*, 5e sér., iii, 1902, p. 775, fig. 4), it seemed to me that the carving was so villainous that no scientific conclusion could be drawn from it; and I am glad to find (p. 778) that this was the opinion of M. Manouvrier. M. Piette, however, assures us (*L'Anthr.*, vi, 1895, p. 143) that the 'Venus of Brassempouy' is 'l'œuvre d'art la plus parfaite qui soit sortie des mains des sculpteurs éburnéens'. Anyhow, though it would not be difficult for a sculptor to make statuettes of steatopygous individuals in the England of to-day, there is no evidence that the 'race' in question, if it existed in palaeolithic Gaul, ever penetrated into this country.

[3] *Early Man in Britain*, p. 207.

[4] *Ib.*, pp. 230, 243.

[5] *Ib.*, pp. 204–5.

[6] *Ib.*, p. 111, fig. 24.

whether the implements to which the professor refers were characteristic of one palaeolithic race to the exclusion of others. Assuming that such implements do not exist outside the area in which they have been found—a very rash assumption—it does not follow that the men who made them belonged to a race different from their contemporaries whose tools have been discovered in the drift. Only one interment of the Late Celtic Period has been found in Scotland, and that quite recently;[1] yet there were numerous Celts then in North as well as in South Britain.

The professor also insists[2] that 'the absence of the higher types of implement in the camping-places of the River-drift men cannot be accounted for on the ground that they are smaller or . . . more perishable'; for, he says, 'camping-places of the Cave-men have been met with in France [for instance at Solutré] . . . in which the implements are associated in the same manner as in the caves'.

I reply, first, that it is begging the question to say that the men who encamped at Solutré were 'Cave-men' as distinct from 'River-drift men'; secondly, that implements of Le Moustier type, which were characteristic of the earliest French 'Cave-men',[3] are common both in France and Britain in the river-drift;[4] and thirdly, with due deference to the professor, that the absence 'of the higher types of implement' from the river-drift is as easily explicable as the absence of implements of bone or wood:—partly they were more perishable and would be more difficult to find, and partly they were less likely to be used in the field.[5] Besides, is it not possible that none of the very few palaeolithic 'camping-places' that have been found in this country belonged to the Solutrean period? As we have seen, the professor himself affirms that 'the Cave-men' encamped as a rule not in caves but in the open air: they, like 'the River-drift men' were, as he himself assures us, hunters: why then have hardly any of their 'higher types of implement' been found in this country in the field? Simply for the reasons which I have given. And since 'the Cave-men', like 'the River-drift men', lived commonly in the open air, how could the latter, even if they belonged to a different race, have escaped the influence of the former or have failed to acquire their culture? And how could the two races have escaped amalgamating?

The 'Cave-men', as Professor Boyd Dawkins himself admits,[6] undoubtedly used certain implements of river-drift type as well as 'the higher types'; nor is there any reason to suppose that the 'River-drift men' did not use implements of 'the higher types' as well as implements of river-drift type, except the fact, easily accounted for, that the former are not found in the drift. Professor Boyd Dawkins himself strenuously maintains that 'River-drift men' as well as 'Cave-men' lived in caves.[7] How then can he

[1] See p. 435, *infra*.
[2] *Early Man in Britain*, pp. 230–1.
[3] *Ib.*, pp. 202–3.
[4] See p. 40, *supra*.
[5] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 475, 641–3.
[6] *Early Man in Britain*, p. 230.
[7] *Ib.*, and p. 243.

prove that the two sets of occupants were ethnologically different? He insists that 'the river-drift implements in the Caves of Creswell Crags, of Kent's Hole, and of the Grotte de l'Église, are found in the strata below those with the implements of the Cave-men, and consequently that the River-drift men lived in Britain and France before the Cave-men.'[1] But on his own showing the owners of both sets of implements did live in caves; and so far nothing is proved except that those who used one set were more ancient than those who used the other. 'Some caves also,' he adds, 'were inhabited by River-drift men, who have left behind their implements without any trace of the higher types of the Cave-men.'[2] But here again nothing is proved save that these particular 'River-drift men' had not yet learned to make 'the higher types'. The professor might have a good case if he could say, River-drift implements have been found in the lower strata of caves: in the upper strata none have been found, but only 'the higher types'; consequently the men who used the higher types were quite different from those of the later Palaeolithic Age whose implements have been recovered from river-drift. But this he could not truly say; for implements of river-drift type have been found, although rarely, in the highest strata of caves.[3] Lastly, I would ask the professor, who insists that 'the Cave-men' were 'northern mammals', and that they did not enter Europe until long after the appearance of 'the River-drift men', to tell us whence they came.

3. Are we to count the palaeolithic inhabitants of Britain among our ancestors? 'I do not consider,' says Dr. Garson, 'that there is any evidence of the existence of the direct descendants of Palaeolithic man among the osteological remains of Neolithic or subsequent date in Britain.'[4] On the other hand, Dr. Beddoe[5] thinks that the oldest inhabitants of this country may have left descendants, whom he is inclined to identify with 'some Mongoloid race', traces of which, he believes, are discernible in the population of the west of England; while two distinguished French anthropologists, MM. de Quatrefages and Hamy, affirm that the Neanderthal race 'has left a permanent imprint on the population of the three kingdoms',[6] and refer to various skulls of the Neolithic and later periods which resemble more or less closely that of Neanderthal.[7] More-

[1] *Early Man in Britain*, p. 230. [2] *Ib.*, p. 243.

[3] Sir J. Evans, *Anc. Stone Implements*, 1897, pp. 475, 499–500, 522, 641; *Guide to the Ant. of the Stone Age* (Brit. Museum), p. 62.

[4] *Nature*, Nov. 22, 1894, p. 90.

[5] *The Races of Britain*, 1885, pp. 8–9.

[6] *Crania Ethnica*, pp. 28–9.

[7] See *Quart. Journ. of Science*, 1864, p. 96; *Mem. Anthr. Soc.*, i, 1865, pp. 288–90; *Anthr. Review*, iii, 1865, pp. 372–3; S. Laing, *Prehist. Remains of Caithness*, pp. 114, 125, and fig. 44–7, 60–61; Worthington Smith, *Man, the Primeval Savage*, pp. 37–9; J. Deniker, *The Races of Man*, p. 312; and *Journal of Anatomy and Physiology*, xxxix, 1905, pp. 423–4. Dr. Wright (*Ib.*, xxxviii, 1904, p. 120) has described two skulls of Chancelade type, found in a round barrow near Garton-on-the-Wolds of the late Stone Age or Early Bronze Age.

over, it is generally admitted that even at the present day a few individuals here and there belong to the same type.[1] But it does not follow that these persons or those to whom Dr. Beddoe and M. Hamy refer were descended from men who lived in Britain in the Palaeolithic Age. That palaeolithic man left no descendants in any part of the world is of course not maintained even by the most ardent supporters of the theory of the 'Hiatus': somewhere or other there must have been a link; but Sir John Evans, as I have observed in the first part of this book,[2] argues from the supposed absence of intermediate forms of implements that it did not exist in this country; and Dr. Keane [3] thinks that 'the few scattered palaeolithic hunters could scarcely have lived through the last ice-age in a contracted region at one time reduced by subsidence to a mere cluster of islets', &c. The answer is, first that there is no reason to believe that in 'the last ice-age' or at any time between the dawn of the latest palaeolithic period and the arrival of neolithic man Britain was 'a mere cluster of islets';[4] and secondly that, as Professor Boyd Dawkins assures us, out of forty-eight species of mammalian fauna living in Britain in the Palaeolithic, thirty-one survived in the Neolithic Age.[5] Professor Boyd Dawkins, however, insists that 'the mere contrast between the Palaeolithic and wild Neolithic faunas implies a zoological break of the first magnitude'.[6] I take leave to say that it implies no break at all, seeing that thirty-one of the older species confessedly lived on: it implies no more than is implied by the disappearance of the urus, the wolf, the wild boar, and many other animals which were living in this island at a time since which it has been continuously inhabited by man. The professor triumphantly points out that in those caves which were successively used as dwellings by palaeolithic and neolithic people 'the remains of the domestic animals are found *alone* in the upper Prehistoric [or neolithic] strata.'[7] Undoubtedly. But what then? The fact does not prove that palaeolithic man had become extinct when neolithic man arrived: it merely proves that the latter had domestic animals, and that the former had not. Arab horses, Siamese cats, and many other animals have been introduced into this country since the Christian era: yet the people who were here before their introduction did not become extinct. And if 'in a great many cases the lower Palaeolithic strata [in caves] are sealed down, and mapped off from the Neolithic, by a layer of

[1] J. Deniker, *The Races of Man*, p. 312, n. 1; *Scottish Review*, xx, 1892, pp. 148, 152–3; *Proc. Geologists' Association*, xv, 1899, p. 261; *Nature*, March 7, 1901, p. 457.

[2] See p. 59, *supra*.

[3] *Ethnology*, 1896, p. 113.

[4] See pp. 19–22, 59–60, 62, *supra*.

[5] *Trans. Internat. Congr. Prehist. Arch.*, 1868 (1869), p. 278. Mr. F. C. J. Spurrell indeed affirms (*Proc. Geologists' Association*, xi, 1891, pp. 226–7) that remains of some of the extinct mammals, including the elephant, 'are found high up in the' alluvium, and that mammoths' teeth, not 'derived', have frequently been met with in peat in the valley of the Thames.

[6] *Journ. Anthr. Inst.*, xxiii, 1894, p. 246.

[7] *Ib.*

stalagmite',[1] that only proves 'a break of continuity between the two periods' as far as those caves are concerned. The Palaeolithic Age, says the professor, 'was continental, the Neolithic insular in North-Western Europe.'[2] He means that in the Palaeolithic Age Great Britain was an outlying part of the Continent, and that the neolithic invaders had to sail across the Channel.[3] But why should the formation of the Channel have extinguished the palaeolithic race? 'There is obviously,' continues the professor, 'a great gulf fixed between the rude hunter civilisation of the one and the agricultural and pastoral civilisation of the other.' Obviously. But the gulf is not more obvious than that which separated the civilization of the Red Indians from the civilization of the Pilgrim Fathers. Yet the Red Indians still lived on.

It is true that if the professor has failed to show that the Palaeolithic Age in Britain was abruptly terminated, he has no difficulty in disposing of certain arguments which have been adduced to show that it was not. When, for instance, Mr. Allen Brown points to the implements of palaeolithic type which were found in the refuse heaps of the neolithic settlement at Cissbury in Sussex, he replies that 'in the vast accumulation of refuse, representing every style in the chipping, from the rough block of flint . . . to the highly finished axe, broken . . . by an unhappy blow, it is obvious that there must be some which would represent well-known Palaeolithic types.'[4] Nevertheless it remains true that not one of the facts which he has stated is inconsistent with the hypothesis that men may have lived on in Britain in the palaeolithic stage of culture until the time when the first neolithic immigrants arrived. What his opponents suggest is that certain types of palaeolithic implements survived into the Neolithic Age;[5] in other words, that implements of those types continued to be manufactured or used then. That this was the case in Ireland is certain;[6] and, since there is no evidence of a Palaeolithic Age in Ireland, it seems not unreasonable to conjecture that they were made by descendants of palaeolithic refugees from Britain or Gaul. Mr. Allen Brown may be wrong in maintaining that implements which he has found 'at or near the surface' at East Dean in Sussex are 'mesolithic', that is, belong to a period of transition;[7] but Sir John Evans himself says[8] of some of the implements, usually classed as palaeo-

[1] *Journ. Anthr. Inst.*, xxiii, 1894, p. 246. [2] *Ib.*, p. 248.

[3] Even this, however, is not absolutely certain. See p. 62, *supra*.

[4] *Journ. Anthr. Inst.*, xxiii, 1894, p. 250.

[5] *Ib.*, p. 255. Cf. A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 10 (pref.), note.

[6] See Mr. W. J. Knowles's valuable article in *Journ. Roy. Soc. Ant. Ireland*, 5th ser., vii, 1897, pp. 1–18.

[7] *Journ. Anthr. Inst.*, xxii, 1893, p. 98. See pl. iii and iv, facing p. 98. Mr. F. C. J. Spurrell (*Proc. Geologists' Association*, xi, 1891, pp. 225–6, note) and Mr. Worthington Smith (*Man, the Primeval Savage*, p. 299) also regard certain British implements as mesolithic; and Mr. Spurrell (*op. cit.*, p. 226) gives reasons for believing that the Tilbury skull belonged to a period of transition.

[8] *Anc. Stone Implements*, 1897, p. 501.

lithic, which have been found in the cave earth of the famous Kent's Cavern in a position which authorizes us to assume that the people to whom they belonged were not separated by any 'hiatus' from the palaeolithic race whose remains were found immediately underneath, that 'so far as form is concerned, there is little or nothing to distinguish them from the analogous implements of the Neolithic Period'. Is it not possible that these and some of the ruder implements which have hitherto been classed as neolithic may have been fabricated not by neolithic immigrants but, after their immigration, by descendants of the palaeolithic race?[1] Those who deny that mesolithic implements have been found in Britain deny also that they have been found anywhere else. Granted for the sake of argument. But if their general absence does not weaken the certainty that the supposed hiatus was not universal, how can their absence in Britain prove that there was a hiatus here? In Part I I have shown that it is impossible to frame any theory which shall account satisfactorily for the assumed disappearance of British palaeolithic man. Professor Boyd Dawkins asks us to believe that the 'Cave-men' fled in terror before the neolithic invaders and eventually settled in Greenland, where they became the ancestors of the Eskimos; and in support of this theory he assures us that 'Palaeolithic man appeared in Europe with the arctic mammalia, lived in Europe along with them, and disappeared with them'; that the gloves of the 'Cave-men' were 'similar to those now used by the Eskimos'; that their implements 'are of the same kind as those of the Eskimos'; that, like the Eskimos, they did not take the trouble to bury their dead; and that 'the most astonishing bond of union between the Cave-men and the Eskimos is the art of representing animals'.[2] Judging from the specimens of Eskimo art which the professor gives, I confess that what I find astonishing is its inferiority to that of the Cave-men;[3] there is no evidence that the Cave-men of Britain wore gloves; and if they did, may not the reason have been, not that there was any connexion between them and the Eskimos, but that their hands were cold? Is the professor sure that 'the River-drift men' did not also wear gloves? We do not know whether palaeolithic man appeared in Europe with the arctic mammalia: he certainly did not accompany them from the north; and it is an article of faith with French anthropologists that he did not disappear with them, but became the ancestor of

[1] To quote Mr. Allen Brown (*Journ. Anthr. Inst.*, xxii, 1893, p. 93), 'Sir J. Evans says, "It is almost demonstrable that some of the chipped celts which have hitherto been classed as Neolithic must be among the earliest of the Neolithic implements," and "must in all probability date back to a very distant period". It is to these forms, which appear to be of transition age, that I would apply the term Mesolithic . . . At present some flint implements, which from their form would be ranged under one of the later Palaeolithic groups by the French geologists, would be included in the . . . Neolithic in England.' Mr Brown's quotation from Sir John Evans (*Anc. Stone Implements*, pp. 85–6) is substantially but not verbally accurate.

[2] *Cave Hunting*, pp. 353–9; *Early Man in Britain*, pp. 233–42

[3] See A. Lang, *Custom and Myth*, 1885, p. 310.

neolithic man. There is a general resemblance between the palaeolithic drift implements of all countries; and in the earlier part of this volume many facts have been noted which show how cautious one should be in inferring identity of race from similarity in implements, weapons, or ornaments. There is not the slightest evidence that 'the Cave-men' did not bury their dead; and there is irrefragable evidence, as we have seen, that cave-men in the Riviera and in Croatia did.[1] Again, since the professor differentiates the 'Cave-men' from the 'River-drift men' of Britain, can he prove that the latter did bury their dead? If not, what becomes of his argument? Finally, the theory that the Eskimos are descendants of 'the Cave-men' of Western Europe has been rejected by every recent inquirer.[2]

How does Professor Boyd Dawkins account for the disappearance, which he assumes, of palaeolithic man? 'Simply,' he says, 'by assuming that at the close of the Pleistocene age, when they came into contact with Neolithic invaders, there were the same feelings between them as existed in Hearne's times between the Eskimos and the Red Indian, terror and defenceless hatred being, on the one side, met by ruthless extermination on the other. In this way the Cave-men would be gradually driven from Europe.'[3] That men who were ruthlessly exterminated should have survived to become the ancestors of the Eskimos is certainly remarkable. But seriously I would ask the professor whether he has really succeeded in persuading himself that 'the Cave-men' were one and all either exterminated or driven out of Europe. Did none remain? He assures us that 'the Cave-men' migrated eastward; and he still insists, in defiance of all French craniologists, that 'neither of the two races of Palaeolithic man have left behind any marks in the existing population of Europe'.[4] How they contrived to make their slow progress across the Continent without leaving one descendant is a problem which he does not attempt to solve. And since he himself admits, or rather affirms, that they 'came into contact with Neolithic invaders', it is difficult to see how he can maintain the existence of a hiatus.

The professor has asserted that there is 'no evidence in any part of the world of a continuity between the Palaeolithic and Neolithic ages'.[5] Yet he of course admits that it must have existed somewhere. Good reasons have been given for believing that it existed in France.[6] Why not also in Britain?[7]

[1] See p. 49, *supra*.

[2] See *Journ. Anthr. Inst.*, xiv, 1885, pp. 387–8; *L'Anthr.*, xvi, 1905, pp. 707–9; xvii, 1906, pp. 180–2.

[3] *Cave Hunting*, p. 243.

[4] *Vict. Hist. of . . . Somerset*, i, 179.

[5] *Journ. Anthr. Inst.*, xxiii, 1894, p. 257.

[6] *L'Anthr.*, vii, 1896, pp. 1–17, 388–9; R. Munro, *Prehist. Problems*, pp. 66–81; *Archaeol. Journal*, lv, 1898, pp. 277–84; *Athenaeum*, Jan. 14, 1899, p. 53; *Rev. de l'École d'anthr.*, ix, 1899, p. 275, xiv, 1904, pp. 160, 378; *Bull. et mém. de la Soc. d'anthr.*, 5e sér., v, 1904, p. 614; *Association franç. pour l'avancement des sc.*, 33e sess., 1904 (1905), p. 1035.

[7] See p. 61, *supra*.

[7] Dr. A. J. Evans, who in 1893 was 'so overpowered by the vision of the

Such are the reasons by which I endeavour to justify myself in refusing to believe that neolithic man, when he entered Britain, found none to welcome or to oppose him save the thirty-one species of mammalian fauna which Professor Boyd Dawkins has spared.

V. THE PYGMIES (?)

British pygmies are the creation of Celtic imagination. The evidence on which we are required to believe that they existed is this. Professor Rhys[1] suggests that the name of the Coritani, a tribe mentioned by Ptolemy,[2] who inhabited the country between the Trent and the Nen, is related to the word *cor*, a dwarf. 'Then,' the professor concludes, 'we should have accordingly to suppose the old race to have survived so long and in such numbers that the Celtic lords of Southern Britain called the people of that area by a name meaning dwarfs.' Afterwards, referring to various articles by Mr. David MacRitchie, he observes that in certain parts of Wales and Scotland there are mounds enclosing cells, which are 'frequently so small as to prove beyond doubt that those who inhabited them were of remarkably small stature';[3] and he finds in Welsh, Irish, and Scotch folk-lore traditions which confirm him in the belief that these cells were inhabited by dwarfs, whom he calls 'the Mound Folk'. 'This strange people,' he tells us, 'seems to have exercised on the Celts . . . a sort of permanent spell of mysteriousness and awe stretching to the verge of adoration . . . the Celt's faculty of exaggeration, combined with his incapacity to comprehend the weird and uncanny population of the mounds and caves . . . has enabled him . . . to bequeath to the great literatures of Western Europe a motley train of dwarfs,'[4] &c. The professor's conclusion[5] is that the earliest people who inhabited these islands [apparently after the Palaeolithic Age] were 'the mound folk, consisting of the short swarthy people variously caricatured by our fairy tales'; and that they were conquered by neolithic invaders, who, he tells us, 'made slaves and drudges of the mound-haunting race.'

'These,' the professor warns us, perhaps superfluously, 'are conjectures which I cannot establish; but possibly somebody else may.'

I venture to hint a doubt. Not only is the derivation of *Coritani* utterly uncertain,[6] but it is safe to assume that the Celtic tribe

yawning hiatus' between the Palaeolithic and the Neolithic Age that he regarded the skeletons of the Baoussé Roussé caves as neolithic, has of course since recanted (*Report of . . . the Brit. Association*, 1896, p. 908).

[1] *Celtic Folk-Lore*, 1901, pp. 675–6.

[2] *Geogr.*, ii, 3, § 11.

[3] *Celtic Folk Lore*, pp. 679–80.

[4] *Report of . . . the Brit. Association*, 1900, p. 888.

[5] *Celtic Folk Lore*, pp. 683–6.

[6] In *Celtic Britain*, 1884, p. 288, Professor Rhys suggested that *Coritani* might be a pre-Celtic word; and as the suggestion is repeated on p. 293 of the

who undoubtedly conquered the country which belonged in Ptolemy's time to the Coritani would not have called its population, themselves included, by a name which described not even the people whom they found in possession, but the 'slaves and drudges' of that people, or rather of their neolithic predecessors! The professor indeed argued in *Celtic Folk-Lore*[1] that the Coritanian dwarfs 'may be conjectured to have had quiet from invaders from the Continent because of the inaccessible nature of their fens'. How then did they themselves and the non-dwarfish invaders of the Bronze Age get there? It is almost superfluous to remark that in the year before and in the year after the publication of *Celtic Folk-Lore* the professor counted the Coritani among the Brythonic 'invaders from the Continent'.[2] The 'mound-dwellings' which Mr. MacRitchie describes[3] belong to the class of structures which are popularly known as 'Picts' houses', 'Earth-houses', or 'Weems', and are immeasurably later than the period to which Professor Rhys's theory would compel him to assign them. The mere fact, indeed, that many of them have been shown by excavation to have been occupied in Roman times does not prove that they were not constructed earlier; but I can find no evidence that any of them belong even to the Neolithic Age. Mr. MacRitchie himself assures us that one which was opened at Crichton in Mid-Lothian 'was proved to have been built not earlier than 80 A.D.';[4] and he assigns the 'mound-dwellings' in general not to a pre-neolithic race but to the Picts of historic times.[5] He also says that one which was

3rd edition, which has just appeared (November, 1904), it would seem that he does not set great store by his intermediate conjecture,—that *Coritani* is derived from the Celtic word *cor*.

[1] Vol. ii, pp. 675–6.

[2] *The Welsh People*, 1900, and 3rd ed., 1902, pp. 111–2. The references which Prof. Rhys gives to Ripley's *Races of Europe* (pp. 322, 328, 521) do not prove that the country of the Coritani was inhabited by dwarfs, but only by descendants of the neolithic population.

[3] *Fians, Fairies and Picts*, 1893, pp. 44–53; *Antiquary*, xxxvi, 1900, pp. 53–6, 70–4; *Scottish Notes and Queries*, 2nd ser., i, 1900, pp. 137–9; *The Reliquary and Illustrated Archaeologist*, N. S., vii, 1901, pp. 89–97; *Monthly Review*, Jan., 1901, pp. 131–48; *Trans. Glasgow Archaeol. Soc.*, N. S., iv, 1902, pp. 179–94. See also *Archaeol. Journal*, x, 1853, pp. 212–23; xx, 1863, pp. 32–7; Sir A. Mitchell, *The Past in the Present*, 1880, pp. 59–72; *Journ. Brit. Archaeol. Association*, xxxvii, 1881, pp. 254–61; *Report of . . . the Brit. Association*, 1902 (1903), p. 755; and *Proc. Soc. Ant. Scot.*, xxxvii, 1903, pp. 352–9.

[4] *Scottish Notes and Queries*, 2nd ser., i, 1900, pp. 137–8. 'Mound-dwellings' and other 'earth-houses' are commonly assigned to the Early Iron Age. See R. Munro, *Prehist. Scotland*, 1899, pp. 345–81.

[5] He tells us (*Monthly Review*, Jan., 1901, pp. 139–40) that 'the early Gaelic chroniclers assert that the Gaels were preceded in Scotland and Ireland by two races' [the Picts and the Dananns]. 'Of them too,' he adds, 'it is said that they lived in hidden habitations, that they also persecuted the newer race.' In other words, the 'mound-folk' who, according to Professor Rhys, were 'slaves and drudges' of the neolithic race, were themselves persecutors of the Celts. That the Picts, or some of them, lived in 'hidden habitations' I am not concerned to deny; as for the 'Dananns' of Irish legend, I would ask Mr. MacRitchie to read what Professor Rhys (*Celtic Heathendom*, p. 119) has written about them. 'The earliest Scottish writer, so far as I am aware,'

explored in 1855 contained four chambers, of which the largest was '6 feet 2 inches long, 4 feet 6 inches in height, and 2 feet 6 inches wide', and, with a fascinating lack of humour, he adds that 'while the size of the stones used in its construction is evidence of great personal strength on the part of the builders, the small and narrow rooms seem to indicate a diminutive race.'[1] When the reader is invited to believe that 'those who inhabited' these 'rooms', which were only built by the exertion of 'great personal strength', 'were of remarkably short stature', he falls to calculating whether even a race of Tom Thumbs, each of whom possessed the muscular power of a Sandow, would not have used their strength to make their rooms a little more comfortable.[2] Mr. MacRitchie shows more acumen when, after remarking that 'two alleged Fairy Knowes in Shetland' proved on investigation to be natural hillocks, and that another in Stirlingshire 'was only a sepulchral mound', he concludes that these instances are 'sufficient to show the unreliable nature of popular tradition'.[3] If it was 'the Celt's faculty of exaggeration' that 'enabled him to bequeath to the great literatures of Western Europe a motley train of dwarfs', why should he not have exercised his faculty upon the comparatively short neolithic population rather than upon the imaginary pygmies whom Professor Rhys has appointed as their 'slaves and drudges'?[4] And if the imagination which created 'a motley train of dwarfs' had pygmies for its basis of fact, will the professor tell us who were the originals of the 'motley train' of giants whom the imaginations of various European peoples associated with the dwarfs?[5] I am aware that Professor

continues Mr. MacRitchie (*Monthly Review*, Jan., 1901, p. 141), 'who speaks of the Picts as a small race living underground was a fifteenth-century Bishop of Orkney, Thomas Tulloch . . . Tulloch compiled a Latin account of Orkney (*De Orcadibus Insulis*) . . . and therein he states that the Picts inhabiting those islands . . . in the ninth century were "not much bigger than pigmies in stature", and that . . . they occasionally took refuge "in little houses underground"'. The work of Tulloch, or rather Tullock, is not mentioned in the catalogue of the British Museum; and I cannot verify Mr. MacRitchie's quotation. But is the statement of a fifteenth-century compiler about the stature of a people who lived in the ninth century to be taken seriously as evidence? And if so, what does it prove about the Picts as a whole? What more does it prove than this,—that in a remote group of islands there were dwarfish people who were included under the name 'Picts',—a name which of course denoted not a race but a heterogeneous population, comprising people whom the physical anthropologist would classify under several heads?

[1] *Antiquary*, xxxvi, 1900, pp. 54–5.

[2] Cf. *Archaeol. Journal*, xx, 1863, pp. 33–4. Some 'mound-dwellings', the chambers in which were of habitable though very small size, have, I am of course aware, been proved to have been really dwellings.

[3] *Antiquary*, xxxvi, 1900, p. 73. See also *Proc. Soc. Ant. Scot.*, vii, 1870, pp. 519–23.

[4] I find that this suggestion is supported by Mr. W. C. Mackenzie (*ib.*, xxxix, 1905, p. 257), who truly says that tradition 'measures its low-statured people by inches, just as it measures its tall peoples by yards'.

[5] See W. C. Borlase, *Dolmens of Ireland*, ii, 552–3, 687, iii, 801, 805, 810, &c. Canon Greenwell (*Brit. Barrows*, p. 344) tells us that he has examined many mounds in Westmorland, locally called 'Giants' Graves', without finding anything in them. In regard to the danger of trusting to legend and folk-lore

Kollmann[1] claims to have proved that pygmies existed in prehistoric times in France, Germany, Switzerland, and other European countries; but the fact remains that no evidence has been produced that a race of pre-neolithic or even prehistoric pygmies existed in this country save only that which is furnished by 'the Celt's faculty of exaggeration'.

VI. NEOLITHIC MAN

The remains of neolithic man have been discovered in caves, in cairns, in submerged forests, and in barrows in Essex, Wiltshire, Gloucestershire, Somersetshire, Derbyshire, Staffordshire, Yorkshire, Caermarthenshire, Denbighshire, the Isle of Man,[2] Argyllshire and the island of Arran, Caithness, and the Orkney Islands.[3] The neolithic population, however, it need hardly be said, were scattered over many other parts of Britain in which their skeletons have not come to light. Many anthropologists consider that all of them belong to one race; but at all events the great majority represent men of medium stature with long skulls; and it is a generally accepted article of faith that no long barrow has ever yielded any article of metal in association with a primary interment, and that no skull whose cephalic index exceeded 79, belonging to a primary interment, has ever been found in a long barrow since the time when anthropologists first began to measure skulls in this country.[4] According to a table[5] published by Dr. Beddoe in 1894, the value of which has been confirmed by later measurements,[6] the cephalic indices of 87 skulls belonging to the Neolithic Age ranged from 63 to 79; and, as Dr. Thurnam points out,[7] some of them are more dolichocephalic than those of any modern European people.

When we come to examine the stature of the neolithic Britons, we find that, according to Thurnam's latest estimate,[8] the average height of 25 male skeletons found by him in long barrows

as evidences of the former existence of giants and dwarfs, see E. B. Tylor, *Prim. Culture*, i, 1903, pp. 385–8.

[1] *Zeitschrift für Ethnologie*, xxvi, 1894, pp. 189–254; *Die Pygmäen und ihre systematische Stellung innerhalb des Menschengeschlechts*, 1902, reviewed in *Man*, iii, 1903, No. 62, p. 112. See also *L'Anthr.*, xv, 1904, pp. 37–9.

[2] *Mem. Anthr. Soc.*, iii, 1870, p. 51.

[3] J. Beddoe, *The Races of Britain*, p. 13.

[4] *Mem. Anthr. Soc.*, iii, 1870, p. 41. Mr. J. R. Mortimer (*Journ. Anthr. Inst.*, vi, 1877, p. 333) has, however, affirmed that 'the few explored long barrows' of the district between Driffield and Aldro' in Yorkshire have yielded skulls whose cephalic indices exceeded 80; and one of the skulls found in the cave of Perthi-Chwareu in Denbighshire had a cephalic index of 80. See p. 396, n. 17, *infra*.

[5] *L'Anthr.*, v, 1894, p. 522. See also p. 517, n. 1.

[6] See A. Pitt-Rivers, *Excavations in Cranborne Chase*, iv, 30 (pref.).

[7] *Mem. Anthr. Soc.*, iii, 1870, p. 59.

[8] Based on Dr. Humphry's estimate of the relation of the thigh-bone to the height, viz. 27·5 : 100.

was 5 feet 5·4 inches,[1] or 1 metre 661 : but Dr. Beddoe gives good reasons, to which I have already called attention,[2] for believing this estimate to be too low; and his own is 5 feet 6·7 inches, or 1 metre 694.[3] Recent measurements (although they include those of individuals under 5 feet) do not invalidate the evidence of these figures;[4] and the few Scottish skeletons which undoubtedly belong to the Neolithic Age have yielded practically the same results.[5]

Dr. Garson, describing the dolichocephalic Long Barrow skulls, with which anthropologists agree in associating those that have been found in the 'horned cairns' of Caithness,[6] in the neolithic cairns of the isle of Arran,[7] and in the caves of Oban,[8] says that 'the superciliary ridges and glabella [the surface between the superciliary ridges] are moderately or even feebly developed . . . the malar [or cheek] bones are never prominent . . . there is no tendency to prognathism . . . as a whole the face is oval in form; the jaws are small and fine . . . the facial characters are mild and without exaggerated development in any direction'.[9] It may be added that the Long Barrow skulls, as Thurnam pointed out, are 'more or less depressed—platycephalic',[10] and that the nose is usually aquiline.

The general truth of the foregoing descriptions will be apparent to any one who examines the plates in *Crania Britannica*; but we must take account of exceptions. As Dr. Davis pointed out,[11] a skull found in the Long Lowe barrow, near Wetton in Staffordshire, is very different from another dolichocephalic skull from a chambered long barrow at Uley in Gloucestershire.[12] In the latter

[1] *Mem. Anthr. Soc.*, iii, 1870, pp. 71–3. According to the method recommended by Thurnam in *Crania Britannica*, ii, pl. 42, p. 3, n. ‡, the average, deduced from the data which he furnishes in *Mem. Anthr. Soc.*, iii, 1870, p. 72, n. 1, would have been exactly 5 feet 8 inches, or 1 metre 727!

[2] See p. 379, n. 3, *supra*.

[3] According to the method of M. Rollet, recommended by Dr. Garson (see p. 379, n. 3, *supra*), the average height of the fourteen Long Barrow skeletons the measurements of which are given in Tables I and II of *Crania Britannica*, would have been just under 5 feet 6⅛ inches, or about 1 metre 680.

[4] The skeletons from the Wor Barrow, referred to on p. 111, n. 3, *supra*, measured by M. Rollet's method, gave the following results:—5 ft. 9·4 in., 5 ft. 7·2 in., 5 ft. 1·9 in., 5 ft. 0·7 in., 4 ft. 11 in., and 4 ft. 10·2 in., or an average of 5 ft. 2·4 in. (A. Pitt-Rivers, *Excavations in Cranborne Chase*, vol. iv, one of unnumbered pages following p. 122).

[5] *Nature*, Jan. 13, 1898, p. 258; *Proc. Soc. Ant. Scot.*, xxix, 1895, pp. 412–3, 425, 436; xxxvi, 1902, p. 142; *Journ. Anthr. Inst.*, xxxii, 1902, p. 402.

[6] *Nature*, Nov. 22, 1894, p. 92.

[7] *Proc. Soc. Ant. Scot.*, xxxvi, 1902, pp. 147, 154–5, 161. The cephalic indices of such of these skulls as could be measured were 75·2, 70, and 66·6 (male), and 75 (female).

[8] *Ib.*, xxix, 1895, p. 436; *Nature*, Jan. 13, 1898, p. 258.

[9] *Nature*, Nov. 22, 1894, p. 92. Mr. C. S. Myers (*Journ. Anthr. Inst.*, xxvi, 1897, p. 123) makes some interesting remarks on 'the two [types of Long Barrow skulls] which Dr. Garson has been able to differentiate' (cf. *Wilts. Archaeol. and Nat. Hist. Mag.*, xxiii, 1887, p. 296): but for the purposes of ethnological investigation the doctor nevertheless places the two types in one group (*Journ. Anthr. Inst.*, xxii, 1893, pp. 13, 15–6).

[10] *Mem. Anthr. Soc.*, i, 1865, p. 154.

[11] *Crania Britannica*, ii, 1865, pl. 33, p. 6.

[12] *Ib.*, pl. 5.

the brow ridges are strongly marked, and the chin is comparatively broad and square.[1] Of another skull, found in a barrow near Littleton Drew in North Wiltshire, Thurnam observes that the lower jaw is 'thick and heavy'.[2] A third, taken from a barrow at West Kennet in North Wiltshire, has an amazingly angular and square lower jaw, which, as Thurnam truly says, 'deviates considerably from the normal type.'[3]

Again, while the average stature of the Long Barrow skeletons which Thurnam examined was, according to the higher estimate of Dr. Beddoe, only 5 feet 6·7 inches,[4] and Rolleston affirmed that he had 'never found the stature to exceed 5 feet 6 inches . . . in any skeleton from a barrow which was undoubtedly of the stone and bone period',[5] a skeleton found in the West Kennet barrow had a thigh bone 20 inches long;[6] and its possessor would therefore have stood 6 feet high, or nearly 1 metre 830, on the lowest computation, and, according to the estimate of Dr. Beddoe, 6 feet 1½ inch or 1 metre 867. Not less remarkable is a dolichocephalic skeleton of almost identical dimensions,[7] described by Dr. Garson, which, although it was found in a round barrow, undoubtedly belonged to the Neolithic Age.[8]

It is evident, therefore, that although not one of the people, so far as we can tell, who buried their dead in long barrows was brachycephalic in index, yet not only was there a very wide range in their indices, but some of them were strikingly different, both in form of skull and feature and in stature, from the normal type. Were they the result of crossing between individuals of the Long Barrow race and tall brachycephalic invaders who will be noticed later? Thurnam himself pointed out that a male skull, whose cephalic index was 79, found in a primary interment in the long barrow of Charlton Abbot's in Wiltshire, was 'unquestionably brachycephalous'.[9] The mere fact that its index was below the conventional limit did not blind him to its true character.

Let us now see how far those skulls of the Neolithic Age which have been found in other surroundings resemble the type which is associated with long barrows.

Putting aside the Scottish skulls which have been already mentioned, they comprise specimens found in the caves of Perthi-Chwareu in Denbighshire and Cefn, near St. Asaph; in a chambered cairn at Tyddyn Bleiddyn, near Cefn; in caves at Rhosdigre and Llandebie, and at Uphill in Somersetshire;[10] in the East Ham Marshes, along with two 'chipped celts', fifteen feet below the

[1] *Crania Britannica*, ii, pl. 5, p. 2.
[2] *Ib.*, pl. 24, p. 4.
[3] *Ib.*, pl. 50, p. 5.
[4] See p. 394, *supra*.
[5] *Brit. Barrows*, p. 654.
[6] *Crania Britannica*, ii, Table II. See also *Anthr. Rev.*, iii, 1865 (*Journ. Anthr. Soc.*, p. lxvii).
[7] The length of its thigh bone was 508 millimetres, or almost 20 inches.
[8] *Journ. Anthr. Inst.*, xxii, 1893, p. 9. See pp. 408–9, *infra*.
[9] *Archaeologia*, xlii, 1869, p. 222.
[10] W. Boyd Dawkins, *Cave Hunting*, pp. 155–87.

surface;[1] in the bed of the Trent at Muskham;[2] and in a submarine forest, thirty feet below the level of the sea, near the Land's End.[3] Skulls found in tumuli at Keiss in Caithness,[4] in a tumulus at Towyn-y-Capel in Anglesey,[5] and in 'what seems to be an alluvial deposit formed by the river Dove', near Ledbury Hall in Derbyshire,[6] may be added doubtfully to the list;[7] but, as we shall afterwards see,[8] there need be no doubt that certain brachycephalic skulls of the type which is commonly associated with the round barrows belonged to the Neolithic Age.

Professor Ripley[9] holds that the Long Barrow people were 'quite similar to' those whose remains have been found in caves, if 'somewhat less extreme in physical type'; and Huxley[10] thought that all the dolichocephalic and mesaticephalic British skulls of the Neolithic Age belonged to the same race. Similarly Dr. Garson[11] identifies the river-bed type, represented in Britain by the Muskham skull, with that of the long barrows; while, according to Professor Boyd Dawkins, the skulls from the Welsh caves and from Tyddyn Bleiddyn 'agree in shape . . . with some of those given in Tables i. and ii. of the "Crania Britannica" as "ancient British"',[12] and 'belong to that type which Professor Huxley terms the river-bed skull',[13] and which, according to him, was identical 'in general characters' with the Long Barrow type.[14] Dr. Beddoe,[15] on the contrary, says that both they and the Caithness skulls 'depart considerably from the typical long-barrow cranium', and is inclined to regard them as belonging to a distinct mesaticephalic race.[16] The cephalic indices of the Welsh skulls, which range from 74·3 to 80,[17] are considerably higher than those of the Long Barrow skulls in general; and (though

[1] *The Geologist*, v, 1862, pp. 213 4.

[2] *Trans. Ethn. Soc.*, N. S., i, 1861, p. 268; S. Laing and T. H. Huxley, *Prehist. Remains of Caithness*, p. 123, and figs. 48–51.

[3] W. Boyd Dawkins, *Cave Hunting*, p. 186; W. C. Borlase, *Dolmens of Ireland*, iii, 944.

[4] S. Laing and T. H. Huxley, *Prehist. Remains of Caithness*, pp. 83–103.

[5] *Ib.*, p. 120, and figs. 52–5; *Archaeol. Journal*, iii, 1846, pp. 223–8.

[6] S. Laing and T. H. Huxley, *Prehist. Remains of Caithness*, pp. 114–5.

[7] It is very doubtful whether the Caithness skulls were neolithic (*Proc. Soc. Ant. Scot.*, xxxvi, 1902, p. 160, n. 1).

[8] See pp. 408–9, *infra*.

[9] *The Races of Europe*, p. 306.

[10] *Prehist. Remains of Caithness*, pp. 128 30. Cf. *Mem. Anthr. Soc.*, iii, 1869, p. 63. Huxley argued that these skulls were also virtually identical with those of the Australian aborigines; but on another occasion, as we have seen already (p. 377, *supra*), when his combative instincts were aroused, he affirmed the contrary.

[11] *Nature*, Nov. 22, 1894, p. 92.

[12] *Journ. Ethn. Soc.*, N. S., ii, 1870, p. 449.

[13] *Ib.*, p. 444. Cf. *Fortnightly Rev.*, N. S., xvi, 1874, p. 336; and W. Boyd Dawkins, *Cave Hunting*, pp. 155, 159, 164, 185, 187.

[14] *Proc. Soc. Ant.*, 2nd ser., iii, 1864–7, p. 282.

[15] *The Races of Britain*, p. 13. See also p. 360.

[16] *L'Anthr.*, v, 1894, pp. 515–6.

[17] Boyd Dawkins, *Cave Hunting*, p. 171. The figures are 76, 75, 80, 79·7, 74·6, 79·4, 74 3 (Perthi-Chwareu); 77 (Cefn cave); and 76·5 (Tyddyn Bleiddyn).

this may be unimportant) the average height of the men to whom they belonged was 'little more than 5 feet',[1] or considerably below the average height of the Long Barrow people. In my opinion neither they nor the Land's End skull, which resembles them,[2] are pure specimens of the Long Barrow type; [3] and the same may be said of the East Ham and Muskham skulls. The one from Towyn-y-Capel, on the other hand, might be supposed to have come from a long barrow. The cephalic indices of the Caithness skulls range from 73 to 78. Four of them [4] might, I think, pass muster as Long Barrow skulls; but the remaining two [5] appear to me different. Of the Ledbury skull, the cephalic index of which is 77, Huxley himself says that 'a little flattening and elongation, with a rather greater development of the supraciliary ridges would convert this into the nearest likeness to the Neanderthal skull which has yet been discovered'.[6] It may be that there was some infusion of the blood of the Long Barrow race in all the people to whom these skulls belonged; but I have little doubt that if, with the few exceptions which I have noted, they were placed on a table among those of the long barrows, a skilled craniologist could pick out every one of them. The difference is easily accounted for when it is remembered that the long barrows were almost certainly erected late in the Neolithic Age,[7] and that there were neolithic men in Scotland when the estuary of the Forth extended 8 or 10 miles west of Stirling, and when the sea relatively to the west coast was 25 feet higher than it is now.[8]

A female skull, belonging apparently to the Neolithic Age, was discovered about the year 1891 'on the Batten promontory, near Plymouth Sound'.[9] According to the report of the discovery, it 'approaches dolichocephaly'. A photograph of this skull [10] re-

[1] *Ib.*, pp. 179, 187; *Journ. Ethn. Soc.*, N. S., ii, 1870, pp. 444–5, 460.

[2] W. Boyd Dawkins, *Cave Hunting*, p. 186.

[3] See the illustrations of the Perthi-Chwareu skulls in *Cave Hunting*, pp. 168–9.

[4] No. 7 (figs. 1–4, p. 84 of *Prehist. Remains of Caithness*); no. 2 (figs. 17–20, p. 90); no. 3 (figs. 34–8, p. 98); and no. 5 (figs. 39–43, p. 96).

[5] No. 8 (figs 9–12, p. 88); and no. 1 (figs. 25–8, p. 92).

[6] *Ib.*, p. 115.

[7] See *Proc. Soc. Ant. Scot.*, xxxvi, 1902, p. 162.

[8] *Nature*, Jan. 6, 1898, p. 235; R. Munro, *Prehist. Scotland*, pp. 58–9, 71. Professor Boyd Dawkins (*Archaeol. Journal*, liv, 1897, p. 338), speaking of the famous Cro-Magnon skeleton and of the gigantic skeleton without a skull, the discovery of which in the Paviland cave, Glamorganshire, was recorded in 1824 by Dean Buckland (*Reliquiae Diluvianae*, p. 82), says:—'In this group of remains so widely spread over Europe, we are on the track of a very early Prehistoric people, belonging to a tall, long-headed race, without the knowledge of pottery and without polished axes, if negative evidence be accepted . . . They are probably the advance-guard of the Neolithic migration . . . Further evidence is needed before we can define their precise relation to the Neolithic culture ordinarily so called.' Further evidence is also needed before we can affirm that the Paviland skeleton was neolithic at all (Sir J. Evans, *Anc. Stone Implements*, &c., 1897, p. 487).

[9] *Trans. Devon. Association*, xxiii, 1891, pp. 119–24.

[10] *Ib.*, facing p. 121.

minded me of some of the illustrations of round skulls in *Crania Britannica*. To quote from the report,[1] 'the most striking features of the face are the great size of the orbits, the strongly marked superciliary ridge, the lowness of the retreating forehead'; and all these features are characteristic of some of the most typical Bronze Age skulls.

A few years ago Professor Macalister said that he had not recognized any skulls of the Long Barrow type in Ireland,[2] where no such barrows exist; but several specimens have since been found.[3]

There is, as we have seen, reason to believe that the neolithic population of Britain were not homogeneous; but, with the qualifications that have been already noted, it may be truly said that the people of the long barrows present a uniform type. Whence did they come, and what were their affinities? The view which may be said to hold the field, although it is not universally accepted,[4] is that they belonged to the so-called 'Iberian' race. Before we discuss this theory, it may be well to warn the reader that among those who hold it are writers who have absolutely no knowledge of 'the Iberian question' except on the side of physical anthropology. The word 'Iberian', as used by ethnologists, is not always confined to the Iberians of history, that is, the inhabitants of the Spanish peninsula and of Southern Gaul between the Pyrenees and the Rhône:[5] it is often loosely applied to a people, possessing certain common physical features, who inhabited various parts of the Mediterranean basin, and, according to some writers, notably Sergi,[6] penetrated in late quaternary and neolithic times into almost every country of Europe. And when it is applied by ethnologists to the Iberians of history, it is not applied to all of them, for the Iberians of history were of course a mixed people: the ethnologists are thinking only of those Iberians who belonged to the dolichocephalic Mediterranean stock.

The arguments which have been brought forward in favour of the theory that the Long Barrow race belonged to the Iberian branch of the Mediterranean stock may be summarized as follows:— First, according to Tacitus,[7] the Silures, a British tribe which in his time inhabited what is now Monmouthshire, Glamorganshire, and Herefordshire, were dark and had curly hair, from which fact,

[1] *Trans. Devon. Association*, xxiii, 1891, p. 120.

[2] *Nature*, Nov. 22, 1894, p. 92.

[3] *Proc. Royal Irish Acad.*, 3rd ser., iv, 1896–8, pp. 570–85; vi, 1900–2, pp. 334–5; *Report of . . . the Brit. Association*, 1901, pp. 795–7.

[4] M. J. Deniker (*The Races of Man*, p. 313) even goes so far as to say that it is not yet certain whether the Long Barrow race immigrated from the Continent or were descended from the palaeolithic inhabitants of Britain!

[5] This definition may be accepted as true in a general sense, though it leaves out of account the Celtic inhabitants of the peninsula, whom Strabo loosely called Iberians; but see my *Caesar's Conquest of Gaul*, 1899, pp. 255–62, and cf. *Rev. des études anc.*, v, 1903, pp. 383–4.

[6] *The Mediterranean Race*, pp. 213–21, 225, 230, 244, 249, &c.

[7] *Agricola*, 11. Silurum colorati vultus, torti plerumque crines, et posita contra Hispania Iberos veteres traiecisse easque sedes occupasse fidem faciunt.

as well as from their geographical position, he inferred that Iberians, that is inhabitants of the Spanish peninsula, had migrated into Britain.[1] But if the dolichocephalic Iberians were dark, so were the brachycephalic people who settled in Gaul in the Neolithic Age: Tacitus's geographical argument was based upon the notion, prevalent among the ancient geographers,[2] that Spain was 'opposite' and near Britain; and it is of course incredible that people should have sailed in the Neolithic Age from Spain to our island.

Secondly, much stress has been laid upon the alleged resemblance of the Long Barrow skulls to those of the Basques, the assumption being that the latter were Iberians, properly so called. Dr. Garson affirms that there is 'a strong similarity between Basque skulls and those of the Neolithic people of Britain';[3] while Thurnam[4] points out that the skulls of the Basques are very 'similar in many respects to the skulls from chambered long barrows of South-West Britain', and that the Long Barrow skulls in general closely resemble 'sixty Basque skulls lately added to the collection of the Anthropological Society of Paris'.[5] Moreover, Dr. Beddoe[6] says, 'Many photographs of Basques . . . are recognized, both by myself and by an observant Welsh anthropologist to whom I have submitted them, as being in no respect different from some of the ordinary types of feature in South Wales.'

Now, as I have shown elsewhere,[7] the investigations which have been made regarding the cranial characters of the Basques have led to widely different results; and Dr. Garson does not say to what group of Basque skulls he refers. Both the Spanish and the French Basques, according to Dr. Collignon,[8] differ in certain respects from all other European peoples; but they also differ from each other, the former being generally dolichocephalic, while the latter are (according to Broca's notation)[9] sub-brachycephalic, and their cranial capacity is considerably less than that of their Spanish brethren. Dr. Collignon is inclined to assimilate the Basques generally to the Kabyle type.[10] Assuming that the Long Barrow

[1] M. d'Arbois de Jubainville formerly pointed out, in support of Tacitus's conclusion, that, according to Festus Avienus (*Ora maritima*, 433), there was a mountain in the Spanish peninsula called *Silurus* (*Les premiers habitants de l'Europe*, i, 1889, p. 44). But, since the origin of the name *Silures* is unknown, it seems rash to found an ethnological argument on its resemblance to *Silurus*. In Mexico there is a river called Tamesi: would M. d'Arbois infer from the name which Caesar latinized into *Tamesis* that the people who named this river were akin to the prehistoric inhabitants of Britain? M. d'Arbois has since argued that the Silures could not have been Iberian (*Les Celtes*, p. 30); but his recantation is hardly more reasonable than his original theory.

[2] Rice Holmes, *Caesar's Conquest of Gaul*, 1899, p. 480.

[3] *Nature*, Nov. 22, 1894, p. 92.

[4] *Crania Britannica*, ii, pl. 59, p. 5, note.

[5] *Mem. Anthr. Soc.*, i, 1865, p. 160.

[6] *The Races of Britain*, p. 26.

[7] *Caesar's Conquest of Gaul*, 1899, pp. 270–3.

[8] *L'Anthr.*, v, 1894, pp. 276–87.

[9] See p. 376, *supra*.

[10] See *Bull. de la Soc. d'anthr.*, 4e sér., vii, 1896, pp. 666–71.

race resembled the Spanish Basques in certain respects, the resemblance only tends to show that the ancestors of the Long Barrow race came from the south. The ancestors, or rather some of the ancestors of the Basques were undoubtedly Iberians,—in one sense of the word: but the French Basque type which Dr. Collignon has described, and which he regards as original,[1] is in many respects different from that of the long barrows, which ethnologists call Iberian; and, as I have shown elsewhere,[2] the purest French Basques are generally fair, the Spanish Basques are less dark than other Spaniards, and the Long Barrow race were undoubtedly dark. Moreover, many ethnologists overlook the fact that the language of the so-called Iberian inscriptions, which have been found scattered over the territory that belonged to the Iberians, cannot be interpreted by the aid of Basque,[3] and shows no trace of kinship with Basque.

Thirdly, Sergi, affirming that the Long Barrow people belonged to the Iberian branch of the stock which he has taught ethnologists to call Mediterranean, says,[4] 'I have compared the forms of the skulls from British graves with ancient . . . Mediterranean skulls, and have found those characteristic of Spain, of Portugal . . . of Greece, of Hissarlik, and of East Africa.' The fact is undeniable; but obviously it does not tend to prove that the Long Barrow race belonged to the Iberian rather than to the Ligurian branch, which, according to Sergi,[5] 'extends from the Iberian peninsula as far as Italy,' or to any other branch of the Mediterranean stock. Moreover, when Sergi affirms[6] that 'wherever the Mediterranean stock established itself, it preserved its primitive burial custom of inhumation', and that 'incineration was of absolutely Aryan [that is to say, on his theory, Asiatic] origin',[7] he weakens his argument,

[1] See also *Rev. mensuelle de l'École d'anthr.*, x, 1900, p. 214.

[2] *Caesar's Conquest of Gaul*, 1899, p. 271.

[3] *Ib.*, pp. 267–8. Professor Boyd Dawkins (*Early Man in Britain*, p. 334) argues that 'the identification of the Neolithic aborigines with . . . the modern Basques, is confirmed by' the fact that *aizcora*, the Basque word for an axe, means 'stone mounted in a handle': but how does this tend to establish the identity of the British neolithic aborigines with the Basques? It only shows that the ancestors of the Basques used stone tools.

Since I wrote the paragraph to which this note relates I have read M. G. Hervé's interesting article 'La race basque' (*Rev. mensuelle de l'École d'anthr.*, x, 1900, pp. 213–37), which confirms my conclusions. He holds (p. 220) that the Spanish Basques represent 'une race croisée, à la constitution de laquelle a pris part, en tant que facteur principal, la *race ibérique*, la vieille race de Baumes-Chaudes'. 'Il est clair', he adds (pp. 221–2), 'que les Hispano-Basques se différenciant des Gallo-Basques par tous leurs points de ressemblance avec les Ibères, les Gallo Basques ne peuvent à aucun titre être rattachés à ces derniers . . . les Ibères, en tout cas, n'ont joué qu'un rôle médiocre dans leur ethnogénie'. On pp. 235–7 M. Hervé offers certain tentative suggestions as to the origin of the Basques, whose purest representatives are the French Basques, and whose physical characters raise them, he considers, 'sans conteste au rang de quatrième race européenne'.

[4] *The Mediterranean Race*, pp. 206–7, 210. See also pp. 159–60, 182, 211–3 218–9, 269, 275.

[5] *Ib.*, p. 212. [6] *Ib.*, p. 269. [7] *Ib.*, p. 286.

for it is certain that incineration was practised by many of the Long Barrow people.[1] Furthermore, Sergi tells us that skeletons of 'the Mediterranean type' are characterized by 'slender and delicate forms',[2] and doubtless most of the skeletons which have been found in long barrows answer to this description; but thirteen skeletons found in a chambered long barrow at Rodmarton, Gloucestershire, were distinguished by 'powerful and vigorous frames'.[3]

I conclude that there is not sufficient evidence for referring the Long Barrow people to the Iberian rather than to some other branch of the Mediterranean stock.

It is generally admitted that the Long Barrow race closely resembled in cranial characteristics, and to a lesser degree in stature, the dolichocephalic neolithic population of Gaul, of whom the people whose remains have been discovered in the caverns of l'Homme Mort[4] and Baumes-Chaudes[5] were perhaps the most typical representatives; and this resemblance confirms the truth of the theory that the Long Barrow people were a branch of the 'Mediterranean' stock. But one argument, upon which Thurnam[6] laid great stress, should warn us to be cautious in drawing conclusions from the skeletal characters of prehistoric peoples of whose other characters we are necessarily ignorant. About the middle of the nineteenth century several skeletons were discovered in a neolithic barrow at Fontenay, near Caen. Their skulls resembled those of the long barrows; and the height of the tallest, according to Thurnam's system of measurement, would not have exceeded 5 feet 1 inch, or 1 metre 550. This, he triumphantly remarks, confirms the opinion that the peoples who erected the sepulchral chambers at Fontenay and in the south-west of England belonged to the same race. But the average height of the Long Barrow people, according to Thurnam, was 5 feet 5·4 inches,[7] and the average height of the brachycephalic Round Barrow people 5 feet 8·4 inches.[8] This difference of 3 inches

[1] See p. 110, *supra*. In France also incineration was common in the Neolithic Age (*Matériaux pour l'hist. . . . de l'homme*, xxii, 1888, pp. 1–2, 4, 6–7).

[2] *The Mediterranean Race*, p. 182.

[3] J. B. Davis and J. T. Thurnam, *Crania Britannica*, ii, pl. 59, p. 3. See also *Brit. Barrows*, pp. 127, 713–4.

[4] See Dr. Beddoe's article in *L'Anthr.*, v, 1894, p. 515; *Rev. mensuelle de l'École d'anthr.*, v, 1895, p. 171; Rice Holmes, *Caesar's Conquest of Gaul*, 1899, p. 251; and *Proc. Soc. Ant. Scot.*, xxxvi, 1902, pp. 161–2. Still, Rolleston points out (*Brit. Barrows*, p. 710) that 'the orbital index [the relation between the length and breadth of the socket of the eye], which does put . . . the Caverne de l'Homme Mort into a position of similarity to skulls such as those of the Tasmanian, Australian, and Melanesian races, puts the neolithic skulls of British Barrows into a position of superiority', &c.

The average height of the people of l'Homme Mort was, according to M. Rollet, 1 m. 578, or nearly 5 ft. $1\frac{8}{10}$ in.; according to M. Manouvrier, 1 m. 620, or nearly 5 ft. $2\frac{0}{10}$ in. (*Mém. de la Soc. d'anthr. de Paris*, 2e sér., iv, 1892, p. 388). See, however, p. 379, n. 3, *supra*.

[5] *Rev. mensuelle de l'École d'anthr.*, v, 1895, pp. 163–4. The cephalic indices of 35 Baumes-Chaudes skulls varied from 64·3 to 76·1.

[6] *Mem. Anthr. Soc.*, i, 1865, p. 160.

[7] See pp. 393–4, *supra*.

[8] *Mem. Anthr. Soc.*, iii, 1870, pp. 72–3.

is one of the facts upon which he relies to prove the distinction—a distinction which is of course as certain as it is universally admitted—between the Long Barrow people and the brachycephalic Round Barrow people. Yet he regards the difference of 4·4 inches between the average height of the Long Barrow people and the tallest of the men who were buried at Fontenay as sufficient to prove the racial identity of the latter with the former!

Dr. Keane[1] maintains that the route followed by the people who introduced the neolithic culture into the British Isles is indicated by the dolmens which abound in many parts of Northern Africa, and are scattered along the western side of the Spanish peninsula and over nearly the whole area of France. This is also the opinion of Professor Flinders Petrie, who affirms that 'the dolmens belong to one continuous series, passing from Syria, along North Africa, and up Spain to Western Europe',[2] of Montelius,[3] Sophus Müller,[4] and Sergi.[5] Penka, on the other hand (I quote from Mr. J. L. Myres's exposition of his views), 'reads the series the other way,' because 'while on the north these monuments go back into the Stone Age, in France and the south they belong to the Bronze Age'. He observes that 'the discovery of dolmens in North Africa and Syria . . . has proceeded *pari passu* with the discovery both of actual survival of a tall blond dolichocephalic race in the same areas, and of evidence in Egyptian portraiture of its wider extension in the second millennium B.C.' He maintains therefore that the earliest dolmen-builders were dolichocephalic blonds, speaking an Aryan language, in Southern Scandinavia and Denmark.[6]

Now the ethnological problem presented by the distribution of the dolmens is exceedingly difficult; and it is not certain that either of the above-mentioned views is right. Dolmens are found not only in the countries which have been already mentioned, but also in Japan, India, Persia, Arabia, Syria, Palestine, Moab, Asia Minor, the Crimea, the Netherlands, Northern Germany, and the Balearic Islands;[7] and it is possible that in certain other countries their non-existence may be due simply to lack of the necessary stones.[8] In the territory which corresponds with ancient Gaul there are no dolmens east of the line formed by the Jura and the Vosges;[9]

[1] *Ethnology*, 2nd ed., 1896, pp. 135–6.
[2] *Journ. Anthr. Inst.*, xxix, 1899, p. 308.
[3] *Man*, i, 1901, No. 88, p. 110.
[4] *Ib.*
[5] *The Mediterranean Race*, p. 70.
[6] *Man*, i, 1901, No. 88, p. 110. Cf. *Rev. d'anthr.*, ii, 1873, p. 113, and A. Bertrand, *Archéol. celt. et gaul.*, 1889, p. 173, *La religion des Gaulois*, p. 4.
[7] *Journ. Anthr. Inst.*, xxiv, 1895, pp. 316–30; A. H. Keane, *Ethnology*, 1896, p. 133; E. Cartailhac, *La France préhist.*, 1889, p. 186; W. C. Borlase, *Dolmens of Ireland*, ii, 476–566, 698–712, iii, 726–55.
[8] It is remarkable, however, that in Prussia there are no dolmens east of the Vistula, although stone is as abundant there as in West Prussia, where there are many. *L'Anthr.*, iv, 1893, p. 485.
[9] *Rev. d'anthr.*, ii, 1873, map facing p. 631; A. Bertrand, *La Gaule avant les Gaulois*, 1891, map facing p. 128.

while the departments in which they are most numerous form a band extending obliquely from Finistère to Gard, that is, from the Channel to the Mediterranean.[1] The single department of the Morbihan contains more megalithic monuments, including menhirs, or single standing stones, than all the other departments put together; but in the list of dolmens it ranks below Aveyron and Ardèche.[2] In the Spanish peninsula almost all the dolmens are concentrated in Portugal, the north-eastern corner of Spain, and the southern and eastern seaboard: in Southern Britain they are found in Cornwall, Devonshire, Dorsetshire, Somersetshire, Wiltshire, Oxfordshire, Berkshire, Kent, Derbyshire, Staffordshire, and Northumberland, in Monmouthshire,[3] Herefordshire, and Wales;[4] while in Scotland they are represented by the horned cairns of Caithness and the chambered cairns of Orkney, Inverness, Argyllshire, Arran, and other islands.[5] In Ireland they are everywhere, but most numerous in the west.[6]

There is a striking resemblance, which, in certain cases, amounts to almost complete identity of form, between many of the dolmens of Western Europe and some even of the Caucasus and India; although, as might have been expected, local peculiarities exist everywhere.[7] Thus the chambered long barrow of West Kennet in Wiltshire is identical in construction with the Hünebedden, or 'Giants' Graves', of Brandenburg, Mecklenburg, and Hanover;[8] and close resemblances have been noted between certain dolmens in Wales and others in Brittany and Portugal,[9] between some in Antrim and others in Denmark,[10] and between certain Irish dolmens and the peculiar ship-shaped monuments of the Balearic Isles.[11] It is of course true that in sepulchres of such rude and simple construction general resemblance is inevitable, and does not necessarily imply community of origin: but when we find that in the Caucasus, in Syria and India, and in every European country in which dolmens exist some few have one of their stones pierced with a hole;[12] that the covering-stones of certain dolmens in Portugal, Ireland, Cornwall, Sweden, and elsewhere are indented with small circular depressions,[13] and that the sepulchral customs discernible in the dol-

[1] *Rev. mensuelle de l'École d'anthr.*, xi, 1901, pp. 36–9. [2] *Ib.*, pp. 36–7, 43.

[3] It would seem that there was also a dolmen in a round barrow in Lancashire. See *Vict. Hist. of . . . Lancs*, i, 240. [4] See p. 66, n. 5, *supra*.

[5] Of dolmens in the narrower sense of the word (see p. 65, *supra*) only two, so far as I know, exist in Scotland. See *Proc. Soc. Ant. Scot.*, xix, 1885, p. 373, and W. C. Borlase, *Dolmens of Ireland*, ii, 424–9, 468.

[6] *Dict. des sc. anthr.*, 1883, p. 1079; E. Cartailhac, *La France préhist.*, 1889, p. 197; B. C. A. Windle, *Remains of the Prehist. Age*, pp. 195–7; W. C. Borlase, *Dolmens of Ireland*, ii, 449, 468, 610 2, 632–4, and the maps facing pp. 1, 102, 200, and 305 of vol. i.

[7] *Ib.*, ii, 445, 460, 463, 493, 501, 557, 567–8, 585, 612–3, 634, 670; iii, 723, 962. [8] *Ib.*, ii, 489–90; iii, 974, n. §.

[9] *Ib.*, ii, 450–1. [10] *Ib.*, pp. 495–6. [11] *Ib.*, p. 701.

[12] *Ib.*, iii, 723; A. Bertrand, *Arch. celt. et gaul.*, 1889, pp. 139, 141, 177. Cf. the remarks of M. Salomon Reinach in *La République Française*, 26 Sept., 1892.

[13] W. C. Borlase, *Dolmens of Ireland*, ii, 661 2; E. Cartailhac, *La France préhist.*, 1889, pp. 246–7.

mens of widely separated countries are virtually identical,[1] it must be admitted that there is ground for the opinion that the custom of dolmen-building originated with some one people.[2] On the other hand, it is easily conceivable that these coincidences originated in customs and beliefs which may have been common property before the first dolmen was set up, or which may have been handed on at a later time from tribe to tribe. De Mortillet argued that dolmens were not the exclusive creation of any one race because in France skeletons of widely different races have been found within them.[3] But this fact only proves that an intruding race buried their dead in dolmens built by others, or else adopted the custom of dolmen-building from their predecessors. It has also been argued that the differences in detail which are noticeable in the dolmens of the various countries of Western Europe prove that they were not the work of one migratory people but of various settled tribes; and that this conclusion is borne out by the similarity between the culture of the dolmen-builders of widely separated countries such as France and Denmark.[4] What is certain is that if the dolmens had been erected successively by peoples who migrated westward from Syria or even North Africa, and whose descendants moved on northward to the British Isles, we should expect to find that the British dolmens belonged to a period very much later than those of the Mediterranean. But the oldest dolmens of North Africa are assigned by General Faidherbe to the very end of the Neolithic and the commencement of the Bronze Age.[5] The arguments of Penka, whatever value they may have in regard to the origin of dolmen-building, certainly do not prove that the Long Barrow race were descended from Scandinavian ancestors: for their skulls are easily distinguishable from those of Scandinavia;[6] the neolithic

[1] *Proc. Soc. Ant. Scot.*, xxxvi, 1902, p. 164.

[2] It has been argued that there must have been a particular dolmen-building race, because certain countries, for instance Austria, which have been continuously inhabited from palaeolithic times, contain no dolmens. But this only proves that certain peoples did not build dolmens.

[3] *Dict. des sc. anthr.*, p. 388.

[4] *Ib.*, pp. 387–8; E. Cartailhac, *La France préhist.*, 1889, p. 199.

[5] W. C. Borlase, *Dolmens of Ireland*, iii, 714–5. See also Gen. Faidherbe's *Collection . . . des inscr. numidiques*, 1870, p. 13; *Matériaux pour l'hist. . . . de l'homme*, xxi, 1887, p. 190, pl. vi; and A. Bertrand, *Archéol. celt. et gaul.*, 1889, pp. 167–72.

[6] W. Greenwell, *Brit. Barrows*, pp. 482, 646, n. 1; *Man*, ii, 1902, No. 41, p. 51. According to Sergi (*The Mediterranean Race*, pp. 225, 249, 254, 259), the primitive Scandinavian dolichocephali were only one of the numerous branches of his ubiquitous 'Eurafrican species'. He insists that the modern Scandinavian 'cranial and skeletal facial forms' are identical with those of the Mediterranean race; and the tallness and fairness of the Scandinavians do not in the least shake his faith. 'Northern Europe,' he says (p. 254), 'has given origin to the white skin, blond hair, and blue or grey eyes' of the Scandinavians. Then why did it not produce the same phenomena among the Lapps and the 'Iberians' of the British Isles? See also *Rev. arch.*, 4e sér., iii, 1904, p. 153. I am of course willing to admit that the 'Iberian' and North European races were branches of the same primitive stock. See p. 434, n. 7, *infra*.

pottery of Britain is utterly different from that of the north;[1] and the distribution of the dolmens in the British Isles, where they are most numerous in Western Britain and in Ireland, is hardly consistent with the theory that the people who erected them came from the north-east. Moreover, the remains which have been found in the oldest Scandinavian dolmens indicate that the culture which they represent was more advanced than that which is manifested in similar tombs in Gaul or the British Isles.[2]

On the question of the origin of dolmens I offer no opinion. But in regard to those of Western Europe the least improbable theory appears to be that which was first tentatively propounded by M. Cazalis de Fondonce,[3] and developed by Mr. Borlase,[4] namely, that a dolichocephalic people who were erecting dolmens in France and the Spanish peninsula, where these monuments may have been evolved from sepulchral caves,[5] were forced westward by the brachycephalic 'Grenelle' race who invaded those countries in the Neolithic Age;[6] that some of them migrated into the British Isles, and others into Holland and Northern Germany,[7] whence the custom of dolmen-building would have spread to Denmark and Scandinavia; and that others [perhaps] moved southward into Africa. The earlier neolithic dolmen-builders of Gaul, like the Long Barrow people of Britain, belonged to the 'Mediterranean' type; and on the theory which I have stated their ancestors might have migrated into Spain and Gaul from Africa long before the first African dolmen was erected.

Lastly, linguistic arguments have been adduced to prove the African origin of the Long Barrow race.[8] Professor Morris Jones[9] endeavours to show that the Celtic language was modified, after

[1] See *Proc. Soc. Ant. Scot.*, xxxvi, 1902, p. 162.

[2] *Ib.*, pp. 163–4.

[3] *Rev. d'anthr.*, ii, 1873, p. 113.

[4] *Dolmens of Ireland*, ii, 610–2. Cf. *L'Anthr.*, iv, 1893, p. 731.

[5] *Congrès internat. d'anthr. et d'archéol. préhist.*, i, 1874 (1876), p. 253; E. Cartailhac, *Les âges préhist. de l'Espagne*, p. 328.

[6] See *ib.*, pp. 144–90, 316, 318, 325; *Crania Ethnica*, pp. 493–4; and *Rev. mensuelle de l'École d'anthr.*, v, 1895, pp. 155–81, 184, 407–13, ix, 1899, p. 278.

[7] M. Salomon Reinach (*L'Anthr.*, iv, 1893, pp. 485, 558) has expressed the opinion that of all European dolmens the most ancient are those of Northern Germany; but the only reason which he gives, namely, that this region is on the limit of the last moraines of the northern ice-sheet, and that the dolmens were constructed of 'erratic' rocks, does not seem worth discussing.

[8] Professor Zimmer (*Zeitschrift der Savigny-Stiftung für Rechtsgeschichte*, xv, 1894, pp. 217–8), while he denies that we are yet justified in saying that the language of the pre-Celtic [or, as I would say (see pp. 428–44, *infra*), the dolichocephalic pre-Celtic] inhabitants of the British Isles was Iberian, affirms that the linguistic evidence is sufficient to show that it was non-Aryan. Similarly Professor Rhys remarked at the meeting of the British Association in 1900 (*Report*, &c., p. 889) that there was 'probably no county in the kingdom that would be too small to supply a dozen or two [of names of streams] which would baffle the cleverest Aryan etymologist . . . and why? Because they belong in all probability to a non-Celtic, non-Aryan language.'

[9] J. Rhys and Brynmor Jones, *The Welsh People*, 1902, pp. 617–41.

it had been introduced into Britain, by the language or languages which it encountered;[1] and he claims to have established the syntactical similarity of the modern Celtic dialects to Egyptian and to the Hamitic dialects generally, and to have demonstrated that 'neo-Celtic syntax agrees with Hamitic in almost every point where it differs from Aryan'.[2] This, he concludes, is 'the linguistic complement of the anthropological evidence, and the strongest corroboration of the theory of the kinship of the early inhabitants of Britain to the North African white race'.

I would not, however, venture to commit myself to the theory of Sergi, that the cradle of the 'Iberian' race, or of the 'Mediterranean' race of which it was an offshoot, was in Northern Africa or Somaliland.[3] Professor Boyd Dawkins infers 'from their range as far north as Scotland, and at least as far to the east as Belgium, that they travelled by the same paths that the Celtic, Belgic, and Germanic tribes travelled . . . coming from the East, and pushing their way to the West; and that another [group] mastered Northern Africa'; and he argues that this view 'is confirmed by the examination of the domestic animals which they possessed. The short-horned ox, the sheep, and the goat, are derived from wild stocks that are now to be found only in Central Asia . . . None of these animals were known in Europe before the Neolithic Age.'[4] But any one who has read so far will have seen that the range of the 'Iberian' race 'as far north as Scotland' lends no support to the theory that it originated in Asia. In regard to the argument which the professor derives from the examination of the domestic animals, Rolleston[5] inclined to the view that 'though coming in the ultimate resort from the east, [they] . . . did not reach the north of the Alps directly from the East, but only . . . from the Greek and Italian peninsulas'. But the truth is that we do not know whether the *earliest* neolithic invaders of the British Isles or of Western Europe possessed short-horned oxen, sheep, or goats.[6] Supposing that these animals came from the East, is it not possible that they were

[1] May it not also have been modified, before it was introduced into Britain, by the non-Aryan language or languages which it presumably encountered on the Continent?

[2] *The Welsh People*, 1902, p. 618; *Celtic Review*, i, 1905, p. 279.

[3] *The Mediterranean Race*, pp. 42–4. Cf. *L'Anthr.*, v, 1894, p. 686, and *Man*, ii, 1902, No. 19, p. 28.

[4] *Fortnightly Rev.*, N. S., xvi, 1874, p. 336.

[5] *Brit. Barrows*, p. 741, note.

[6] After I had written these words, I was glad to learn that they had the support of Dr. Arthur Evans, who, speaking of the discoveries in the Mentone caves, says (*Journ. Anthr. Inst.*, xxii, 1893, p. 301) that 'it will no longer be allowable to say that these supposed immigrants from Asia brought with them at their first coming certain domestic animals, and had already attained a knowledge of the potter's art, and of the polishing of stone weapons'. And, as M. Salomon Reinach has justly remarked (*L'Anthr.*, vii, 1896, p. 687), in a criticism of the address which Dr. Evans delivered in 1896 at the meeting of the British Association, 'La race méditerranéenne s'offre d'abord à nos yeux dans une région [Mentone] d'où elle a pu fort bien gagner l'Afrique avant les modifications géologiques.'

introduced into Europe not by the 'Mediterranean' race but by brachycephalic neolithic immigrants? Moreover, Professor Boyd Dawkins has himself admitted that 'the common domestic hog, descended from the wild boar, may have been originally tamed in Europe',[1] and that the vegetables possessed by the Swiss lake-dwellers may have been 'derived from Southern Europe';[2] and it is now generally held that the domestic animals of the neolithic inhabitants of Europe were of European origin, and that there is no evidence that their plants and cereals were derived from Asia.[3]

On the whole the evidence shows that the neolithic inhabitants of Britain, or at all events a large proportion of them, were descended from ancestors who lived in the Mediterranean basin. But it does not follow that they were more intimately related to the people whom the ancient writers called Iberians than to some other branch of the Mediterranean stock. It is certain that before the Romans entered the Spanish peninsula two languages at least besides Celtic were spoken there,—Basque and the language of the so-called Iberian inscriptions.[4] The latter has not yet been deciphered: but, as we have seen,[5] all attempts to explain it by means of Basque have failed; and, as Professor Morris Jones admits, all attempts to discover traces of Basque influence in the Celtic dialects have been equally unsuccessful.[6]

Therefore it should be distinctly understood that if the term 'Iberian' is to be applied to the neolithic inhabitants of Britain, it must be taken in a purely conventional sense.[7]

M. d'Arbois de Jubainville[8] adduces various British place-names,

[1] *Early Man in Britain*, pp. 296–7. [2] *Ib.*, p. 302.

[3] *L'Anthr.*, iv, 1893, pp. 551–4; xvi, 1905, p. 187; *La Grande Encyclopédie*, xiv, 856; *Association franç. pour l'avancement des sc.*, 33e sessn., 1904 (1905), pp. 1034–49.

[4] Rice Holmes, *Caesar's Conquest of Gaul*, 1899, pp. 257–73.

[5] See p. 400, *supra*.

[6] The late Mr. Elton (*Origins of Eng. Hist.*, 1890, pp. 149–50) affirmed that certain customs, of which the couvade was one, had 'left distinct traces in the usages which still prevail in the region of the Pyrenees. But,' he continued, 'at present there seems to be no point of connection between them and anything which was ever observed in this country'; and he insisted that this 'should be taken into account by those who assert the identity of the Iberians with the Britons of the Silurian type'. I have not asserted that identity in the narrower sense in which Mr. Elton used the word 'Iberian': nevertheless his objection has no force. The answer to it is, first, that the couvade did survive in historical times, or leave traces of its former existence, in Ireland, Scotland, and Yorkshire (pp. 94–5, *supra*); secondly, that the custom prevails, or has prevailed, among peoples of every continent except Australia, who could never have influenced one another (*ib.*); and lastly, that it cannot be expected that widely scattered peoples who originally sprang from one stock should continue to preserve all the customs of their ancestors.

The other 'customs' of which Mr. Elton spoke are not worth mentioning. He simply affirmed that certain tribes who inhabited the Iberian peninsula in ancient times had different customs. Naturally. The fact in no way tends to prove that they did not belong to the same stock.

[7] Cf. *Rev. mensuelle de l'École d'anthr.*, x, 1900, p. 230.

[8] *Les premiers habitants de l'Europe*, ii, 1894, p. 213.

for example, *Sabrina* (the Severn), Isca (the Exe), *Albion*, and *Cantium* (Kent), which he chooses to call Ligurian; but I am not aware that he has made any converts. Little or nothing is known about the Ligurian tongue;[1] and even if M. d'Arbois's conjectures could be verified their ethnological value would be comparatively slight; for, as I have shown elsewhere,[2] there is some reason to believe that the Ligurians, like the Iberians, belonged to the 'Mediterranean' stock.

It is perhaps hardly necessary now to insist upon the fact that the Long Barrow race is not extinct. Not only have their remains been found, as we shall presently see, in graves of the Bronze Age and the Late Celtic period,[3] but men of the same type, but little modified, are still numerous.[4]

It is often taken for granted that no round barrows were erected in Britain before the close of the Neolithic Age, and that the earliest of the brachycephalic invaders whose remains have been found in them landed with bronze weapons in their hands.[5] But these assumptions are made in spite of conclusive evidence. There is not the slightest doubt that most if not all of the circular chambered cairns of Argyllshire, Caithness, Orkney, and Derbyshire were erected before the Bronze Age in those parts began.[6] Dr. Garson, speaking of brachycephalic skulls which have been found in round barrows in Orkney, says that 'the fact that no metals of any kind were found, and that all the implements were of the most primitive manufacture, points to the people belonging to the unpolished stone period', and concludes that 'we probably post-date the existence of the people who buried in the round barrows of Orkney if we attribute them with (*sic*) the same antiquity as those of the round barrows of England'.[7] Dr. Garson has also shown that the round barrow of Howe Hill in Yorkshire was erected in the Neolithic Age, and that the skeletons found in it belong to the Long Barrow type.[8] The round-headed people who introduced drinking-cups into our island brought no bronze with them. According to

[1] Rice Holmes, *Caesar's Conquest of Gaul*, 1899, pp. 276–7. See also *Rev. arch.*, 4e sér., i, 1903, pp. 65–6; *Rev. celt.*, xxx, 1904, p. 372; and p. 296, n. 4, *supra*.

[2] *Caesar's Conquest of Gaul*, 1899, pp. 280–1, 318.

[3] See pp. 426 and 434, *infra*; A. Pitt-Rivers, *Excavations in Cranborne Chase*, i, p. xv; *Archaeol. Journal*, lviii, 1901, p. 337; and *Journ. Anthr. Inst.*, xxvi, 1897, pp. 122–3, xxxiii, 1903, pp. 66–73.

[4] *Anthr. Rev.*, iv, 1866, p. 14; W. Greenwell, *Brit. Barrows*, pp. 630, 711; J. Beddoe, *The Races of Britain*, ch. v; A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 64; *Journ. Anthr. Inst.*, xxvi, 1897, pp. 88, 113; W. Z. Ripley, *The Races of Europe*, pp. 321–2, 326. Dr. Beddoe (*op. cit.*, p. 270) emphasizes 'the undoubted fact that the Gaelic and Iberian races of the west . . . are tending to swamp the blond Teuton of England by a reflux migration'. Cf. his paper in *Journ. Anthr. Inst.*, xxxv, 1905, p. 235, and Addenda, p. 740.

[5] See, for example, Prof. Boyd Dawkins's article in *Archaeol. Cambr.*, 5th ser., viii, 1891, p. 72; and cf. p. 129, n. 2, *supra*.

[6] See pp. 107–8, *supra*.

[7] *Journ. Anthr. Inst.*, xiii, 1884, pp. 83–4. See also *Anthr. Rev.*, iv, 1866, p. 99.

[8] *Journ. Anthr. Inst.*, xxii, 1893, pp. 11, 15–6, 18.

Barnard Davis, a skull from a chambered round barrow at Parsley Hay Low in Derbyshire, which had a cephalic index of 81, 'without doubt belongs to the early "stone-period"';[1] and he assigns to the same epoch another skull, the cephalic index of which was the same, from Green Gate Hill barrow, Pickering, Yorkshire.[2] Canon Greenwell suggests that some of the round barrows 'belong to an age before bronze was discovered'; and it is certain that the round barrows of this country were connected by evolution with the earlier long barrows.[3] Finally, if Sergi[4] is right in maintaining that 'the new burial custom of cremation' was introduced into Europe by brachycephalic immigrants, it follows that they invaded Britain in the Neolithic Age; for in this country, as in Gaul, cremation was then practised.[5]

VII. THE 'PICTISH QUESTION'

A view which has become fashionable of late years, owing to the influence of Professor Rhys and Professor Zimmer, is that the [dolichocephalic] neolithic people of this country were identical with the Picts,[6] whose name first occurs in the panegyric addressed about A.D. 296 to Constantius Caesar.[7] To clear the ground, I should say, first, that it is universally admitted that descendants of the neolithic race survived not only in the part of Scotland which was inhabited by the Picts but in most parts of Britain. The question is whether the Picts represented that race in a special sense, and still spoke the neolithic non-Aryan language. As we shall see, Professor Rhys himself, who maintains that they did, emphatically affirms that among the medley of tribes who were known as Picts some were Celtic and spoke a Celtic tongue. Secondly, it may be well to state certain elementary facts of Celtic phonology (although I dare say that to most of those who may read these pages they are already familiar), without a knowledge of which parts of the following discussion and of the later section on the Celts would be unintelligible. The ancient Gauls, for the most part,[8] and the Brythons, from whose dialect modern Welsh is descended, are commonly called the P Celts; while the Goidels, whose dialect was the ancestor of Gaelic, Irish, and Manx, are known as the Q Celts.

[1] *Crania Britannica*, ii, pl. 2, pp. 1–2.

[2] *Ib.*, pl. 3 and 4, p. 1.

[3] *Brit. Barrows*, pp. 131, 450, 480, note; *Journ. Roy. United Service Inst.*, xiii. 1870, pp. 522–3; Sir J. Evans, *Anc. Stone Implements*, 1897, p. 148.

[4] *The Mediterranean Race*, p. 263.

[5] See p. 110, *supra*.

[6] 'The most tenable hypothesis may be said to be that the Picts were non-Aryans, whom the first Celtic migrations found already settled here . . . the Picts were the descendants of the Aborigines' (*The Welsh People*, 1902, pp. 13–4).

[7] *Incerti Pan. Constantio Caesari*, c. 11 (XII *Panegyrici Latini* recensuit Aemilius Baehrens, 1874).

[8] See pp. 410, 438, n. 3, *infra*.

The reason of this distinction is that the Gauls and Brythons changed the original sound *qu* into *p*, while the Goidels retained it, and in the sixth century of our era modified it into *c*.[1] It has been affirmed, however, on the evidence of the formularies of Marcellus of Bordeaux, that some of the Western Gauls in the fourth century spoke a dialect which was akin to Goidelic;[2] and Professor Rhys and Mr. Nicholson[3] regard the words *Sequani* and *Sequana* (the Gallic name of the Seine) as proving that this dialect was not confined to the west: but M. d'Arbois de Jubainville refuses to admit that these names are Celtic,[4] and contemptuously denies that the formularies are to be taken seriously.[5] Professor Rhys[6] and Mr. Nicholson[7] also infer from certain inscriptions found in the departments of the Ain and Deux-Sèvres, which probably belong respectively to the first and the fourth century of our era, that a dialect akin to Goidelic was spoken in those localities: but here again M. d'Arbois dissents;[8] and he remarks that an inscription found at Géligneux in the department of the Ain contains a word, *petru-decametos*,[9] which belongs to the language of the P Celts. Professor Rhys urges that 'the presence of monuments in the language occupying the subordinate position may be taken as evidence presumptive of its being the vernacular in the immediate neighbourhood':[10] but, as we shall see hereafter,[11] a pillar, bearing a Goidelic inscription, has been found at Silchester, where the vernacular was undoubtedly Brythonic; and the obvious explanation is that the inscription was the work of a stranger. M. d'Arbois,[12] moreover, unlike Professor Rhys, maintains that when the Celts first invaded Britain, the Celtic language everywhere was one and the same: according to him, none of the Celts had then changed *q* into *p*, but that change was made at a later date by the Celts who conquered Gaul, and some of whose descendants afterwards conquered Britain. Until near the end of the nineteenth century Celtic scholars unanimously believed that all the Celtic dialects had rejected 'Indo-European *p*', except, as Mr. Nicholson says,[13] 'in borrowed words or in certain combinations of consonants':

[1] See J. Rhys, *Celtic Britain*, 1904, pp. 215–6. Similarly the Latins retained *qu*, as in *equus*, while the Greeks, as in ἵππος, changed it into *p*.

[2] Rice Holmes, *Caesar's Conquest of Gaul*, 1899, p. 299, note; E. W. B. Nicholson, *Keltic Researches*, 1904, pp. 6–7.

[3] *Ib.*, pp. 6, 128, 149, 167.

[4] *Rev. celt.*, xi, 1890, p. 377; xx, 1899, pp. 108–9. In the latest volume of his review (xxvii, 1906, p. 107) M. d'Arbois reiterates his dissent, asking whether *Britain*, *Thames*, and *London* are words of Anglo Saxon origin.

[5] *Ib.*, xxv, 1904, pp. 351–3; xxvii, 1906, pp. 107 8.

[6] *Celtae and Galli*, 1905. See especially pp. 1–2, 46, 55 64. Professor Rhys (*ib.*, pp. 48–50) somewhat doubtfully regards two other inscriptions, which have been found near Bourges and near Evreux, as akin to Goidelic.

[7] *Keltic Researches*, pp. 116–53.

[8] *Rev. celt.*, xxv, 1904, pp. 351–3; xxvii, 1906, p. 107.

[9] *Corpus inscr. Lat.*, xiii, 2494.

[10] *Celtae and Galli*, p. 62.

[11] See pp. 451–2, *infra*.

[12] *Les premiers habitants de l'Europe*, ii, 1894, pp. 255–82; *Les Celtes*, pp. 17–9.

[13] *Keltic Researches*, p. 127.

in other words, that wherever the Indo-European or Aryan tongue from which Celtic was descended had the sound of *p* the Celtic dialects had all lost it: but Professor Rhys holds that Mr. Nicholson has proved from the above-mentioned inscriptions, found in the departments of the Ain and Deux Sèvres, that it was retained by the Sequani and the Pictones.[1] M. d'Arbois de Jubainville of course rejects this conclusion; and he reminds his opponents that *p* is absent from all Ogam inscriptions.[2]

1. In 296, when the panegyric addressed to Constantius was written, the Picts to whom the writer referred were confined to the part of Scotland which extends northward from the firths of Forth and Clyde; but Professor Rhys and Professor Zimmer maintain that the habitat of the Pictish people was once much more extensive. 'Irish literature,' says Professor Rhys,[3] 'alludes to Picts here and there in Ireland . . . in such a way as to favour the belief that they were survivals of a race holding possession at one time of the whole country.' That the Picts once inhabited the whole of Britain is proved, in the opinion not only of the two professors but also of M. d'Arbois de Jubainville, who differs from them on the question of Pictish ethnology, by the following linguistic facts.[4] The Irish name of the Picts was *Cruthni*.[5] Britain has, since the Middle Ages, been called in Welsh *ynys Prydein*: *Prydein* is the Welsh equivalent of *Cruthni*; and *ynys Prydein* means 'the island of the Picts'. Now, as Professor Rhys remarks,[6] *Prydein*, with its cognate forms, *Prydain*, *Prydyn*, and *Pryden*, represents an old Welsh word *Priten*; and accordingly, the Brythonic or the Gaulish name of the Picts, when it reached the ears of the Greeks, would have been written by them Πρεταν(ν)οί. It must of course be borne in mind that *Cruthni*, *Prydain*, and *Priten* did not appear in literature until long after Caesar's time; but the etymology which connects Πρεταν(ν)οί and Πρεταν(ν)ικαὶ (νῆσοι)—the name by which Ptolemy and other Greek writers call the British Isles[7]—with *Priten* is accepted by Celtic scholars who, on the question of the ethnology of the Picts, differ widely among themselves. M. d'Arbois de Jubainville[8] concludes that in the time of Pytheas the masters of Britain were the Picts; while Professor Rhys holds that when, shortly before that epoch, the Brythons first landed in Britain,[9] not the Picts but the Goidelic

[1] *Report of . . . the Brit. Association*, 1900, p. 895.

[2] *Rev. celt.*, xxvii, 1906, pp. 107–8.

[3] *The Welsh People*, 1902, p. 13. Cf. Zimmer in *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte*, xv, 1894, pp. 214, 215, n. 1.

[4] *Ib.*, pp. 215–6; J. Rhys, *Celtic Folk-Lore, Welsh and Manx*, p. 281; *The Welsh People*, 1902, p. 76; H. d'Arbois de Jubainville, *Principaux auteurs de l'ant. à consulter sur l'hist. des Celtes*, pp. 69–70.

[5] The forms *Cruithni* and *Cruithnig* were also used. See Dr. Whitley Stokes's article in A. Bezzenberger's *Beiträge zur Kunde der indogermanischen Sprachen*, xviii, 1892, pp. 84–5, and J. Rhys, *Celtic Britain*, 1904, pp. 241–2.

[6] *The Welsh People*, 1902, p. 76. Cf. *Scottish Review*, xviii, 1891, pp. 133–8.

[7] See pp. 459–61, *infra*.

[8] *Les premiers habitants de l'Europe*, i, 1889, p. 45, n. 2; ii. 1894, pp. 282–3; *Rev. celt.*, xiii, 1892, pp. 399–400; *Les Celtes*, p. 25.

[9] *Celtic Britain*, 1904, p. 4.

Celts were the dominant race. In other words, he believes that the Goidelic Celts called the island which they conquered by 'some such a Goidelic name as Inis Chruithni, "Island of the Picts"'.[1] M. d'Arbois identifies the Picts of the time of Pytheas with the ancestors of the Goidelic Celts: like Professor Rhys he regards the word *Pretani* as simply the Brythonic, or Gaulish form of a Goidelic word *Qrtanoi*, of which *Cruthni* was the later Irish equivalent;[2] but he holds that no Brythons had set foot in Britain until after the time of Pytheas, and that the word *Pretani* was learned by Pytheas not in Britain but in Gaul.

Both the views that have just been stated seem to involve difficulties. If Professor Rhys is right in believing that the pre-Roman Goidelic invaders of Britain (whose very existence, as we shall afterwards see, is not universally admitted) called the people whom they found in possession by some such name as Chruithni or Cruthni, the name which, transformed by Brythons into *Pretanoi*, was applied by Pytheas to the inhabitants of Britain generally, it would appear either that the Goidelic invaders had no name of their own or that it was suppressed.[3] Moreover, Professor Rhys does not explain how it happened that Pytheas never learned the name by which, as he tells us, the Brythons called themselves, namely, Brittones. On the other hand, M. d'Arbois's view would compel us either to assign the first Brythonic invasion to a date a century later than that which is now generally accepted,[4] or to assume that Pytheas, although he visited Britain, learned nothing there of the name of its inhabitants. I confess that I cannot suggest any satisfactory solution.

It remains to be inquired whether the Picts of history did really, in a special sense, represent the neolithic population, and whether they spoke a non-Aryan language.

2. Was the word *Pict*, in its original form, pre-Aryan or Celtic? The answers that have been given to this question only serve to amuse the ignorant scoffer, and to illustrate the truth that even if the labours of Zeuss placed the study of the ancient Celtic languages upon a scientific basis, Celtic scholars still know very little about them. When we inquire of Professor Rhys, we are perplexed by the quick changes of front to which his most devoted disciples have by this time become accustomed. In the second edition of his *Celtic Britain*[5] he said that 'neither the Picts nor the Scotti probably owned these names, the former of which is to be traced to Roman

[1] *Scottish Review*, xviii, 1891, pp. 134–5.

[2] See pp. 418–9, *infra*. M. d'Arbois's latest pronouncement (*Les Druides*, pp. 35–6, n. 5) is that '*Cruithne* est le même mot que Πρεττανία, le nom que prit la Grande-Bretagne avant de s'appeler' Πρεττανία, &c.

[3] Prof. Rhys's suggestion (*The Welsh People*, 1902, p. 114) that 'the word Dumnonii [which (see p. 447, *infra*) was the name of a Brythonic tribe] was a collective name of the Goidels of Britain when the Brythons arrived' may be taken for what it is worth.

[4] See p. 234, *supra*.

[5] p. 239.

authors'; and he described the theory which 'connected the Pict with the Gaulish Pictones' as a 'clumsy invention'.[1] In his Rhind Lectures he assured us that 'the principal non-Aryan name of the inhabitants of both islands [Great Britain and Ireland] was some prototype of the word Pict',[2] and gave reasons, which are now generally accepted, for believing that that name was not connected with the Latin *pictus*.[3] At the same time he definitely committed himself to the view which he had previously derided as a 'clumsy invention', and affirmed that 'the word Pict . . . is hardly to be severed from the Pictones of ancient Gaul'. In *The Welsh People*, which first appeared in 1900, and in a later edition of the same work, dated 1902,[4] he argued that '*Ictis* [the name of an island mentioned by Diodorus Siculus[5]] and *Icht* [the old Irish name of the English Channel] represent possibly a Celtic pronunciation of the same Aboriginal word which the Romans made into *Pictus* . . . we must', he added, 'suppose it an early name which the Aborigines adopted, while the Celts . . . applied another name *Qurtani*, *Pretani*, *Cruithni*,' &c. But in the same year in which the first edition of *The Welsh People* appeared he told the members of the British Association that '*pictos* was a Celtican word of the same etymology, and approximately, doubtless, of the same meaning as the Latin *pictus*; that the Celticans had applied it at an early date to the Picts on account of their . . . tattooing themselves; and that the Picts had accepted it'.[6] It is not absolutely clear whether by 'the Celticans' he means only those people of Gaul who spoke a language akin to Goidelic or the first Celtic invaders of Britain. As, however, we are told that the Picts accepted their name from 'the Celticans', it would seem that those 'Celticans' were, or at all events included, the British Goidels; and we ask ourselves in bewilderment why, if the 'Celticans' applied the name *pictos* to the Picts, they also applied the name *Qurtani*.[7] But when we open the latest edition of *Celtic Britain*,[8] we find that the professor's views are still in process of development, or of flux. He now reverts to the theory that 'the native name which suggested the Latin [*Pictus*] was not of Celtic origin either, though only found treated as Celtic'. He adds that 'the term Pictones, as occurring in Gaul in Caesar's time, makes it probable that it was also a name of long standing in Britain'; and finally he avows with characteristic candour that 'we know not from what language it comes'. Turning to our other authorities, we learn from Zimmer that *Picti* is obviously a Latin translation of the name [the ancestor of *Prydain*] which the Romans learned from the Britons.[9] In other words, the German *savant* holds that the word Pictos [if it ever existed except as a

[1] p. 243.
[2] *Scottish Review*, xviii, 1891, p. 142.
[3] *Ib.*, p. 124.
[4] pp. 78–9.
[5] See pp. 499–507, *infra*.
[6] *Report of . . . the Brit. Association*, 1900, p. 895.
[7] *The Welsh People*, 1902, pp. 78–9.
[8] pp. 311–3.
[9] *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte*, xv, 1894, pp. 213–4.

Latin accusative plural] was neither aboriginal in Britain, nor Celtican. It has been suggested[1] that *Picti* is connected with the old Irish word *cicht*,[2] a carver or engraver, and is the Cymric form of a Goidelic word *Qicti*;[3] while Mr. Nicholson, who insists that *Picti* is not Cymric but Goidelic, claims to have 'fully shown that this name is . . . from the root *peik-* "tattoo", with Ind.-Eur. *p* preserved'.[4]

The one absolutely certain conclusion to which the student of ethnology can come is that the name of the Picts has not been proved to be of pre-Aryan origin.

3. Still, Professors Rhys and Zimmer will have it that the Picts must have been a non-Aryan people. Caesar,[5] in a well-known passage, states that among the Britons groups of ten or twelve men had wives in common; in other words, that one of the British customs was polyandry. It has generally been assumed that he meant to say that the custom was prevalent among the Britons generally; but Zimmer, after reviewing the whole chapter in which the passage occurs, concludes that it refers only to *interiores*—the Britons of the interior[6]—whom Caesar contrasts with *maritimi*,—the descendants of the Belgic invaders. The latter, he argues, according to Caesar's express statement, differed but slightly in their customs from the Gauls:[7] therefore the words in which Caesar describes the British custom of polyandry cannot refer to them, but must refer to *interiores*.[8] The two professors agree in thinking that Caesar, owing to his 'inability to realize a state of society exclusively based on birth',[9] misunderstood the institution which he tried to describe; in other words, that that institution was not polyandry but matriarchy,—the rule of succession by which rank and property are transmitted in the female line; a king, for example, being succeeded not by his own son but by the son of one of his sisters.[10] Zimmer, referring to Schrader's *Prehistoric Antiquities of*

[1] E. Windisch in *Allgemeine Encyklopädie der Wissenschaften*, &c., 35. Theil, 1884, p. 136; A. Holder, *Altceltischer Sprachschatz*, ii, 993; E. W. B. Nicholson, *Keltic Researches*, p. 5.

[2] *Cormac's Glossary*, ed. Whitley Stokes, 1868, p. 40.

[3] See the remarks of M. d'A. de Jubainville (*Les Celtes*, p. 22), who regards the *p* in *Picti* as a trace of the Belgic invasion, and Prof. Rhys's *Celtic Britain*, 1904, pp. 311–2.

[4] *Keltic Researches*, pp. 32, 147–50.

[5] *B. G.*, v, 14, §§ 4–5.

[6] See p. 267, *supra*.

[7] *B. G.*, v, 14, § 1.—Ex his omnibus longe sunt humanissimi qui Cantium incolunt . . . neque multum a Gallica differunt consuetudine.

[8] *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte*, xv, 1894, pp. 224–5.

[9] J. Rhys, *The Welsh People*, 1902, pp. 36–7. Cf. *Celtic Britain*, 1904, pp. 35–6.

[10] Dr. F. B. Jevons (*Journal of Philology*, xvi, 1888, p. 104), remarking that 'the Joint Undivided Family persisted in Sparta long after it had disappeared in the rest of Greece', and that 'Polybius, misunderstanding the practice, was led to imagine, where brothers lived on the joint estate, and one alone had a wife, that the wife was common to all the brothers', says (*ib.*, n. 1) that 'precisely the same mistake, due to the same cause . . . is made by Caesar

the Aryan Peoples,[1] remarks that among all Aryan-speaking peoples and among the primitive Aryans the custom by which a father is succeeded by his own son (*das Vaterrecht*) was the foundation of social ordinance.[2] Professor Rhys,[3] indeed, thinks that this generalization cannot be proved, and refers to a well-known passage in the 20th chapter of the *Germania* of Tacitus,—'Sisters' sons are held in as much esteem by their uncles as by their fathers: indeed, some regard the relation as even more sacred and binding',[4] &c. (*Sororum filiis idem apud avunculum qui apud patrem honor: quidam sanctiorem artioremque hunc nexum sanguinis arbitrantur*); but he suggests that the tribe of which Tacitus speaks may have been mixed with some 'aboriginal race practising the same institution as the aborigines of the British Isles'. And I suggest that the Picts were Celts mixed with aborigines who practised this same institution, and consequently that if it prevailed among the Picts, its prevalence does not prove that they were in any special sense representatives of the aborigines, or that they spoke a non-Aryan language.[5]

Having corrected Caesar's narrative to his own satisfaction, Professor Rhys sets himself to prove that matriarchy was a Pictish institution. He observes[6] that 'a Pictish king [during the later period of the Roman occupation and afterwards] could not be succeeded by a son of his own, but usually by a sister's son. The succession,' he continues, 'was through the mother, and it points back to a state of society which, previous to the conversion of the Picts to Christianity, was probably based on matriarchy as distinguished from marriage and marital custom.' To show that matriarchy had formerly prevailed in Britain outside the territory within which the Picts of history were confined, he adds[7] that 'the ancient literature of Ireland abounds in allusions to heroes who are usually described with the aid of the mother's name', and that 'this kind of nomenclature implies the Pictish succession as its origin'. Again, he quotes an inscription found at Colchester, which ends with the words

DONVM. LOSSIO. VEDA. DE SVO
POSVIT. NEPOS. VEPOGENI. CALEDO.

('This gift has been dedicated at his own expense by Lossio Veda,

when he ascribes polyandry to the ancient Britons'. M. d'Arbois Jubainville, however (*Rev. celt.*, xxv, 1904, pp. 188–9), referring to *Ancient Laws of Ireland* (*Senchus Mor*), ed. W. N. Hancock, i, 122, l. 19, 126, l. 4, 142, l. 30, concludes that 'en Irlande, à une époque reculée, la communauté des femmes entre frères a existé d'une façon générale'. The editor (p. 143) does not share this view.

[1] pp. 369–80.

[2] *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte*, xv, 1894, p. 234.

[3] *The Welsh People*, 1902, pp. 61–2.

[4] I quote from the translation of Messrs. Church and Brodribb.

[5] I find that M. J. Loth (*Annales de Bretagne*, vi, 1890–1, p. 113) has made a suggestion which is substantially the same.

[6] *The Welsh People*, 1902, p. 14.

[7] *Ib.*, pp. 14–5.

the son of the sister [?] of Vepogen, a Caledonian'), and remarks that when Lossio calls himself a Caledonian, that 'is for our purpose much the same as if he had called himself a Pict', and that, moreover, both *Veda* and *Vepogeni* 'may be said to occur in the list of Pictish kings', where the latter is 'written *Vipoig*'. *Vepogeni*, indeed, is a Celtic word, borrowed, the professor assures us, in accordance with Pictish custom; but 'the reduction of *Vepogen* to *Vepog*, which is what underlies *Vipoig*, is impossible on Celtic ground . . . while Pictish offers a simple and natural explanation'.[1]

Professor Morris Jones remarks, in support of Professor Rhys's argument, that 'the Pictish succession' has 'come down to our own times among the Berbers'[2] (or rather Kabyles), who, he says, have been shown, on craniological grounds, to be akin to our neolithic race.

Apparently Professor Rhys does not regard the custom of reckoning descent 'by birth alone' as confined in these islands to the Picts, or to the pre-Aryan aborigines: if, as he is inclined, like Professor Zimmer, to believe, it was non-Aryan, 'it must,' he says, 'have been accepted by the Goidelic Celts from the aborigines.'[3]

Now, in regard to this last observation, the comment suggests itself that what Professor Rhys has not yet proved is that those aborigines were Picts. The Picts, as we shall presently see, were, according to some Celtic scholars, themselves Goidelic Celts (mixed of course with aborigines whom they had subdued and Celticized); according to others, their speech was akin to Brythonic.[4] And if, as Professor Rhys insists, matriarchy may have been accepted by the Celts from the aborigines, it is perhaps not incredible that, as Mr. Sidney Hartland suggests, the Celts themselves, in prehistoric times, may have passed through the matriarchal stage,[5] and that the survival of matriarchy among the Picts is not necessarily attributable to pre-Aryan ancestry.[6] But, be that as it may, the sur-

[1] J. Rhys, *The Welsh People*, 1902, pp. 45–7. Cf. *Proc. Soc. Ant. Scot.*, xxxii, 1898, pp. 324-98, and especially 324–30; also *Archaeol. Cambr.*, 5th ser., viii, 1891, pp. 29–32.

[2] J. Rhys, *The Welsh People*, pp. 640–1. Professor Jones refers to A. Hanoteau, *Essai de grammaire de la langue tamachek*, 1860, p. xv.

[3] *The Welsh People*, 1902, pp. 61–2.

[4] See p. 422, *infra*.

[5] *Archaeol. Cambr.*, 6th ser., ii, 1902, p. 59. Cf. J. G. Frazer, *Early Hist. of the Kingship*, pp. 229–46.

[6] M. d'Arbois de Jubainville (*Rev. celt.*, xxii, 1901, p. 122) gives various instances from history to show that the 'Pictish succession' did not imply matriarchy. 'Julius Caesar,' he says, 'chose as his heir Octavius, his sister's grandson: was this matriarchy? Tiberius was the stepson of Augustus: was this matriarchy? When a king had to be chosen among the Picts, the son of the late king's sister may sometimes have been preferred to his own son; but the sister's son must often have been the elder and more experienced of the two.' And so on (see also vol. xxiii, 1902, p. 359, vol. xxv, 1904, p. 206, and *Rev. arch.*, 4e sér., v, 1905, p. 447). But the point is that during the time for which the history of the Picts is known to us a Pictish king was never once succeeded by his own son. M. d'Arbois de Jubainville's arguments are not required for the purpose of demonstrating that the 'Pictish succession' does not prove the Picts to have been *the* representatives of the neolithic aborigines.

vival of matriarchy among the Picts proves nothing more than that among the Picts, as among every other British people, the substratum of the population was pre-Aryan: it does not prove that the dominant element among them was pre-Aryan, or that they spoke a non-Aryan language.

As for Professor Morris Jones's argument, it may perhaps raise a probability that the 'Pictish succession' prevailed among the neolithic race, although, if the argument is worth anything, the professor ought to be able to show that the same institution belonged to the 'Iberians' of Spain, of Gaul, and of other countries who have also been shown 'on craniological grounds' to be akin to the Kabyles: but at all events it lends no support to the theory that the Picts were, in any special sense, descendants of the neolithic aborigines; for, assuming that they were Celts, they might have accepted the Pictish succession from them. There remains Professor Rhys's statement that 'the reduction of *Vepogen* to *Vepog*, which is what underlies *Vipoig*, is impossible on Celtic ground'. Is the professor quite sure? A few years ago he would certainly have said that the retention of 'Indo-European *p*' was 'impossible on Celtic ground'; but in 1900 he announced that the 'Celtican language' which was spoken in the country of the Sequani 'preserves intact the Aryan consonant *p*'.[1] He has himself assured us that both the Celtic dialects spoken in the British Isles were greatly modified by a pre-Aryan language.[2] Assuming, for the sake of argument, that the Pictish language was Celtic, is he prepared to deny that it could have been so far modified by a non-Aryan tongue that 'the reduction of *Vepogen* to *Vepog*' would still have been 'impossible on Celtic ground'?[3] Finally, when he tells us that Lossio's description of himself as a Caledonian 'is for our purpose much the same as if he had called himself a Pict', we cannot help recalling his own statement[4] that 'the Caledonians were, as we understand their history, Goidels'; though, to be sure, in the latest edition of *Celtic Britain*[5] he expunges this compromising sentence, and substitutes for it 'the Caledonians were Picts'.

For my part I accept the professor's emendation unreservedly. Picts the Caledonians certainly were; for does not the author of the panegyric addressed to Constantine speak of 'the Caledonians and other Picts'?[6] But for me the Picts were a mixed people, comprising descendants of the neolithic aborigines, of the Round Barrow race, and of the Celtic invaders,—a mixed people who

[1] *Report of . . . the Brit. Association*, 1900, p. 895.

[2] *Ib.*, p. 896.

[3] Mr. Nicholson (*Keltic Researches*, pp. 144, 174) offers one explanation of *Vipoig*, and Dr. Macbain (W. F. Skene, *The Highlanders of Scotland*, 1902, pp. 394–5) another.

[4] *Celtic Britain*, 1884, p. 222. See also p. 153.

[5] p. 224.

[6] *Incerti Pan. Constantino Augusto*, c. 7 (published in XII *Panegyrici Latini* recensuit Aemilius Baehrens).—Caledonum aliorumque Pictorum silvas, &c. For the manuscript reading Baehrens, following Eyssenhardt, needlessly substitutes (Caledonum,) *Pictorum aliorumque*.

spoke a Celtic dialect. And what puzzles me is that the professor should not have been struck by the anthropological facts that are fatal to the theory that the Caledonians were Picts in the sense which he attaches to the word,—that is, pure survivors of the neolithic aborigines, who spoke a non-Aryan language. For the neolithic aborigines, as we have seen, were, speaking generally, small dark men of the 'Iberian' type: the Caledonians were big fair or red-haired men. Doubtless there were, as I have said, 'Iberian' survivors among them; but who will deny that the powerful race whom Tacitus describes were predominant, or that their Aryan tongue had prevailed?[1]

4. It is usually inferred from statements in Claudian[2] and Herodian[3] that the Picts tattooed themselves; and their testimony is supposed to be strengthened by the etymology of the names by which the Picts were known to the Irish and Welsh respectively,—*Cruthni* and *Prydain*. The former is said to be derived from *cruth*,[4] the Gaelic word for 'form' or 'shape'; and the latter from its Welsh equivalent, *pryd*.[5] Thus *Cruthni* and *Prydain* would mean

[1] The view, advocated by W. C. Borlase (*Dolmens of Ireland*, iii, 1042–3) and others, that the Caledonians were Germans is hardly worth discussing. There is absolutely no evidence for it, except the remark of Tacitus (*Agricola*, 11) that 'the red hair and large limbs of the inhabitants of Caledonia indicate a German origin'; and everybody knows that the physical characters of the Germans and the Celts, as described by the ancient writers, were virtually identical (see my *Caesar's Conquest of Gaul*, 1899, pp. 309–10, and *Rev. mensuelle de l'École d'anthr.*, vii, 1897, pp. 74, 89). It is possible that some of the Caledonians may have been descended from immigrants who came from Germany; but this, I need hardly say, would be quite consistent with the view that they were a Celtic-speaking people.

[2] *De bello Gothico*, 416–8. —

> Venit et extremis legio praetenta Britannis,
> Quae Scotto dat frena truci ferroque notatas
> Perlegit exanimes Picto moriente figuras.

[3] iii, 14, § 7.—*τὰ δὲ σώματα στίζονται γραφαῖς ποικίλαις καὶ ζώων παντοδαπῶν εἰκόσιν· ὅθεν οὐδ' ἀμφιέννυνται ἵνα μὴ σκέπωσι τοῦ σώματος τὰς γραφάς.*

[4] M. d'A. de Jubainville (*Rev. celt.*, xiii, 1892, p. 401, n. 1) rejects this derivation.

[5] Those who are familiar with Professor Rhys's writings will not be surprised to find that his notion of the meaning of these words is unstable. In 1884 he wrote (*Celtic Britain*, p. 240), 'These words Cruithni and Prydyn are derived from *cruth* and *pryd* respectively, which mean form'; and he added that 'Duald MacFirbis, quoted by Todd in a note on the Irish version of Nennius, p. vi,' 'has rightly explained the former [Cruithni] as meaning a people who painted the forms (crotha) of beasts, birds, and fishes on their faces, and . . . on the whole of the body. This,' he observed, 'agrees well enough with Claudian's vivid description of Stilicho's soldiery, scanning the figures punctured with iron on the body of the fallen Pict,' &c. In 1891 he threw both MacFirbis and Claudian overboard: 'We are not warranted,' he said (*Scottish Review*, xviii, 1891, p. 124), 'in supposing that he [Claudian] drew his inspiration from any deeper source than the popular etymology of the name Pictus, interpreted as a Latin word.' He went on to say (p. 131) that the silence of Gildas, who hated the Picts, 'is proof positive that neither Picts nor Scots were in the habit of discolouring their skins to any greater extent than his own people'; and he insisted that there was a grave objection to the explanation given by MacFirbis, 'namely, that it accounts for too few of the elements of the word Cruithne.' In 1900 (*Report of . . . the Brit. Association*, p. 895) he

'the people whose bodies were decorated with figures'; and, as we have seen, Zimmer has no doubt that the Roman name for the Picts—*Picti*, or 'painted men'—was simply a translation of *Prydain* or its older equivalent. Professor Rhys, who, in one of his many and diverse utterances on the subject, affirmed that *pictos* was a Celtican word,[1] drew this conclusion from the fact, pointed out by Mr. Nicholson,[2] that a coin of the Gallic tribe of the Pictones[3] bears on the obverse a tattooed face; and he supposes that the reason why the Celticans applied this word to the Picts was that the latter tattooed themselves. 'The Picts of Britain and Ireland,' he remarks, 'are found also called *Pictones*'; and 'ancient Egyptian monuments represent the Libyans of North Africa with their bodies tattooed'.[4]

Now what does this community of custom prove about the ethnology of the Picts? The inhabitants of the Tonga and Society Islands and of New Guinea tattoo themselves: so do the Burmese, the Shans, the Maoris, and the people of British East Africa;[5] so do very many Englishmen. All the available evidence tends to show that among the ancient inhabitants of the British Isles tattooing was not confined to the Picts. Herodian does not mention the Picts at all: he merely says that the Britons tattooed themselves. Professor Rhys admits, or rather strenuously maintains, that in the territory inhabited by the Picts in Scotland there were also numerous Celts;[6] and he would hardly deny that they were included among the people whom Herodian describes. He himself remarks that 'the Scotti (that is to say the Goidels)'[7] practised tattooing.[8] Mr. Nicholson, to whom he appeals, argues from the evidence of

brushed aside the 'proof positive', and proclaimed his conviction that, after all, the Picts really had tattooed themselves. In 1902 (*The Welsh People*, pp. 79–80, n. 2) he observed that if *Cruithni* and *Prydyn* had been really derived from *cruth* and *pryd*, 'one could scarcely avoid treating *Cruithni* and *Prydyn* as translations . . . of the word *Pict* regarded as the Latin *pictus*, "painted"'; and that 'the supposition here suggested as to *Pretani* being merely a sort of translation of . . . *pictus* would compel us to regard the first use of *Pretani* as dating no earlier than Caesar's time', which, as he truly remarks, chronology will hardly allow us to do. In the 3rd edition of *Celtic Britain*, 1904, p. 242, he reverts to his view of 1884.

Candour is a virtue; but how are we to follow a guide who is for ever changing his mind?

[1] See p. 413, *supra*.

[2] *The Language of the Continental Picts*, 1900, pp. 22, 26.

[3] E. Muret and M. A. Chabouillet, *Cat. des monn. gaul. de la Bibl. Nat.*, 4439.

[4] *Report of . . . the Brit. Association*, 1900, pp. 889–90. Cf. Rhys's *Celtic Folklore, Welsh and Manx*, pp. 681–2.

[5] A. H. Keane, *Man, Past and Present*, 1899, pp. 138, 198–9; *Journ. Anthr. Inst.*, xxxiv, 1904, p. 255; xxxv, 1905, pp. 283–94; *L'Anthr.*, xvi, 1905, p. 129; *Man*, v, 1905, No. 53, pp. 86–7; vi, 1906, No. 4, pp. 6–9. Needless to say, tattooing is practised by many other peoples besides those mentioned in the text.

[6] *Celtic Britain*, 1904, pp. 94–5, 162, 184, &c. On the last-named page, for instance, among the 'nations of Pictland' are included 'the Verturian Brythons'.

[7] *Ib.*, p. 275. [8] *Ib.*, pp. 241, 245.

coins that tattooing was customary not only among the Pictones, but also among several other tribes of Gaul,—the Ambiani, the Baiocasses, the Caletes, the Coriosopites, the Osismi, the Sequani, and the Unelli. All these peoples were undoubtedly Celtic; that is to say, they were Celtic-speaking tribes among whom the Celtic element, ethnologically speaking, was, I do not say numerically, but politically predominant. Professor Rhys would certainly not argue that they were Picts: yet if he admits, as he does, that they were Celtic, the argument which he bases on the practice of tattooing collapses.

5. Some years ago Professor Rhys attempted to prove that the Pictish language was related to Basque;[1] 'but,' he says, 'whether it is related or not, my attempt to prove that it is has been pronounced, and doubtless justly pronounced, a failure.'[2] At the same time, however, pointing to a famous ogam inscription, he wrote, 'my challenge still remains, that if Pictish resembled Gaelic or Welsh, or in fact any Aryan language, those who think so should make good their opinion by giving us a translation of such an inscription, for instance, as the following from Lunasting, in Shetland:—*Xttocuhetts : ahehhttmnnn : hccvvevv : nehhtonn.*'[3].

The lay reader will perhaps mentally endorse the comment of another Celtic scholar, Dr. Alexander Macbain, who disposes of the cacophonous puzzle by observing that 'it is neither Welsh nor any other language'.[4] For the present, at all events, it is safe to say that Dr. Macbain is as likely to be right as Mr. Nicholson, who, having boldly accepted Professor Rhys's challenge, first judiciously reconstructed the text of the inscription, and then made an heroic attempt to translate his own version. It is Goidelic, so he assures us; and it means

> 'Place of O' Cuhetts
> his place within:
> CUAIBH of Nehton'.[5]

On the other hand, the translation which Professor Rhys 'provisionally' offers of *his* text runs

> '"Kin—Ahehhtmnnn King Nechtan".

That is to say, King Nechtan of the kin of Ahehhtmnnn'.[6]

Perhaps it shows a slight lack of humour to attempt, even 'provisionally', to translate an inscription assumed to be written in a language the very existence of which is doubtful. Still it is conceivable that Professor Rhys's text means what he says. But, supposing that it resembles neither Gaelic, nor Welsh, nor any Aryan language, what does it prove? Not that the Picts repre-

[1] *Proc. Soc. Ant. Scot.*, xxvi, 1892, pp. 263–351.
[2] *Ib.*, xxxii, 1898, p. 324.
[3] *Ib.* See also *The Welsh People*, 1902, p. 16.
[4] W. F. Skene, *The Highlanders of Scotland*, 1902, p. 398.
[5] *Keltic Researches*, pp. 71–3.
[6] *Proc. Soc. Ant. Scot.*, xxxii, 1898, p. 374.

sented the neolithic aborigines, but simply that in the remotest of the British isles there still survived the non-Aryan language which, as every scholar admits, was once spoken in Britain.

But the truth is that the so-called Pictish inscriptions, even in the hands of the philologist, are so intractable that for ethnology they are practically useless. 'I can hardly do more,' says Professor Rhys,[1] 'than pick from previous attempts by others and by myself what seems to me the most probable reading.' This is only one of numerous instances in his well-known article on the inscriptions which show how impossible it is to construct the text with any approach towards certainty.

Professor Rhys remarks, further,[2] that 'we have indications in Adamnan's Life of Columba that [in the sixth century of our era] the language of the aborigines was still a living tongue'. The indications are that when Columba, who spoke Goidelic, visited the province of the Picts, he preached 'to peasants or plebeians by interpreter'. To those who hold, with Dr. Whitley Stokes and Dr. Macbain, that the Pictish dialect was akin to Brythonic, the fact on which Professor Rhys lays stress presents of course no difficulty. M. d'Arbois de Jubainville, however, while he agrees with Dr. Macbain,[3] makes a reply to Professor Rhys which might be used by those who hold, with Mr. Nicholson, that Pictish was akin to Goidelic. He tells a story of a Breton priest of the diocese of Quimper who assured him that he himself could not understand the Breton dialect of a woman who belonged to the diocese of Vannes.[4]

Mr. Nicholson[5] says that 'we have abundant materials for deciding whether Pictish was or was not (1) Aryan, (2) Keltic, (3) Goidelic, in (*a*) the place-names recorded by ancient geographers and one or two mediaeval documents, (*b*) the person-names given by one or two ancient historians and in mediaeval chronicles, (*c*) the inscriptions'. From these materials Mr. Nicholson undertakes to demonstrate that Pictish was Goidelic, and that 'it stands to Highland Gaelic in exactly the same relation in which Anglo-Saxon stands to modern English';[6] while Dr. Whitley Stokes[7] and Dr. Macbain[8] undertake with equal confidence to demonstrate that it was related to Brythonic. According to Bede,[9] the place which marked the western termination of the wall of Severus was called in Pictish *Peanfahel*. *Pean* is commonly identified with the Welsh word *penn*, 'a head'; and accordingly it has been inferred that Pictish was 'a Kymric or semi-Kymric dialect'.[10]

[1] *Ib.*, p. 361. [2] *Celtic Britain*, 1904, p. 272. [3] *Les Celtes*, p. 30.
[4] *Rev. celt.*, vii, 1886, p. 181. [5] *Keltic Researches*, p. 24. [6] *Ib.*, p. 21.
[7] A. Bezzenberger, *Beiträge zur Kunde der indogermanischen Sprachen*, xviii, 1892, pp. 84–115, and especially 113–4.
[8] W. F. Skene, *The Highlanders of Scotland*, 1902, pp. 381–401.
[9] *Hist. eccl.*, i, 12.—Incipit autem duorum ferme milium spatio a monasterio Aebbercurnig ad occidentem, in loco qui sermone Pictorum Peanfahel, lingua autem Anglorum Penneltun appellatur, &c.
[10] E. W. B. Nicholson, *Keltic Researches*, pp. 4, 21. Cf. A. Bezzenberger, *Beiträge*, &c., xviii, 1892, pp. 98, 108.

Mr. Nicholson, on the other hand, claims to have shown that *Pean* is 'a Goidelic borrowing from the Latin *penna* or *pinna*'. Professor Rhys[1] formerly clung to the view that *Peanfahel* was a Brythonic name, but was not in the least disconcerted thereby; for, he explained, 'the Picts must have learnt it . . . from the Verturian Brythons.' On the question of etymology he has now become a convert to Mr. Nicholson's view:[2] but on the question of ethnology he retains his own opinion; for, he explains, 'The non-Celtic Picts, when we find them coming southwards, seem to have been fast adopting the idioms of their neighbours.'[3] Mr. Nicholson[4] analyses with laborious ingenuity a large number of names in Adamnan's *Life of Columba*, of place-names in the *Pictish Chronicle*, of Pictish historical names, and of words which occur in the 'Pictish inscriptions', and insists that they are Goidelic: Dr. Whitley Stokes[5] and Dr. Macbain[6] produce words from the same sources, from Ptolemy's Geography, and from Dion Cassius, and insist that they are Brythonic. Dr. Stokes's authority is so great that his verdict is worth quoting:—'The foregoing list of names and other words contains much that is still obscure; but on the whole it shows that Pictish, so far as regards its vocabulary, is an Indo-European and especially Celtic speech. Its phonetics, so far as we can ascertain them, resemble those of Welsh rather than of Irish.'[7]

But the arguments for Brythonic, on the one hand, and for Goi-

[1] *Celtic Britain*, 1884, p. 153.

[2] *Ib.*, 1904, pp. 153–4. Referring to p. 24 of Mr. Nicholson's book, Professor Rhys says (*Celtic Britain*, 1904, p. 153) that *Peanfahel* 'points back to a Latin term *penna*(*e*) or *pinna*(*e*) *valli*, "wing of the vallum," that is, the pinnacle or turret at the end of the wall'. Now 'pinnacle or' appears to have been inserted in order to save the face of *pinna*. Does the professor mean 'a pinnacle', or does he mean a 'turret'? And if he means 'a turret', in what sense does he use the word? A pinnacle would have been a feature far too insignificant to give rise to a place-name; and a turret would have been equally insignificant unless it was a defensive tower, in which case it would have been called not *pinna* but *turris*. Professor Haverfield (*Archaeologia*, lv, 1897, p. 196) speaks of 'the corner turret' of the fort of Aesica on the Roman wall about midway between Newcastle and Carlisle. Read his description of it, and you will appreciate the absurdity of calling it a *pinna*, and the still greater absurdity of the supposition that even a 'corner turret' could beget a geographical name. There is no authority for the use of the word *pinna* in connexion with a defensive wall, except in the sense of 'pinnacle', in which sense it is used twice by Caesar (*B. G.*, v, 40, § 6; vii, 72, § 4). The *pinnae* which he describes were merely small pinnacles rising from a breastwork on an earthen rampart, breastwork and pinnacles forming a battlement, and both being made of wattlework (*pinnae loricaeque ex cratibus contexuntur* [*B. G.*, v, 40, § 6]. See also C. E. C. Schneider's note in his edition of Caesar, vol. ii, p. 565). The notion that the geographical name *Peanfahel* 'points back' to a *pinna* is too ridiculous to be discussed. Why not be content with Dr. Stokes's etymology in Bezzenberger's *Beiträge*, xviii, 1902, pp. 98, 108?

[3] *Celtic Britain*, 1904, p. 154. Cf. *Rev. celt.*, vi, 1883–5, p. 398.

[4] *Keltic Researches*, pp. 33–80.

[5] A. Bezzenberger's *Beiträge zur Kunde der indogermanischen Sprachen*, xviii, 1892, pp. 84–115.

[6] W. F. Skene's *Highlanders of Scotland*, 1902, pp. 387–401.

[7] Bezzenberger's *Beiträge*, &c., xviii, 1892, pp. 113–4. M. J. Loth (*Annales de Bretagne*, vi, 1890–1, p. 115) is substantially in agreement with Dr. Stokes.

delic, on the other, leave Professor Rhys unmoved. Prove as many Pictish words as you please to have been Goidelic, as many as you please to have been Brythonic: he will regard them with serene indifference.[1] For, he tells you,[2] 'the Pictish language would seem to have been rapidly becoming overloaded with loan-words from Goidelic or Brythonic when we first hear anything about it. So, failing to recognize this borrowing of words by the Picts, some have been led to regard Pictish as a kind of Gaelic, and some as a dialect akin to Welsh. The point to have been decided, however, was not whether Gaelic or Welsh explains certain words said to have been in use among the Picts, but whether there does not remain a residue to which neither Gaelic nor Welsh, nor, indeed, any Aryan tongue whatever can supply any sort of key.' The professor is still thinking of that outlandish inscription which, according to Mr. Nicholson, is Goidelic, and the professor's reading of which, according to Dr. Macbain, is no language at all. But, admitting provisionally the existence of 'a residue' to which no Aryan language 'can supply any sort of key', we should, I must repeat, only have to conclude that in certain remote parts of the extensive territory occupied by the Picts a non-Aryan language survived into the Christian era, just as in a remote part of France a non-Aryan language survives at this day: we should not have to conclude that that language was spoken by the Picts in general. 'La question,' says M. d'Arbois de Jubainville, in a notice of Professor Rhys's article on the Pictish inscriptions,[3] 'la question est de savoir si cette population [the pre-Aryan population] est restée dominante. Les noms de peuples tels que *Smertae* . . . des noms d'hommes tels que celui du Calédonien *Argentocoxos* . . . me semblent décisifs.' It is absolutely certain, and is insisted upon by Professor Rhys himself, that in Roman times many of the tribes which were included under the general designation of Picts bore Celtic names, and that many of the geographical names in the country which they inhabited were Celtic also. On the other hand, not a single Pictish name, tribal, or geographical, or personal, not a single Pictish word which has been preserved by Ptolemy or by our other authorities, has been proved to be non-Celtic; and if, as Professor Rhys maintains, Pictish was a non-Aryan language overlain by loan-words from the two Celtic dialects, it was so buried beneath them as to be no longer discernible. Argentocoxos,[4] as the professor says, was a Pict, and one of the many Picts whose names were Celtic: if the Picts had spoken a non-Celtic language, however much overloaded with Celtic loan-words, would not their own names have been non-Aryan? As their names were Celtic, it is reasonable to infer that their language was Celtic also. The professor, it is true, points out that 'in Wales many a man has the English name John Jones,

[1] I find that my criticism has been anticipated by M. J. Loth (*ib.*, p. 114).
[2] *The Welsh People*, 1902, pp. 15–6.
[3] *Rev. celt.*, xx, 1899, p. 390.
[4] *Proc. Soc. Ant. Scot.*, xxxii, 1898, p. 398.

though he cannot speak English'.[1] Yes, but the Welsh are a conquered or, let us say, absorbed people, whereas the professor himself assures us[2] that before the time of Ptolemy 'the Goidels and the Picto-Brythons [of the North] had come under the power of the more purely non-Celtic tribes beyond them'.[3] But this is of course a pure assertion. The professor fails to prove that any Celtic people in Britain came under the power of non-Celtic tribes. Many centuries before the time of Pytheas the neolithic population had for the most part been reduced to subjection; and, although remote clans may possibly have retained their individuality, in many parts of the island the descendants of the aborigines had become intermingled, first with the 'Round Barrow' invaders, the earlier of whom at all events, as I shall presently show,[4] were not Celts, and secondly with the Celts themselves. Professor Rhys[5] himself admits that the name of the Picts 'was never, perhaps, distinctive of race, as Brythons and Goidels seem to have been sometimes included under it'; and, although he goes on to say that 'the term probably applied most strictly at all times' to 'the non-Celtic natives', it is not likely that the name of non-Celtic natives should have prevailed over that of the Celts.

For all these reasons it appears to me infinitely more probable that in Pictland as, according to Professor Rhys himself, in the rest of Britain,[6] the non-Aryan language should have been absorbed by Celtic than that Celtic should have been absorbed by the non-Aryan language.

There is probably this grain of truth in Professor Rhys's theory, that the non-Celtic natives continued to exist in greater purity in the country which was occupied by a group of tribes who, during the latter part of the Roman occupation and afterwards, were called Picts, than in any other part of Britain. But I doubt whether this eminent scholar could have spent his time less profitably than in striving to demonstrate, first, that the language of the Picts was related to Basque, and, when he was forced to abandon this attempt, in clinging to the theory that it was a non-Aryan tongue.

VIII. THE ROUND-HEADS

There is, as we have already seen,[7] sufficient evidence that round-headed immigrants had begun to appear in Britain towards the end of the Neolithic Age; but the majority of the prehistoric skulls of this kind undoubtedly belong to the Age of Bronze. Men of the

[1] *Celtic Britain*, 1904, p. 275.
[2] *Ib.*, p. 165.
[3] I am glad to find that I have been anticipated by M. d'A. de Jubainville (*Rev. celt.*, vii, 1886, p. 381). Replying to Professor Rhys's argument, which appeared also in the earlier edition of *Celtic Britain*, he remarked that 'l'usage des vaincus est de copier les noms propres des vainqueurs'.
[4] See pp. 429-40, *infra*. [5] *Celtic Britain*, 1904, p. 160.
[6] See, for instance, *The Welsh People*, 1902, p. 19. [7] See pp. 408-9, *supra*.

same type were living in England at the time of the Saxon invasion ; [1] and their descendants may be recognized here and there at the present day.[2] The prehistoric skeletons have been found not only in the round barrows of Yorkshire, Derbyshire, Staffordshire, Wiltshire, Dorsetshire, Denbighshire, Man, and Orkney, and in secondary interments in long barrows, but also in Welsh caverns and graves and in the short cists of Scotland.[3] The range of this people in Britain was, however, it need hardly be said, far wider than that which the discovery of a few skeletons has indicated.

The round-headed invaders are commonly described as physically finer men than the neolithic population whom in most parts of Britain they subdued ; [4] but the truth is that, both in respect of stature and of cranial form, they belonged to two utterly different groups, though, as might be expected, some exhibit characteristics of both.[5] The average height of 17 brachycephalic men whose skeletons had been found in round barrows before 1865 would have been, according to Dr. Beddoe's estimate, 5 feet 9 inches, or almost 1 metre 753 ; while the average height of 27 men of various cephalic indices, including the 17 just mentioned, whose skeletons (described in *Crania Britannica*) have been found in round barrows, would, according to the same authority, have reached 5 feet $9\frac{2}{5}$ inches,[6] or approximately 1 metre 763. Measurements of

[1] W. Greenwell, *Brit. Barrows*, p. 713.

[2] *Nature*, Jan. 13, 1898, p. 258 ; W. Z. Ripley, *The Races of Europe*, p. 309.

[3] D. Wilson, *Prehist. Annals of Scotland*, i, 1863, pp. 268–75 ; *Anthr. Rev.*, iii, 1865, p. 76 ; *Crania Britannica*, ii, Tables i and ii ; *Mem. Anthr. Soc.*, iii, 1870, p. 52 ; *Proc. Soc. Ant. Scot.*, xix, 1885, pp. 39–41 ; xxxvi, 1902, pp. 157–9 ; xxxviii, 1904, p. 81 ; *Journ. Anthr. Inst.*, xxvi, 1897, pp. 96–7 ; xxxii, 1902, pp. 402–3 ; *Nature*, Jan. 13, 1898, p. 258 ; *Archaeol. Journal*, lviii, 1901, pp. 330–8 ; *Archaeol. Cambr.*, 6th ser., ii, 1902, p. 31.

[4] Thirty years ago, however, Mr. J. R. Mortimer (*Journ. Anthr. Inst.*, vi, 1877, pp. 328–34) said, 'My computation of the stature of these two types of men—the brachycephalic and the dolichocephalic—is the very reverse of' Thurnam's ; and he stated that of ten skeletons found in round barrows of the Yorkshire Wolds between Driffield and Aldoborough, five, of which the cephalic indices ranged from 70 to 75, averaged 5 ft. $9\frac{3}{5}$ in. in height, while five others, the indices of which ranged from 79 to 94, averaged only 5 ft. 5 in. The barrows, however, although no bronze was found in them, contained not only 'drinking-cups' but also 'food-vessels' (*Anthropologia*, i, 1873–5, pp. x–xi) ; and it may be concluded that they belonged to the Bronze Age. Rolleston (*Brit. Barrows*, p. 654, n. 2) was therefore justified in presuming that the tall dolichocephali who were buried in them belonged to 'a mixed race' ; and, he said, 'the effect of crossing . . . is very usually to increase the size of the mixed races.' Still, the low stature of Mr. Mortimer's brachycephali is remarkable ; and we shall see that they belonged to a distinct race, of which other examples have since been exhumed.

[5] *Proc. Soc. Ant. Scot.*, xxxix, 1905, pp. 437–8.

[6] *Journ. Anthr. Inst.*, xvii, 1888, p. 209. Dr. Beddoe's figures are not absolutely correct. The measurements of the thigh bones of the twenty-seven skeletons to which he refers are given in Tables I and II of *Crania Britannica*. They do not include the Arras skeleton, mentioned in Table I, which belonged to the Early Iron Age. The average height of the seventeen brachycephali, calculated by Dr. Beddoe's method, would have been just over 5 ft. $9\frac{1}{4}$ in. (1 m. 758) ; of the twenty-seven mixed skeletons, 5 ft. $9\frac{11}{5}$ in. (within a very

skeletons which have been discovered since the publication of *Crania Britannica* have yielded results virtually the same.[1] On the other hand two groups of skeletons have recently been described which belonged to a much shorter race. Four, taken from round barrows in Glamorganshire, showed, according to Dr. Beddoe's method, an average height of about 5 feet 5¾ inches ;[2] while 7 male skeletons, found in short cists in and near Aberdeenshire, ranged, according to Mr. Alexander Low, between 5 feet and 5 feet 7 inches, the average being only 5 feet 3 inches.[3] The skulls of these skeletons will be presently described.

The cephalic indices of 103 male skulls, found before the year 1894 in round barrows or in other interments of the Bronze Age,[4] ranged from 70 to 88, 55 of them exceeding 80; while those of 19 skeletons from round barrows in which no bronze was found ranged from 68 to 88, six of them exceeding 80.[5] In both series a large proportion of the skulls whose indices fell short of 80 belonged, wholly or in part, to the Long Barrow race. Other skulls, however, which have since been described and of the characteristics of which Dr. Beddoe, the compiler of this list, may have been ignorant, yielded indices higher still.[6]

But it is not enough to describe the invaders of the Bronze Age as brachycephalic: they shared that characteristic with peoples who were otherwise markedly different from them. Let us first consider those which belong to the so-called characteristic type, which, until a recent date, received more than its share of attention,—that which is seen only in the taller skeletons. Their foreheads, says Rolleston, were 'sometimes . . . especially in cases where the whole skull and skeleton are marked by great strength and even ruggedness, markedly sloping'.[7] Their supraciliary ridges

minute fraction), or approximately 1 m. 768. Calculated by M. Rollet's method (see p. 379, n. 2, *supra*), the figures would have been just under 5 ft. 8½ in. and just over 5 ft. 9⅞ in. respectively.

[1] A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 6–7, 50–62; iii, 225.

[2] *Archaeol. Cambr.*, 6th ser., v, 1905, pp. 222, 235–6. The average length of the thigh-bones was 446 millimetres, or rather more than 17·55 inches.

[3] *Proc. Aberdeen Univ. Anatom. and Anthr. Soc.*, 1902–4, pp. 11–20, 31.

[4] Dr. Beddoe (*L'Anthr.*, v, 1894, p. 522) assigns all the skeletons in question to the Bronze Age; but I suspect that some are older.

[5] *Ib.* Thurnam's figures are much about the same. He found that out of 70 skulls from round barrows 44 had indices ranging from 80 to 89 (*Memoirs Anthr. Soc.*, iii, 1870, pp. 48–50; *Archaeologia*, xliii, 1871, pp. 543–4). There is reason to believe that some of the round skulls found in round barrows had been artificially flattened on the occiput in infancy; but Thurnam (*Crania Britannica*, ii, pl. 45, p. 6) shows that their brachycephaly was only due in a minor degree to this cause. I presume that Dr. Beddoe, in his article in *L'Anthr.* (v, 1894, p. 522), did not take account of 15 skulls which were found in 1885–7, in association with bronze and remains of the *urus*, during the excavation of the Ribble Docks at Preston. Their cephalic indices range between 70·41 and 81·76. See *Vict. Hist. of . . . Lancs*, i, 250.

[6] *Journal of Anatomy and Physiology*, xxxviii, 1904, p. 127; xxxix, 1905, pp. 438–9; *Proc. Soc. Ant. Scot.*, xxxix, 1905, p. 426. Apparently Dr. Beddoe's list did not include Scottish skulls.

[7] *Brit. Barrows*, pp. 639–40, 642.

were often extraordinarily prominent. 'The eyebrows,' says the same authority, 'must have given a beetling and probably even formidable appearance to the upper part of the face, whilst the boldly outstanding and heavy cheek bones must have produced an impression of raw and rough strength . . . Overhung at its root, the nose must have projected boldly forward.'[1] These men were, in some instances, extremely prognathous:[2] their teeth were often extraordinarily large;[3] and, to quote Thurnam, 'the prominence of the large incisor and canine teeth is so great as to give an almost bestial expression to the skull.'[4] The reader who scans the illustrations in *Crania Britannica* and in Canon Greenwell's *British Barrows* will, however, see that the brachycephalic skulls even of the taller skeletons are not all of the same type. Moreover, some few of the Round Barrow skulls combine the contour of the characteristic brachycephalic skull of the British Bronze Age with dolichocephaly;[5] and this is one of the facts which tend to prove that in certain parts of England the brachycephalic invaders intermarried with the people whom they found in possession. In the East Riding of Yorkshire, indeed, it would seem that the old race and the new were as completely intermingled as the modern population. Dr. William Wright tells us that in a collection of 80 skulls, taken from round barrows and preserved in the Mortimer Museum at Driffield, 'almost all the varieties of cranial shape met with in Europe are represented.' Their cephalic indices ranged from 69 to 92; and, says Dr. Wright, 'it is doubtful if it is possible to find a materially more mixed series of skulls in a community of to-day.'[6] Dr. Wright, however, does not believe that the skulls of apparently hybrid form prove intermarriage between the invaders and the old neolithic population, or that the former were purely brachycephalic. 'To grant this,' he argues, 'one must believe that a pure round-headed race could have made its tardy progress across Europe unmixed,—an assumption which to my mind is incredible.'[7] Has the doctor forgotten that ten male skulls, found in short cists in and near Aberdeenshire and evidently assignable to the end of the Neolithic or the beginning of the Bronze Age,[8] were all brachy-

[1] *Brit. Barrows*, pp. 644–5. See also *Crania Britannica*, ii, pl. 45, p. 4.

[2] *Mem. Anthr. Soc.*, i, 1865, p. 154. In Scotland, however, of 12 skulls from short cists, the mean cephalic index of which was 81·4, only one, says Sir W. Turner (*Nature*, Jan. 13, 1898, p. 258), was prognathous.

[3] *Mem. Anthr. Soc.*, i, 1865, pp. 151–2. [4] *Ib.*, p. 154.

[5] *Brit. Barrows*, p. 681. Cf. *Crania Britannica*, pl. 11; *Reliquary*, N. S., vii, 1901, pp. 240–2; *Wilts. Archaeol. and Nat. Hist. Mag.*, xxxiii, 1904, pp. 18–9; and *Journal of Anatomy and Physiology*, xxxviii, 1904, pp. 120–4, xxxix, 1905, pp. 418–21, 423–4, 429–30.

[6] *Report of . . . the Brit. Association*, 1903 (1904), pp. 801–2. Cf. *Journ. Anthr. Inst.*, vi, 1877, p. 333, and *Journal of Anatomy and Physiology*, xxxix, 1905, pp. 417–21.

[7] *Ib.*, p. 442.

[8] Six of the skeletons were associated with drinking-cups (*Proc. Soc. Ant. Scot.*, xxxix, 1905, p. 431); and no bronze was found with any of them, only flint and bone implements (*Proc. Aberdeen Univ. Anatom. and Anthr. Soc.*, 1902–4, p. 33).

cephalic, and that nine of them belonged to the same pure type.[1] Has he forgotten that the round barrow skulls of Wiltshire were mainly brachycephalic? Has he ever walked over the mountains of Auvergne? Very likely the round-headed race which he has in mind did not make its way across Europe unmixed; but the mixture did not greatly diminish the roundness. Very likely when it reached Britain it included a few long-heads; but the contrast between the uniformity in Wiltshire and the diversity in East Yorkshire suffices to disprove the doctor's theory.

Some of Dr. Wright's brachycephalic specimens belonged to a type which is quite different from the 'characteristic' Round Barrow type, and is also common to almost all the short Welsh and Scottish skeletons mentioned above.[2] These skulls are generally broader than those of the other kind. The ten found in Aberdeenshire and its neighbourhood ranged between 80·8 and 92·3, their average index being 85·39;[3] while those of Glamorganshire ranged between 81·7 and 86, and yielded an average of 84·2.[4] Not one of these skulls is prognathous:[5] all are high as well as round and broad: the supraciliary ridges are only slightly developed: the cheek bones are not prominent: the face is both broad and short; and the lower jaw is small.[6]

Who were the brachycephalic people of the round barrows and the short cists, and whence did they come? Those who have attempted to solve these problems have generally had in mind only the tall round-heads, whether their skulls belonged to the characteristic type or showed signs of crossing with the other. Wherever the short people came from, their ethnical affinities are certain: they belonged to the so-called Alpine type of Central Europe, of which the French Grenelle race were a branch. Let us for the present confine our attention to the others. To the questions which I have asked at least six different answers have been given:—that they were Goidelic Celts; that they were Belgae; that they were Finns; that they came from Denmark or the Scandinavian peninsula; that their original home was Dalmatia; and, lastly, that they may be traced back to the valley of the Rhine.[7] But the view which has been repeated by almost every recent writer is that they were Goidels.[8]

[1] *Proc. Soc. Ant. Scot.*, xxxix, 1905, p. 431.
[2] *Journ. Anat. and Physiol.*, xxxviii, 1904, pp. 127–9.
[3] *Proc. Soc. Ant. Scot.*, xxxix, 1905, pp. 426, 437.
[4] *Archaeol. Cambr.*, 6th ser., v, 1905, p. 219.
[5] *Ib.*; *Proc. Aberdeen Univ. Anatom. and Physiol. Soc.*, 1902–4, p. 26.
[6] *Ib.*, p. 34. A skeleton has been found with a drinking-cup in a short cist in Caithness, which belonged to the same type (*Proc. Soc. Ant. Scot.*, xxxix, 1905, pp. 421–4).
[7] This view, stated independently, would leave it an open question whether they were Celts or not.
[8] See for instance A. Pitt-Rivers (*Archaeol. Journal*, liv, 1897, p. 390); A. H. Keane, *Man, Past and Present*, p. 527; Romilly Allen (*Archaeol. Cambr.*, 5th ser., xvii, 1900, p. 225); W. Boyd Dawkins (*Vict. Hist. of . . . Hampshire*, i, 261); B. C. A. Windle (*Vict. Hist. of . . . Worcester*, i, 179); G. Sergi, *The*

1–2. The Goidelic theory and the Belgic (which I ought perhaps to apologize for noticing) may be considered together; for if any argument tells in favour of the latter, it tells as much or more in favour of the former.

Thurnam, who does not trouble himself about the distinction between Goidelic and Brythonic Celts, points out that 'extremely brachycephalic skulls have been exhumed from many of the French chambered tumuli';[1] that seven skulls with cephalic indices of 80 and upwards from a dolmen near Senlis, which is in the territory that was occupied by the Belgae,' have much resemblance to those from the round barrows';[2] and that three skulls with indices of 80, 80, and 85 respectively from a sepulchral grotto in the Belgic department of the Oise are 'very similar in general character to the short skulls from the round barrows'.[3] He argues that of the cranial types represented by the peoples of the long barrows and the round barrows respectively 'one at least must be Celtic':[4] he points out that in the cremation interments which have been discovered in round barrows 'the appearances are consistent with what we are told of the funerals of the Gauls . . . by Caesar and Pomponius Mela';[5] and his general conclusion is that the Round Barrow people were 'an offshoot through the Belgic Gauls from the great brachycephalic stock of Central and North-Eastern Europe'.[6] Finally, Professor Rhys maintained in 1890[7] (it would be rash to assume that his opinion is unchanged) that the Round Barrow race belonged to the Brythonic group, who, he asserted, being comparatively broad-headed, were less pure than the Goidels.

According to Professor Boyd Dawkins, the Round Barrow race must have been Goidels, and not Wends, Finns, or Slaves, because the latter would not have subsequently retreated eastward 'against the current of the Celtic, Belgian, and German invasions';[8] while the late Canon Isaac Taylor[9] affirmed that the skulls of the well-known 'Sion type', which by some anthropologists are believed to have belonged to the Celtic Helvetii, resembled those of the round barrows.

Now the view that the tall brachycephalic people of the round barrows were the Belgae is so utterly absurd that it is difficult to conceive how writers who posed as authorities on ethnology could ever have entertained it.[10] If some benighted classical scholar had

Mediterranean Race, p. 243; and H. d'A. de Jubainville, *Les Druides*, pp. 15–6. It is useless to multiply references.

[1] *Mem. Anthr. Soc.*, i, 1865, p. 135.

[2] *Ib.*, pp. 484–5.

[3] *Ib.*, pp. 482–3.

[4] *Ib.*, p. 128.

[5] *Ib.*, iii, 1870, p. 76.

[6] *Ib.*, p. 79; *Archaeologia*, xliii, 1871, p. 304. Huxley (S. Laing, *Prehist. Remains of Caithness*, pp. 117–9) agreed with Thurnam.

[7] *Scottish Review*, xv, 251.

[8] *Fortnightly Rev.*, xvi, 1874, p. 337.

[9] *Origin of the Aryans*, pp. 86, 88.

[10] The statement in the text is of course perfectly consistent with the fact that some of the earlier Brythonic invaders buried their dead in small round barrows. See p. 435, n. 1, *infra*.

I am astonished to find that even such a well-informed writer as Mr. H. J.

ascribed the Copernican system to Ptolemy, one may imagine how he would have been derided by scientists; yet such a blunder would not have been different in degree from that which Thurnam committed and Huxley approved. For the Belgic invasion began, at the earliest, in the third, and, as Professor Rhys himself maintains,[1] in the second century before the Christian era; and the first invaders of the Round Barrow race landed in Britain, at the latest, about 1400 B.C.,[2] and probably several centuries earlier. The argument which Thurnam bases upon the alleged similarity between Round Barrow skulls and some which have been exhumed from French dolmens has no weight. To begin with, the theory that any Celtic-speaking people invaded Gaul in the Neolithic Age is contrary to historical and archaeological evidence;[3] and, assuming that they did, the resemblance between the skulls to which Thurnam refers and most of those of the tall Round Barrow skeletons is purely superficial. Any one may convince himself of this who will take the trouble to compare the illustrations of Round Barrow skulls in *Crania Britannica* with those in *Crania Ethnica*; and Thurnam himself in more than one passage[4] admits, indeed emphasizes, the distinction. Even Broca[5] denied that there was any physical affinity between the tall brachycephali of the round barrows and the [so-called] 'real Celts of Gaul'; and, as we shall see presently, by the latter he simply meant the brachycephalic people, descended from neolithic ancestors, that formed the substratum of the population whom Caesar called Celtae. Similarly Dr. Beddoe truly says that the [characteristic] Round Barrow skulls resemble those of Borreby in the Danish island of Falster, rather than those of Broca's Celtae.[6] It is true indeed, as we have seen, that some of the Round Barrow skulls resemble some of the neolithic French

Mackinder (*Britain and the British Seas*, 1902, p. 185) suggests that the Belgae 'may well have been the broad-skulled "bronze" men of the round barrows'; and that, according to Mr. C. H. Read (*Guide to the Ant. of the Bronze Age* [Brit. Museum], p. 15), 'the Gaels and Brythons . . . are the people of the Round-barrows.' It is rather puzzling to find that he fixes 'the close of the Barrow period about 900 B.C.' (*ib.*, p. 23), and yet assigns the first Brythonic invasion to the fourth century B.C. He appears to think that the earliest invaders of the Round Barrow period belonged to a non-Aryan race (*ib.*, pp. 24–5); and he rightly distinguishes both the Goidels and the Brythons from the brachycephalic neolithic population of Gaul (*ib.*, p. 22), whom he nevertheless erroneously calls 'the true Kelts'. See pp. 433–40, *infra*. I am still more puzzled when I read in the *Guide to the Ant. of the Early Iron Age* (p. 2), for which Mr. Read has made himself responsible, that 'the Bronze Age inhabitants of this country seem to have been the most closely connected with the true Kelts', whereas in the *Guide to the Ant. of the Bronze Age* (p. 15) they are sharply distinguished from them.

[1] *Report of . . . the Brit. Association*, 1900 (1901), p. 894.

[2] See p. 127, *supra*.

[3] M. Déchelette's remarks in *Rev. de synthèse hist.*, iii, 1901, pp. 32–3, are worth reading.

[4] *Mem. Anthr. Soc.*, i, 1865, pp. 482–3, 486–8. See also *Brit. Barrows*, pp. 639–41, and J. Beddoe, *The Races of Britain*, p. 253, n. *.

[5] *Mém. d'anthr.*, ii, 1874, p. 126.

[6] *L'Anthr.*, v, 1894, p. 516.

skulls; but, speaking generally, the former are far more rugged and in every way more strongly marked than the latter.[1]

More striking, however, than the contrast between the skulls of the characteristic Round Barrow skeletons and those of the French brachycephalic neolithic race is the discrepancy in stature. The average height of the former was, as we have seen, on the lowest computation, 5 feet $8\frac{2}{5}$ inches; that of the latter was very little over 5 feet.[2] Moreover, while the brachycephalic Finns and Danes and the few modern brachycephalic inhabitants of England are generally tall or moderately tall and fair, those of France and Central Europe are generally not only short but dark.[3]

The argument that since the Long Barrow skulls were pre-Aryan, those of the round barrows must have been Celtic, begs the question. As we shall see presently, there are other skulls in museums, which belong to neither type, and which undoubtedly are Celtic. What reason is there to deny that the earlier brachycephalic invaders who were buried in round barrows may, as Mr. C. H. Read[4] reasonably suggests, have been pre-Aryan? The British Celts of the later Bronze Age were doubtless cremated; and therefore their skulls are not forthcoming. And if the resemblance between the cremation interments of the round barrows and those described by Caesar proved that the former were all Celtic, it would also prove that they were Greek![5]

In answer to Professor Boyd Dawkins it may be said that if the tall Round Barrow race were not Finns or Slaves, it does not follow that they were Goidels. And supposing that they were Finns or Slaves, why should it be necessary to assume that they 'subsequently retreated eastward against the current of the Celtic, Belgian,

[1] See *Crania Britannica*, pl. 1, 53, 41, 11, 32, 43, 42, and the descriptions of these skulls in vol. ii; also the illustrations facing pp. 571, 579, 583, 587, 591, and 599 of Greenwell's *Brit. Barrows*.

The description which Dr. Collignon gives of the brachycephalic race of France will show how totally unlike it is to the characteristic Round Barrow type. He speaks (*Ann. de Géogr.*, v, 1896, p. 164) of 'les caractères bien connus de la race brachycéphale, à savoir, taille plutôt petite, cheveux foncés, tête globuleuse, face ronde, courte, large, plate, nez large et court', &c.

[2] *Rev. mensuelle de l'École d'anthr.*, iv, 1894, pp. 396, 400. The 'maximum of frequency', according to M. Hervé (*ib.*, vi, 1896, p. 105), lies between 1 m. 50 (just over 4 ft. 11 in.) and 1 m. 59 (just over 5 ft. $2\frac{1}{2}$ in.).

I was glad to find, after I had finished the rough draft of this article, that Prof. A. C. Haddon (*Proc. Roy. Irish Acad.*, 3rd ser., iv, 1896–8, pp. 583–4) distinguishes 'the short, swarthy, black- [or rather dark-brown] haired brachycephalic race of Central Europe (the "Celtae" . . . or the "Type de Grenelle" . . .)' from 'the tall, fair, brachycephalic race that may have come from Denmark (the "Celts" of some authors . . . the "Round Barrow Race" of all authors)'. To identify the Grenelle race with the Celtae is, however, misleading. The Celtae (see pp. 438–9, *infra*) were a mixed population, comprising descendants of various neolithic dolichocephalic tribes and of the Grenelle race and also real Celts—the introducers of the Celtic language—who invaded Gaul about the eighth century B.C.

[3] *Journ. Anthr. Inst.*, xxxiv, 1904, p. 203.

[4] *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 25.

[5] See Greenwell's *Brit. Barrows*, pp. 10, 15–6.

and German invasions'? Or that they retreated eastward at all? The 'Iberian' immigrants certainly did not retreat 'against the current' of the Round Barrow invaders: they retreated, if at all, to the remoter parts of Britain. The argument that the Round Barrow skulls resemble those of the Sion type is disposed of by merely comparing the measurements and the illustrations of the two series. The Sion type, as Rolleston[1] says, 'corresponds to many of our long-barrow skulls,' and is not brachycephalic but dolichocephalic or mesaticephalic:[2] there is no proof that it was that of the Helvetii;[3] and, as I have pointed out elsewhere,[4] there is strong reason to believe that the Helvetii did not appear in Switzerland before the Iron Age.

So much for the arguments which have been adduced in favour of the popular theory. There are facts which absolutely disprove it. First, there is no evidence that the brachycephalic people who built round barrows ever reached Ireland, at least in appreciable numbers; for not a single skull of the characteristic Round Barrow type has ever been found there, and only four brachycephalic skulls which can be referred to prehistoric times.[5] Yet it is needless to say that since a time long anterior to the Roman invasion of Britain Ireland has been one of the principal abodes of the Goidelic stock. Secondly, it is, as we have seen, in the highest degree probable, if not certain, that the Round Barrow race first invaded Britain in the Neolithic Age. Let us, however, for the sake of argument, accept Professor Boyd Dawkins's assumption that their advent synchronized with the beginning of the British Bronze Age. Now, according to Professor Montelius, the Bronze Age in this country began about 2000 B.C.; according to Sir John Evans,[6] six centuries later. It is impossible to fix with certainty the date of the earliest Celtic invasion of Britain; but such historical evidence as we possess points to the conclusion that it was not earlier than the seventh

[1] *Brit. Barrows*, p. 682.

[2] *Ib.*, p. 746; L. Rütimeyer and W. His, *Crania Helvetica*, 1864, p. 12. The average cephalic index of 29 skulls of the Sion type described in *Crania Helvetica* is 77·2, the highest being 81·9, and the lowest 73. Not one of the 22 illustrations has the slightest resemblance to the more strongly marked brachycephalic Round Barrow type. The Sion type, moreover, is orthognathous, whereas the tall Round Barrow men were often extremely prognathous.

Taking into account the skulls of the Sion type which have been measured since the publication of the work of His and Rütimeyer, the average cephalic index is 76. See *Rev. mensuelle de l'École d'anthr.*, v, 1895, p. 153.

[3] Rice Holmes, *Caesar's Conquest of Gaul*, 1899, p. 308.

[4] *Ib.*, p. 296, and n. 3.

[5] See J. Beddoe, *The Races of Britain*, p. 16; *Scottish Review*, xxi, 1893, p. 361; W. Z. Ripley, *The Races of Europe*, p. 310: and cf. Sir W. R. Wilde, *The Beauties of the Boyne*, 2nd ed., 1850, p. 40; W. C. Borlase, *Dolmens of Ireland*, iii, 1006–12; and *Proc. Roy. Irish Acad.*, xxiv, 1902–4, sect. C, pp. 1–6. Professor A. C. Haddon (*ib.*, 3rd ser., iv, 1896–8, p. 584) suggests that the brachycephalic people who did invade Ireland were 'the Neolithic brachycephals of Central Europe', and that 'the Round Barrow race had comparatively little to say to Irish ethnology'.

[6] See pp. 126–7, *supra*.

century before the Christian era.[1] M. Salomon Reinach has argued that a Celtic-speaking people appeared in North-Western Gaul in the ninth century,—the earliest date which has ever been proposed by any scholar; but his view is based on the mere conjecture that κασσίτερος, the Greek word for tin, which occurs in Homer, is of Celtic derivation.[2] M. d'Arbois de Jubainville, indeed, who adopts this conjecture,[3] supposes that the Celts actually landed in Britain as early as the ninth century before Christ; but even if we accept his chronology, we are confronted with the fact that the very earliest date that has been assigned on historical or linguistic grounds for the first Celtic invasion[4] is four or five centuries later than the latest, ten or eleven centuries later than the earliest date which has been assigned by archaeologists for the commencement of the Bronze Age in Britain. Yet anthropologists and antiquaries will go on repeating the dogma that the builders of the round barrows, who, at the latest, began to arrive in Britain at the commencement of the Bronze Age, were Goidelic Celts. The moral is that anthropologists and antiquaries would not be worse equipped if they enlarged the sphere of their studies.

Again, the view that a Celtic-speaking people invaded Britain at the close of the Neolithic or the beginning of the Bronze Age implies that Celtic and Latin, the nearest of kin in the Aryan family of languages, had become differentiated long before the Neolithic Age came to its end. Would any philologist who knew the rudiments of archaeology sanction a theory so preposterous?[5]

The foregoing arguments apply equally to the short men whose remains have been found in the greatest purity in North-Eastern Scotland. The race to which they belonged began to arrive in Gaul very early in the Neolithic Age:[6] they themselves landed in Britain before its close. Whoever they may have been, they were neither Goidels nor Belgae nor Brythons of any tribe.

Finally, although I am aware that I am about to tread upon thorny ground, I affirm that there is not the slightest reason to doubt that the Celtic invaders of Britain, in so far as they were descended from the Celtic-speaking people who conquered Gaul, were not a brachycephalic but a dolichocephalic or mesaticephalic people. I have already argued in favour of this thesis in a dis-

[1] See K. Müllenhoff, *Deutsche Altertumskunde*, ii, 1887, pp. 236–8, and cf. H. d'A. de Jubainville, *Les premiers habitants de l'Europe*, i, 1889, p. 262, and *Report of . . . the Brit. Association*, 1900, p. 894.

[2] See p. 494, *infra*.

[3] *Les Celtes*, pp. 19–20.

[4] Professor Rhys, who a few years ago (*Report of . . . the Brit. Association*, 1900, p. 893) assigned the Goidelic invasion to 'the seventh and the sixth centuries B.C.', has recently (*Celtic Britain*, 1904, p. 2) dated it back to 'more than a millennium before the Christian era', but without giving any reasons.

[5] See *L'Anthr.*, xiv, 1903, p. 344. The Aryans, before their dispersion, were acquainted with the use of copper (O. Schrader, *Prehist. Ant. of the Aryan Peoples*, pp. 187–91; *L'Anthr.*, iv, 1893, p. 547; *Rev. de l'École d'anthr.*, xiv, 1904, pp. 163, 207–19; *Bull. et mém. de la Soc. d'anthr.*, 5e sér., v, 1904, p. 88).

[6] *Rev. de l'École d'anthr.*, xv, 1905, p. 407.

sertation on 'the Ethnology of Gaul',[1] and I will now adduce fresh evidence in its favour. But first let me make my meaning perfectly clear. I do not mean that the Celtic invaders of Britain were all of the same type. On the contrary, I assume that the dominant race had intermixed and intermarried, before they embarked from the Continent, with descendants of the neolithic stocks. I do not mean that even the invaders who introduced the Celtic language into Gaul, even those who beat the Romans on the Allia, were homogeneous. Dr. Beddoe, as I have remarked elsewhere,[2] warns us not to believe that there was ever a period when, for example, all the Caledonians were red-haired. I only mean that among the Celtic-speaking conquerors of Britain dolichocephaly, as well as tallness and fairness, was a prevailing characteristic.

Thurnam[3] asserted that 'we may ask in vain for a series of ancient dolichocephalic skulls which, on satisfactory archaeological grounds, can be assigned to the immediately pre-Roman, and therefore to the Celtic period, either in England or in France'. Let us consider England first. Now it happens that the skulls of the 'Late Celtic' period, or Early Iron Age, which have been found in this country are almost all either dolichocephalic or mesaticephalic.[4] Canon Greenwell,[5] it is true, explains this fact by the assumption that 'the intruding round-headed people . . . were gradually absorbed by the earlier and more numerous [Long Barrow] race'. 'In this way,' he says, 'it appears to me that we may account for the skull type of the Early Iron Age without the necessity of requiring any immigration into Britain or its conquest after the time of the presumed occupation by the bronze-using round-headed people,' &c. But that necessity is imperative. Had Canon Greenwell momentarily forgotten his Caesar? The immigration of the Belgae took place, at the earliest, in the third century B.C., many centuries after the 'occupation by the bronze-using round-headed people'. It is true that some of the British skulls which belong to the Late Celtic period are of the same type as those of the Long Barrow race:[6] but this only proves that the Long Barrow race survived; and others are of a type which, as Rolleston says, is 'entirely wanting . . . in the series from the long barrows'.[7]

[1] *Caesar's Conquest of Gaul*, 1899, pp. 281–319.

[2] *Ib.*, p. 305.

[3] *Mem. Anthr. Soc.*, i, 1865, p. 514.

[4] W. Greenwell, *Brit. Barrows*, pp. 636, 683, 711. See also *Crania Britannica*, ii, pl. 6, pp. 7–8; *Proc. Soc. Ant.*, 2nd ser., vol. xvii, 1897–9, p. 126, n. *; and p. 435, n. 1, *infra*.

[5] *Brit. Barrows*, pp. 129, 213.

[6] The skulls which have been found in the fort of Worlebury, near Weston-super-Mare, belong, according to Prof. Macalister (C. W. Dymond and H. G. Tomkins, *Worlebury*, 1886, pp. vii, 102–4), 'to the so-called Iberian type'; but they have 'strong brow ridges', and 'the men were of strong muscular build'. They appear to me to show signs of crossing with individuals of the 'characteristic' Round Barrow type; but it is impossible to determine whether they were of Gallo-Brythonic descent or not. Prof. Macalister computed the stature of five males, whose bones, except in one instance, did not belong to the skulls, at 5 ft. 3 in., 5 ft. 5½ in., 5 ft. 8 in., 5 ft. 10 in., and 6 ft. 4 in., the average being 5 ft. 8½ in.

[7] *Brit. Barrows*, p. 683. It appears, however, highly probable that the

Unfortunately, however, the Late Celtic skulls which have been found in Britain are comparatively few;[1] and hardly any of them can be assigned with certainty to the Brythonic invaders.

'Iberian' and North-European dolichocephalic types, to the latter of which the type which I call Celtic belongs, are traceable to the same origin. See *Geogr. Journal*, xxviii, 1906, pp. 538, 541.

[1] Partly because during the latter part of the period the custom of cremation was prevalent in South-Eastern Britain. See p. 286, *supra*.

A considerable number of skeletons has been discovered in the so-called 'Danes' Graves' in the parish of Driffield, Yorkshire, which undoubtedly belong to the Early Iron Age, and were earlier than the time of Agricola (*Proc. Soc. Ant.*, 2nd ser., xx, 1904–5, p. 257), by Dr. Thurnam (*Archaeol. Journal*, xxii, 1865, pp. 109 n. 8, 264), Canon Greenwell (*ib.*, pp. 108–11, 264), and Mr. J. R. Mortimer (*Proc. Soc. Ant.*, 2nd ser., xvii, 1897–9, pp. 119–28). The cephalic indices of those male skulls which were found by Thurnam and Canon Greenwell are 75, 76, 70, 75, and 71: the mean index of those in the collection of Mr. Mortimer, who does not give the individual measurements, is 75·5; and the indices of fourteen, which have lately been measured by Dr. Wright (*Journ. Anthr. Inst.*, xxxiii, 1903, pp. 67, 70–1), and which, for aught that I know, may have included the others, ranges from 68 to 79. Neither Thurnam, nor Canon Greenwell, nor Mr. Mortimer says anything about stature; but the average height of the men whose bones Dr. Wright measured would only have been 5 ft. 3½ in. This is so low as to suggest that they were not Celts; and the question of their origin has caused much discussion. The remains of a chariot were found in one of the graves which Mr. Mortimer opened; but chariots may of course have been used by non-Celtic Britons. According to Thurnam, the skulls 'appear to be distinguished from the . . . long-barrow type', and might pass for those of modern inhabitants of Scandinavia; but the pottery found in the graves by Canon Greenwell was not only unlike any which he had discovered in other parts of Yorkshire, but also different from Scandinavian or Anglo-Saxon ware. Moreover, he describes the mode of interment as 'unlike any which has been found in Denmark, Norway, or Sweden'. Therefore I cannot agree with Dr. Wright, who thinks that the people in question came from Scandinavia. All that is certain is that, like most of our Late Celtic skeletons, they did not belong to the familiar tall Celtic type.

In Scorborough Park, near Beverley, there is a group of small mounds, similar to the 'Danes' Graves'. Mr. Mortimer opened six of them in 1895, and found two skulls 'of a decidedly long type'.

Fourteen skulls at least have been found in and just outside the Glastonbury marsh-village (*Report of . . . the Brit. Association*, 1895, p. 519; 1896, p. 658; 1898 [1899], p. 695; 1899 [1900], p. 594; *Proc. Somerset. Archaeol. and Nat. Hist. Soc.*, l, 1904, p. 80; li, 1905, pp. 88, 99–100); but no detailed description of them has yet been published, though Prof. Boyd Dawkins (*Vict. Hist. of . . . Somerset*, i, 200) affirms that they 'belong to the small dark Iberic inhabitants', and argues that as some of them belonged to men who had been decapitated, they do not represent inhabitants of the village, but their enemies. Some, however, belonged to young children, and were found in the hut-circles. There is the same dearth of information about skeletons which have been found near Birdlip, on the Cotswold Hills (*Trans. Bristol and Gloster Archaeol. Soc.*, v, 1880–1, pp. 137–41), and in the parish of St. Keverne, Cornwall (*Archaeol. Journal*, xxx, 1873, pp. 267–72).

In the only interment of the Early Iron Age that has yet been discovered in Scotland—a cist on the estate of Moredun in Midlothian (*Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 427–38)—which was probably not earlier than the second century of our era (*ib.*, p. 438), two skeletons, apparently of females, were found. It was only possible to calculate the stature of one, which, estimated from the *femur* alone, by what method I do not know, was about 5 ft. 5½ in. This, for a woman, would be comparatively tall. The cephalic index was 75; and, according to Dr. T. H. Bryce, who measured the skull

In France, on the other hand, the skulls of the corresponding period are very numerous; but few of them have been measured. Those few, however, confirm my argument. They belonged with very few exceptions to tall mesaticephalic or dolichocephalic men; and two of them may be seen in Salles IX and X of the Musée de St. Germain, near Paris, the former having been buried with his war-chariot, iron helmet, and long iron sword. The mean index of twenty-seven adult male skulls of this type, found in *tumuli* of the Early Iron Age in the department of the Marne, was 78·49; but Broca, who has described them, maintains that the index of skulls of the purest 'Kymric' (or, to use the term which is now in vogue, 'Galatic') type would be considerably lower; for, he argues, as the Gauls of the Marne lived very near the frontier of the Celtae, they must have intermarried with the brachycephalic people who formed the great majority of that group of tribes.[1]

Again, in a recent article on *tumuli* of the Early Iron Age in the department of the Côte-d'Or, Dr. Hamy points to the noteworthy fact that two brachycephalic skulls, belonging to descendants of an earlier race, were found 'among the dolichocephali who predominated in that population';[2] and in a paper which he has just published on the earliest Gallic invaders of the Iron Age he shows that the cephalic indices of the available skulls from the Châtillonnais and the arrondissement of Beaune range between 73·1 and 76·59, while the average stature was 1 metre 75·7, or just over 5 feet 9⅛ inches.[3]

The prevalent view in this country is, I am aware, that the Celts were a brachycephalic people; but it is begotten of sheer confusion of thought. Professor Ripley[4] remarks that 'there is practically

(*ib.*, pp. 439–45), 'all the measurements and the indices deduced from them are such as might belong to a [neolithic] skull from the chambered cairns,' but 'the general characters are markedly different. It resembles in general proportions certain of the skulls from the "Danes' Graves" . . . described by Dr. W. Wright . . . but in form it does not fall in with any of his types . . . the skull shows rather closer affinities with the modern than with any ancient type,' &c. Has Dr. Bryce seen any of the skulls from the Gallic tumuli of the Early Iron Age?

For further information about skeletons of this period see *Crania Britannica*, ii, pl. 6 and 7, pp. 2, 7 (Arras), pl. 43, p. 3 (Roundway Hill); *Proc. Soc. Ant.*, 2nd ser., iv, 1867–70, pp. 275–6 (Grimthorpe); *Brit. Barrows*, p. 683; *Archaeologia*, lii, 1890, pp. 325–6; and *Guide to the Ant. of the Early Iron Age* (Brit. Museum), pp. 124–5, 130. The Arras and Grimthorpe specimens at least were probably Brythonic.

[1] *Rev. d'anthr.*, ii, 1873, pp. 605, 607, 611. Unhappily Broca does not give the indices of all the skulls, but only the average.

[2] *Bull. du Muséum d'hist. nat.*, &c., 1902, p. 178.

[3] *L'Anthr.*, xvii, 1906, pp. 7, 10, 16–7, 25. See also *Crania Ethnica*, p. 498; *Scottish Review*, xxi, 1893, p. 171; A. Bertrand and S. Reinach, *Les Celtes*, &c., pp. 122–34; *Rev. mensuelle de l'École d'anthr.*, vii, 1897, pp. 65–87; *Bull. et mém. de la Soc. d'anthr.*, v^e sér., ii, 1901, pp. 721–2; and *Archiv für Anthr.*, xxviii, 1902, pp. 185–6.

[4] *The Races of Europe*, p. 126. 'The philologers,' says Professor Ripley, 'properly insist upon calling all those who speak the Celtic language, Celts . . . while the physical anthropologists, finding the Celtic language spoken by

to-day a complete unanimity of opinion among physical anthropologists, that the term *Celt*, if used at all, belongs to the brachycephalic darkish population of the Alpine highlands'; and he adds that the only dissentient is M. G. de Lapouge.[1] But Dr. Beddoe,[2] whom he counts among the professors of the orthodox faith, has emphatically recorded his opinion that, at the time of the Roman conquest, the Celtic-speaking people of Southern Britain 'partook more of the tall blond stock of Northern Europe than of the thick-set, broad-headed dark stock which Broca has called Celtic'; and the 'unanimity' (which is far from being 'complete') upon which Professor Ripley pins his faith is due partly to misunderstanding or misinterpretation of Broca's famous essay, *Qu'est-ce que les Celtes*, partly to the desire of establishing a uniform connotation, and partly to the fact that some physical anthropologists have neglected to supplement their scientific researches by the study of classical texts. Broca found the term 'Celt' used in a multiplicity of senses, and he attempted to put an end to confusion by attaching to it one limited, conventional, and, as we shall see, misleading signification. When, in the essay to which I have just referred, he endeavoured to prove that the Celts were a dark brachycephalic people, he expressly limited the term 'Celts' to the population of that part of Gaul which, according to Caesar,[3] was inhabited by 'a people who call themselves Celts and whom we [the Romans] call Gauls'. 'There is no proof,' he insists, 'that the existence in the British Isles of a people bearing the name of Celts has ever been authoritatively affirmed':[4] according to him, the invaders of Britain who spoke the so-called Celtic languages were the Belgae,[5] for he knew nothing about Goidels or pre-Belgic Brythons; and, although he allowed himself to be persuaded that the tall Round Barrow race spoke Celtic, he denied 'that there is any other affinity except that of language between the brachycephali of the round barrows and the real Celts of Gaul'.[6] When he insisted that 'the Celts' were a dark brachycephalic people, he did not mean that darkness and brachycephaly were characteristic of the conquerors who

people of divers physical types, with equal propriety hold that the term Celt, if used at all, should be applied to that physical group or type of men which includes the greatest number of those who use the Celtic language.' I, on the contrary, hold that in an ethnological inquiry the term should be applied to 'that physical group' (if we can discover it) among whom the Celtic language came into being and who imposed it upon those whom they subdued; and I would remind the philologers that if all who speak the Celtic language are Celts, all who speak the English language, including the inhabitants of the United States and the negroes of Jamaica, are Englishmen.

[1] See *L'Anthr.*, iii, 1892, p. 748. We shall see that MM. Collignon, Hervé, and Wilser are also dissentients. So too is Dr. Laloz (*L'Anthr.*, xiii, 1902, p. 776).

[2] *The Races of Britain*, p. 29. See also *L'Anthr.*, v, 1894, p. 517.

[3] *B. G.*, i, 1, § 1.—Gallia est omnis divisa in partes tres, quarum unam incolunt Belgae, aliam Aquitani, tertiam qui ipsorum lingua Celtae, nostra Galli appellantur.

[4] *Bull. de la Soc. d'anthr.*, 2e sér., xii, 1877, p. 511.

[5] *Ib.*, p. 514.

[6] *Mém. d'anthr.*, ii, 1874, p. 126.

introduced the Celtic language into Gaul: he meant that they were characteristic of the great mass of the mixed population whom Caesar called *Celtae*,[1] who were in the main descended from neolithic invaders, and whose uppermost stratum, so to speak, consisted of invaders whom Broca, speaking as a physical anthropologist rather than a philologist, called 'Kimris'.[2] That the name *Celtae* did not belong to the people of Gaul until it was introduced by these Celtic-speaking 'Kimris' is evident from the fact that it belongs to the Celtic tongue:[3] in other words, the Celts, anthropologically speaking, were originally identical with the invaders who introduced the Celtic language first

[1] 'La race celtique,' he says (*Bull. de la Soc. d'anthr.*, 2e sér., ix, 1874, p. 713), 'est le résultat du mélange des races indigènes avec les immigrants.'

[2] *Mém. d'anthr.*, i, 1871, p. 395.

[3] A. Kuhn's *Beiträge zur vergleichenden Sprachforschung*, &c., v, 1868, p. 98. Cf. J. Rhys, *Celtic Britain*, 1904, p. 291. The remark of Professor Rhys (*ib.*, p. 2) that 'Recent writers are of opinion that the terms Galli and Celtae argue an ancient distinction of race', and that 'the latter first applied exclusively to the aborigines', is apparently based upon an entire misconception of the writings of Broca and M. Alexandre Bertrand. Does the professor mean by 'the aborigines' the dolichocephalic neolithic Baumes-Chaudes race, or the totally different brachycephalic neolithic Grenelle race? No French ethnologist holds the opinion which Professor Rhys attributes to 'recent writers'; and even M. Bertrand, who distinguished 'les Celtes' from 'les Galates', was careful to point out (*Les Celtes dans les vallées du Pô et du Danube*, p. 36) that between them there was no 'distinction of race'. The reader should note that, according to M. Bertrand, 'les Galates,' who conquered Gaul in the Iron Age, belonged to 'la race celtique'; that his tall fair 'Celtes', who had invaded Gaul before, were not identical with, but only part of the mixed population whom Caesar called *Celtae*; and that his 'Galates' were to be found among the Celtae as well as among the Belgae. Professor Rhys, in a recent paper (*Celtae and Galli*, pp. 57–9, 62), assumes that as (according to his view) both Goidelic and Gallo-Brythonic were spoken in the country of the Celtae, the names *Celtae* and *Galli* correspond to the peoples who spoke the two dialects: he argues that the Celtae were conquered by the Galli; and he concludes that the two peoples were ethnologically distinct. Probably Goidelic Celts were conquered by Gallo-Brythonic Celts; but what then? It remains certain that conquered and conquerors were by themselves called collectively Celtae. Why did the name of the conquered prevail over that of the conquerors if it was essentially different? And does not Caesar expressly say that the two names denoted one and the same people? As a matter of fact, the terms *Celtae* and *Galli*, as used by the ancient writers, including Polybius, were, generally speaking, synonymous. Diodorus Siculus (v, 23, § 1) distinguished between them; but as his Γαλάται included the Cimbri and other Germans, his testimony, which implicitly contradicts that of Caesar, is worthless. Even if it could be accepted it would only show that the Celtae, as a whole, differed from the Γαλάται, not that the Galatic conquerors of the people who, after the conquest and including the conquerors, were called Celtae, differed in race from earlier Celtic conquerors. Moreover, as I have remarked in *Caesar's Conquest of Gaul* (p. 290), 'anybody who carefully reads through the chapters in which he [Diodorus] describes the inhabitants of Gaul, will see that he habitually uses the word Γαλάται not in the restricted but in the general sense, including both Γαλάται and Κελτοί. . . In fact, though he thinks it necessary to warn his readers that the Celtae were geographically distinct from the Galli, he draws no physical distinction between them; and, in conformity with ancient usage, he as a rule uses the two terms indifferently.' See my *Caesar's Conquest of Gaul*, 1899, p. 300; M. Déchelette's article in *Rev. de synthèse hist.*, iii, 1901, pp. 32 3; *Rev. de l'École d'anthr.*, xv, 1905, pp. 216–30; and *Rev. celt.*, xxvii, 1906, pp. 109–10.

into Germany and then into Gaul.[1] These invaders were tall and mesaticephalic or dolichocephalic; and the Celtic-speaking conquerors of Britain belonged to the same stock.

'The radical errors in Broca's definition of the "Celts of history" [so I wrote some years ago [2]] are these:—first, he calmly assumes that no classical writer's testimony, except Caesar's, is of any value; and secondly, he fails to see that Caesar, by saying that the people who called themselves "Celts" were called by the Romans "Gauls", makes it as clear as noon-day that for him and for his countrymen, as for Polybius and Pausanias, the words "Celt" and "Gaul" were synonymous. Broca admits that the older population of Gallia Celtica was conquered by men of the same race as the Gauls or Celts who captured Rome. Therefore it is absolutely certain that the Celtae of Transalpine Gaul were called after their conquerors. The truth is that Broca, while he aimed at putting an end to confusion, only made confusion worse confounded. Moreover, throughout his discussion, *he simply ignores the Helvetii, who, according to Caesar, were included among the Celtae.*'

Since the foregoing paragraph was written, I have lighted upon a passage [3] in which Broca himself justifies my argument and uses the word 'Celt' in the sense which I attach to it. The Celtae of Gaul, he remarks, 'were already mixed before the arrival of the Kimris [or Gallo-Brythonic invaders], since the name [Celtae] under which they appeared for the first time in history had been imposed upon them by the conquering race of the Celts properly so called, which, like the Kimris and the Germans, came from the east, and, like them, was dolichocephalic.' [4]

Professor Ripley appeals to the German ethnologist, Johannes Ranke,[5] whose arguments, he insists, are 'decisive'. But any one who will take the trouble to read the chapter which Ranke devotes

[1] I am glad to find that I have the support of Dr. Collignon (*Annales de géogr.*, v, 1896, p. 159), who speaks of 'la population pré-gauloise que Broca nommait à tort les Celtes'. Similarly M. G. Hervé (*Rev. mensuelle de l'École d'anthr.*, vi, 1896, p. 99) says that '*la race brachycéphale néolithique* ou *race de Grenelle* est devenue à l'âge de bronze ... celle des *Celtes*, au sens que les anthropologistes ont accoutumé d'attacher depuis Broca à ce dernier terme'; and, as he remarks (*ib.*, p. 104), the Celtic language was imposed on this people, long before they and their conquerors were called by Caesar *Celtae*, by invading Gauls. MM. Collignon and Hervé do not perhaps make it sufficiently clear that the people whom Broca called 'les Celtes' were not the brachycephalic neolithic race alone, but that race *plus* mesaticephalic people also of neolithic origin *plus* the conquerors of both.

[2] *Caesar's Conquest of Gaul*, 1899, p. 291. See also pp. 245–8, 281–301 of the same book.

[3] *Bull. de la Soc. d'anthr.*, ii, 1861, pp. 508–9.

[4] ... 'les Kimris s'étaient établis en grand nombre dans cette région [the neighbourhood of Paris], au milieu des populations celtiques; que celles-ci, enfin, étaient déjà mélangées avant l'arrivée des Kimris, puisque le nom sous lequel elles ont pour la première fois paru dans l'histoire leur avait été imposé par ... la race conquérante,' &c. 'Cette première opinion,' says Dr. L. Wilser (*L'Anthr.*, xiv, 1903, pp. 496–7), 'oubliée plus tard par son auteur et par ses disciples, était juste.'

[5] *Der Mensch*, ii, 1887, pp. 261–7.

to the Celts will see that his argument does not support Professor Ripley's contention. Virchow, he reminds us, has pointed out that wherever the Celts are known to have penetrated dark peoples are now to be found. But, as he fully admits, Virchow himself said, 'I am not on that account inclined to assume that the original Celts were . . . dark,' and reminded his readers that the ancient writers described the Celts as fair. Ranke points out, further, that wherever the Celts originally dwelled in Central Europe we now find the people not only dark but also brachycephalic; but at the same time he warns us to bear in mind that in certain Celtic districts of Britain dolichocephaly is unmistakable, and that there is evidence that on the Continent *the Celtic invaders found a dark brachycephalic people in possession.* In other words, Ranke does not commit himself to any theory as to the physical characters of the Celts properly so called,—the invaders who introduced the Celtic dialects into Germany, Gaul, Britain, and other countries which they subdued. The reader will also bear in mind that the writers who identify the tall brachycephalic Round Barrow race with the Goidelic Celts unanimously maintain that they were fair.

That the Celtic-speaking invaders of Gaul and Britain were commonly dolichocephalic or mesaticephalic is not only attested by the skulls of warriors of the Iron Age, but is either attested or at least not disproved by the results of modern observations of existing Celtic-speaking peoples[1] and of the country which was formerly inhabited by the Gallic Belgae.[2] When Sergi[3] tells us that the Gauls who captured Rome were 'composed of brunet Celts and blond Teutons', he makes an assertion which, as it is absolutely unsupported by any evidence, calls for no refutation; and it would be useless to ask him who were the 'blond Teutons' who were the ancestors of the red-haired Gauls of the Perthshire Highlands.[4] As Dr. Beddoe[5] puts it, the Gauls of Scotland are probably descended from 'Iberians' crossed with 'a long-faced, harsh-featured, red-haired race, who contributed the language and much of the character'.[6]

3. The late Mr. Charles Elton,[7] referring to Professor A. H.

[1] Cf. *Scottish Review*, xxi, 1893, p. 368; *Proc. Roy. Irish Acad.*, 3rd ser., iii, 1893–5, pp. 323, 369; v, 1898–1900, pp. 43, 45, 71, 227–8; vi, 1900–2, p. 506; *Journ. Anthr. Inst.*, xxvii, 1898, pp. 104–30, and especially p. 117; *Trans. Roy. Soc. Edinburgh*, xl, part iii, 1903, pp. 547–614; and Rice Holmes, *Caesar's Conquest of Gaul*, 1899, pp. 281–320.

[2] *Journ. Anthr. Inst.*, xxvi, 1897, p. 124. Cf. my *Caesar's Conquest of Gaul*, 1899, pp. 301–5.

[3] *The Mediterranean Race*, pp. 74–5.

[4] See p. 418, n. 1, *supra*.

[5] *The Races of Britain*, pp. 270–1.

[6] An Italian anthropologist, Dr. V. Barteletti (*Archivio per l'antropologia e la etnologia*, xxxiii, 1903, pp. 277–85) affirms that red hair is an anomaly due to the crossing of blond with dark people. On this theory it seems inexplicable that in certain parts of the Highlands of Scotland and Wales red hair is very much more common than anywhere in England or in those parts of the Continent in which blonds and brunets have long been intermixed, and much more common in the department of Finistère than elsewhere in France. See *Crania Britannica*, i, 210; my *Caesar's Conquest of Gaul*, 1899, pp. 315–6; and Dr. Beddoe's article in *Journ. Anthr. Inst.*, xxxv, 1905, p. 223.

[7] *Origins of Eng. Hist.*, 1890, p. 162.

Sayce's *Science of Language*,[1] affirmed that 'a Finnish idiom has been traced in several of the British languages', and inferred that the tall builders of the round barrows were Finns. The idiom in question may, for aught that I know, have been traced by some philologist who had determined to find it, but not by Professor Sayce nor by any one to whom Professor Sayce refers. Mr. Elton's argument is as obsolete as that which Professor Rhys founded upon his imaginary tracing of Basque in the language of the Picts.

4. Much may be said for the theory of the late Professor Rolleston, that the tall people of the round barrows came from Denmark or some of the adjoining islands, if it be duly modified. On the coast near Flamborough Head are remains of earthworks, which, as has been demonstrated by General Pitt-Rivers, who excavated them, were erected by invaders fighting their way inland; and, as he remarks, 'it is unlikely that any but Northmen should have landed in this spot.'[2] Thurnam himself admits that there is 'a great resemblance' between the characteristic Round Barrow skulls and those from 'the Giants' Chamber at Borreby [in the island of Falster], and from other Scandinavian megalithic tombs';[3] and his testimony is confirmed by Rolleston[4] and Dr. Beddoe.[5]

Dr. A. H. Keane[6] argues, in opposition to Rolleston's view, that if any of the Round Barrow invaders had come from Scandinavia, 'they must have spoken some Low German dialect, of which there are no clear traces in the tribal and place-names of the Bronze Age.' The answer is, first, that, as Mr. C. H. Read[7] suggests, they may have spoken not a Low German but a pre-Aryan dialect; and, secondly,

[1] Vol. i, p. 85. Probably Mr. Elton intended to refer to vol. ii, p. 85; but neither there nor on any other page of the book is there a single sentence which bears out his statement.

[2] *Journ. Anthr. Inst.*, vi, 1877, p. 505. In a more recent paper (*ib.*, xi, 1882, pp. 467–8), after remarking that 'the defenders of the earth-work used flint, and consequently the work itself is not later than the bronze period', and that the people who buried their dead on the Yorkshire wolds 'were in the early bronze phase of civilisation', General Pitt-Rivers goes on to say, 'the archaeologists of Denmark have shown that the Early Bronze Age did not exist in Denmark; the art of working in bronze was full-blown when it first entered Denmark. If the invaders of Flamborough came from Denmark, and were, as we suppose . . . a bronze-using people, they would have brought with them weapons of a more advanced type than those found in the tumuli of the wolds . . . We are narrowed, therefore, to the opinion that the invaders of Flamborough, if invaders they were, were the same people who landed on the south and south-east coasts of England [the extreme improbability of which he has already shown], or else that these dykes belong to the people of the country, who . . . were driven to the coast by another . . . people who occupied the interior,' &c. But why should the general assume that 'the invaders of Flamborough' were 'a bronze-using people'? See pp. 119, 129, 131 2, 408 9, *supra*.

[3] *Mem. Anthr. Soc.*, i, 1865, pp. 130, n. *, 508–10.

[4] *Brit. Barrows*, pp. 588–9, 680. Rolleston also mentions 'the discovery in Yorkshire of monoxylic coffins with similar contents and fashion to those found in South Jutland', &c. (*ib.*, p. 631, n. 2).

[5] *Journ. Anthr. Inst.*, xix, 1890, pp. 482–3. Cf. *Scottish Review*, xxi, 1893, p. 162, and W. Z. Ripley, *The Races of Europe*, p. 309.

[6] *Man, Past and Present*, p. 528.

[7] *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 24–5.

that we know absolutely nothing about either the tribal or the place-names of Britain in the Bronze Age. Assuming that Low German tribal or place-names existed in Britain before the Celtic invasion, they would for the most part have been superseded by Celtic names, just as the Celtic invaders of Gaul generally substituted their own tribal and place-names for those of their predecessors, and just as in certain parts of Scotland Celtic names of rivers gave place to Norse names.[1]

5. Messrs. J. Gray and J. F. Tocher infer from their observations of the physical characteristics of the population of West Aberdeenshire that 'a tall, broad-headed, dark-haired, light-eyed people', whom they regard as 'the descendants of the men of the Bronze Age', formerly inhabited Aberdeenshire, but were driven inland by later blond immigrants, who were shorter and had narrower heads, and whom they identify with North Germans.[2] The resemblance of the tall dark people to modern Dalmatians[3] is, they say, 'significant when taken in conjunction with the fact that bronze first came into the British Isles from South-East Europe.'

'The fact!' But is it the fact? Archaeology has certainly shown that Britain, in the Bronze Age, was commercially connected with Northern France, which, as Mr. C. H. Read[4] says, was 'supplied to a certain extent from Italy'. But no archaeologist supposes that bronze was carried all the way from Italy, still less from Dalmatia, into Britain or even into Northern France by Italians or Dalmatians. It came through the methods of primitive commerce. Moreover, as we have already seen,[5] 'the men of the Bronze Age,' by whom Messrs. Gray and Tocher mean the tall brachycephalic people of the round barrows, were still in their Stone Age when they began to invade Britain. A direct immigration from the coasts of the Adriatic into West Aberdeenshire or even Southern Britain is inconceivable; and if it had taken place gradually across the Continent, we should find that the immigrants had left traces of their presence on the way, which is not the case. Notwithstanding the thoroughness with which Messrs. Gray and Tocher conducted their investigation, I fear that it throws no new light upon the ethnology of Ancient Britain. After the successive invasions and immigrations, the internal migrations, and the intermarriages of 3,000 years, it is utterly impossible to establish by dint of even the most elaborate census of a living population the fact that the people of the Bronze Age even in West Aberdeenshire were 'tall, broad-headed, dark-haired, and light-eyed'; and if they were, why only in West Aberdeenshire?

6. The Honourable John Abercromby maintains that the brachycephalic invaders, or some of them, came at the beginning of the Bronze Age or in the period of transition between the Neolithic Age

[1] *Man*, ii, 1902, No. 79, p. 110.
[2] *Journ. Anthr. Inst.*, xxx, 1900, No. 84, pp. 86–8.
[3] Cf. *Scottish Review*, xx, 1892, p. 378, and *Journ. Anthr. Inst.*, xxxiv, 1904, pp. 203–4.
[4] *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 24.
[5] See pp. 408–9, *supra*.

and the Bronze Age from the neighbourhood of the middle Rhine or from some intermediate district between it and Britain.[1] Remarking[2] that 'the recorded finds of the last hundred years are sufficient to establish the fact that the beaker [or drinking-cup] is the oldest form of fictilia in the Bronze Age of this country', he argues that the immigrants who introduced the oldest drinking-cups of the kind which Thurnam designated as 'type β' must have belonged to a tribe who at one time lived in the valley of the Rhine, because between British and Rhenish specimens of this type 'there is a substantial agreement' both in form and ornament, which 'seems too great to be the result of pure accident'; and he points out[3] that 'the type exists not only in the central Rhine, but also near its mouth', though the intermediate stages cannot be traced. The Rhenish cups belong to the Neolithic Age; and it seems impossible to prove that the earliest British examples were not made before any objects of bronze were manufactured in or introduced into Britain:[4] but Mr. Abercromby has certainly established a very strong probability in favour of the locality to which he refers their origin.[5]

The great mistake that has been made in discussing the question is the not uncommon assumption that the brachycephalic immigrants who buried their dead in round barrows arrived in Britain at one time and came from one place. Some of them certainly appeared before the end of the Neolithic Age: others may have introduced bronze implements or ornaments; others doubtless came, in successive hordes, during the course of the Bronze Age. Some of those who belonged to the Grenelle race, who certainly came from Eastern Europe and possibly from Asia,[6] and whose centre of dispersion was the Alpine region,[7] may have started from Gaul;[8] others could have traced their origin to some Rhenish tribe; and I am inclined to believe

[1] *Journ. Anthr. Inst.*, xxxii, 1902, pp. 373-97.

[2] *Ib.*, p. 374.

[3] *Ib.*, pp. 388, 393.

[4] *Ib.*, pp. 394–5. Dr. T. H. Bryce, who has made a special study of the chambered cairns of South-Western Scotland, and has found no bronze in any of them, tells us (*Man*, iv, 1904, No. 110, p. 176) that in one at Glecknabae, Bute, 'fragments of four vessels were recovered, of the "beaker" or "drinking-cup" class.' 'If,' he says (*Proc. Soc. Ant. Scot.*, xxxviii, 1904, p. 78), 'we accept Mr. Abercromby's conclusions that this class of ceramic was introduced at the end of the Neolithic period, and that the type named α is earlier than . . . β and γ, we are obliged to conclude that the culture of the Stone Age prevailed in the Western Islands for the whole period corresponding to type α in South Britain.'

[5] Some of the skulls examined by Dr. Wright (see p. 427, *supra*) resembled the 'Row Grave' (*Reihengraber*) skulls of Germany, and he suspects that they belonged to immigrants from the valley of the Rhine (*Journ. Anat. and Physiol.*, xxxix, 1905, p. 441).

[6] *Rev. mensuelle de l'École d'anthr.*, viii, 1898, p. 207; *Proc. Roy. Irish Acad.*, 3rd ser., iv, 1896–8, p. 584.

[7] *Rev. mensuelle de l'École d'anthr.*, vi, 1896, p. 105.

[8] Does not the radical difference between British and Gallic pottery of the Bronze Age (see *L'Anthr.*, xvii, 1906, pp. 325, fig. 2; 337, fig. 8; 338–9, figs. 9 and 10; 340, fig. 11) tell against the view that many immigrants from Gaul entered Britain in the earlier periods?

that those who belonged to the characteristic rugged Round Barrow type crossed over, for the most part, from Denmark or the outlying islands. That the first Celtic-speaking invaders landed in Britain before the end of the Bronze Age I do not deny; and if they came from that part of Gaul which was inhabited by the Celtae, I have no doubt that many of them were brachycephalic. But it is nevertheless certain that among these invaders the dominant element, who were Celtic in blood as well as in speech, and whose physical type was that described by the ancient writers, were not brachycephalic but mesaticephalic or dolichocephalic. And if I am asked where the Celtic skulls of the later British Bronze Age are to be found, I answer, Nowhere: they were reduced to ashes by cremation.[1]

It is interesting to find that, according to Huxley, of the skeletons that were found in the famous Heathery Burn Cave, near Durham, which was inhabited in the closing period of the Bronze Age, not one belonged to either of the brachycephalic types, but all to 'the same race of rather small and lightly-made men with prominent superciliary ridges and projecting nasal bones'[2] which is represented by the river-bed skulls of England and Ireland.[3]

IX. THE CELTS

1. Little can be added to what has been said in the previous section about the physical characteristics of the Celtic invaders of Britain. Some Celtic scholars, as we shall presently see,[4] deny that any Goidels reached this country before the Roman conquest; but, assuming that some did so, there is no reason to suppose that they differed much physically from the Brythons. If Strabo[5] was right in saying that the Britons generally were less fair-haired than the Gauls, the inference would seem to be that the Celtic invaders of Britain had intermarried more freely than those of Gaul with the descendants of the aborigines; nor would this inference be weakened by the fact that, according to the same authority,[6] they were conspicuously taller than their Gallic kinsmen.[7] I believe, however, that Strabo's statements were based upon nothing more than his own observation of the few Britons whom he says that he himself saw in Rome, supplemented perhaps by hearsay evidence derived from Roman soldiers or traders who were not trained observers; and that his testimony

[1] Moreover, it must be remembered that only one interment of the Early Iron Age has been found in Scotland (see p. 435, n. 1, *supra*), although the culture of the same period is represented by 'finds' that range from Dumfriesshire to the Orkneys.

[2] *The Geologist*, v, 1862, p. 204. Cf. *Archaeologia*, liv, 1895, pp. 110–1.

[3] See pp. 396–7, *supra*.

[4] See p. 448, *infra*.

[5] iv, 5, § 2.—*οἱ δὲ ἄνδρες εὐμηκέστεροι τῶν Κελτῶν εἶσι καὶ ἧσσον ξανθότριχες*, &c.

[6] *Ib.*,—*σημεῖον δὲ τοῦ μεγέθους· ἀντίπαιδας γὰρ εἴδομεν ἡμεῖς ἐν Ῥώμῃ τῶν ὑψηλοτάτων αὐτόθι ὑπερέχοντας καὶ ἡμιποδίῳ*, &c.

[7] See p. 425, n. 4, *supra*.

is worth neither more nor less than that of Lucan, who speaks of 'the fair-haired Britons'.[1] Dr. Beddoe[2] has concluded, from his observation of the modern inhabitants of 'those parts of Scotland and the north of England where Kymric blood may well be supposed to remain in large proportion,' that the Belgae who invaded Britain as well as those of Gaul were on the whole somewhat dark: but his arguments, which I have examined fully elsewhere,[3] do not prove that the dominant Celts among the Belgae were dark, but simply that, before they invaded Britain, they had become largely intermixed with an older dark population, and that, since they reached this country, they and their descendants have intermarried with people darker than themselves.[4]

2. Professor Rhys has more than once changed his opinion about the Celtic invaders of Britain since he began to handle the subject. In the second edition of his *Lectures on Welsh Philology*[5] he argued that they were not 'two distinct nationalities, speaking two distinct languages'; in other words, he maintained that the Goidelic and Brythonic dialects had been evolved within the British Isles after the Celts had entered them. In the preface to *Celtic Britain*, however, which was written in January, 1884, he recanted; and his old view is now obsolete. For many years past he has maintained that the earliest invaders were Goidels, or, as he now prefers to call them, Celticans;[6] and that the later comers were Brythons. But whereas until a recent date he held that the only Brythonic invasion was that of the Belgae, and that Pytheas, who visited Britain towards the end of the fourth century B.C., 'is not likely to have found any Brythons here,'[7] he now holds, or at all events held a few weeks before the time when I am writing, that the first Brythonic invaders 'appear to have settled here before the middle of the fourth century B.C., for Pytheas . . . gives indirect evidence to their presence'.[8] To this view I hope he will firmly adhere. There is, indeed, no direct evidence that any Brythonic immigrants landed in Britain before the Belgae. But indirect evidence there is; and that of two kinds. The first has been already noticed in the section on the Picts. There are good grounds for believing that the authority whom Diodorus

[1] *Phars.*, iii, 77–8.—

celsos ut Gallia currus
Nobilis et flavis sequeretur mixta Britannis.

[2] *The Races of Britain*, pp. 26, 249, 258.

[3] *Caesar's Conquest of Gaul*, 1899, pp. 302–5.

[4] In regard to the ethnology of the Belgae, see *ib.*, pp. 301–25, with which cf. J. Rhys, *Celtae and Galli*, p. 60. [5] pp. 17, 35.

[6] *The Welsh People*, 1902, p. 12. When that book appeared he was disposed to apply the word 'Goidel' to the mixed population of 'Celticans' and aborigines, who, he holds, became more closely fused under pressure from the Brythons. [For 'Celticans' he is now (?) inclined to substitute 'Kelts' of the 'Celtic' (not 'Keltic') family. Unlearned readers who scoff at subtle distinctions will find an explanation in the professor's *Celtae and Galli*, p. 56. Is not the word Celtican unfortunate? The Celtici (Strabo, iii, 1, § 6) were in N.W. Spain.]

[7] *The Welsh People*, 1902, p. 75.

[8] *Celtic Britain*, 1904, p. 4.

Siculus followed in his notices of Britain was Pytheas.[1] Diodorus speaks of the British Isles as Πρετ(τ)ανικαὶ νῆσοι;[2] and the *P* in Πρετ(τ)ανικαί (if that reading is certain), shows that Pytheas learned the word from lips which spoke a Brythonic, or Gaulish dialect. M. d'Arbois de Jubainville asserts that his informants were Gauls:[3] but that is simply his opinion; it is open to any one to argue that Pytheas probably learned the name of the Britons as well as the facts which he reported about them and their country in Britain, and not in Gaul. Be this, however, as it may, it is, as we shall presently see, certain that during the earlier period of the Roman occupation, the greater part of England and a considerable part of Scotland were inhabited by Brythons; and, as we shall also see, it is extremely improbable that they were all of Belgic origin. The question of the chronological order of the various Celtic invasions is, according to Professor Rhys,[4] answered by the present geographical distribution of the Celtic-speaking peoples of the British Isles: 'it may be regarded,' he says, 'as fairly certain that those who are found driven furthest to the west were the earliest comers.' The argument might be sound enough (though the word 'driven' begs the question) if we were considering the British Isles as a whole, and not merely Britain;[5] and even those who maintain that there were no people of Goidelic descent in Britain in the time of Caesar could hardly answer Professor Rhys unless they assumed that the Goidelic invaders of Ireland came from Spain, or that they dared not risk a contest with the Southern Britons; for otherwise it is hard to believe that they would not have directed their immigration towards Britain, the nearer country.

Professor Rhys, in his *Celtic Britain*,[6] endeavours to trace the distribution of the Brythonic and Goidelic peoples, as he believes it to have existed at the time of the coming of the Romans; and in so doing he uses materials on which he founds another argument to show that there were Goidels in Britain at that time. These materials are Goidelic inscriptions which have been found in North Wales, in Cornwall, and in Devonshire:[7] but not one of them belongs to an earlier date than the fifth century of our era. With the exception of the districts in which they occur, of the greater part of Somersetshire and Dorsetshire, of South Wales and the adjoining parts of England

[1] See p. 490, *infra*.

[2] F. Vogel, in his edition of 1888, adopts the reading Βρεττανικῶν νήσων in i, 4, § 7; but everywhere else he prints the word with Π, following the codex *Vindobonensis*. See p. 459, *infra*.

[3] *Rev. celt.*, xiii, 1892, pp. 399–400.

[4] *The Welsh People*, 1902, pp. 3–4.

[5] I am of course aware that Professor Kuno Meyer disregards this argument; but he makes no attempt to answer it.

[6] pp. 218–63 (216–60 of the older edition).

[7] *The Welsh People*, 1902, p. 8; *Celtic Britain*, 1904, pp. 216–8. When the second edition of *Celtic Britain* was published, Professor Rhys held (p. 216) that the inscriptions were the monuments of Goidels retreating before Brythonic invaders, 'and not those of Goidelic invaders from Ireland.' In the new edition (p. 218) he says that 'it is partly the monuments of these retreating Goidels of Britain that we have in the old inscriptions, but partly perhaps those also of Goidelic invaders from Ireland'.

which lie between the Severn and the Teme, and of Cumberland, part of Westmorland, the Isle of Man, Dumfries, Kirkcudbright, Wigtown, Ayrshire, Renfrew, and part of Lanarkshire, the professor regards the whole of Britain south of the Firths of Clyde and Forth as Brythonic; and he prints a list of proper names, most of which are certainly Brythonic, in support of this conclusion.[1] The northern part of the island he divides, for reasons which have been already examined, between Goidels and aboriginal tribes, whom he identifies with the Picts properly so called.[2] It will, however, of course be understood that when he speaks of Goidelic and Brythonic tribes, he means tribes who spoke the Goidelic and Brythonic dialects. The former he regards as mingled largely with the aborigines, and the latter with both Goidels and aborigines. But it is difficult to understand how he has been able to maintain that the Dumnonii of Cornwall and Devonshire were Goidels in the face of the fact that most of the British emigrants who invaded Brittany came from the Cornish peninsula,[3] bringing the name *Dumnonii* with them, and that he himself formerly insisted that the Dumnonii who inhabited what is now Renfrew and Ayrshire were Brythons.[4] I say 'formerly', because this is one of the many opinions which the professor has felt obliged to discard: 'the southern portion' of the Scottish Dumnonii have just been transformed by a stroke of the pen into 'Goidels who adopted Brythonic speech'.[5] However, as M. d'Arbois de Jubainville says, referring to the inscriptions upon which Professor Rhys relies, 'To conclude from the fact that five Goidels were buried, during the period which elapsed from about 400 to about 700 A.D., in the territory of the Dumnonii, that the entire population of that territory was Goidelic seems extremely rash;'[6] and, he asks,[7] 'if they were Goidels, how came it that they brought a Brythonic dialect into Brittany?' Further, he asks why Professor Rhys maintains that the Novantae of Galloway were Goidels when he admits that the Trinovantes of Essex were Brythons;[8] and the only answer which the professor vouchsafes to this question is that the name *Novantae*

[1] *Ib.*, pp. 229–31.

[2] See the map facing the title page of *Celtic Britain*.

[3] Professor Rhys (*Celtic Britain*, 1904, pp. 214–5) denies that Brittany was colonized 'by Brythons from here': but one of his arguments is simply that the Dumnonii were not Brythons, which I deal with in the text; and the other is equally unsatisfactory. Remarking that Procopius 'gives a very fabulous account of an island called Brittia', he says that 'Brittia must have been a real name, as it is exactly the form which would result in that which is the actual Breton name of Brittany—namely Breiz: this last,' he continues, 'cannot be derived from any known form of the kindred name of our country and its people, and thus tells not a little against the tradition that Brittany was first colonised by Brythons from here,' &c. But who ever heard of 'the tradition that Brittany was *first* colonised by Brythons from here'? And what if Brittany received the name which would have resulted in 'Breiz' *before* the British immigration? See my *Caesar's Conquest of Gaul*, 1899, p. 416, and J. Loth, *L'Émigration bretonne en Armorique*, 1883, pp. 21, 50–1, 75–82.

[4] *Celtic Britain*, 1884, p. 221.

[5] *Ib.*, 1904, pp. 223–4.

[6] *Rev. celt.*, vii, 1886, pp. 379–80.

[7] *Ib.*, xxii, 1901, p. 124.

[8] *Ib.*

was 'given them probably by Brythons'.[1] What are the grounds of his opinion, he does not say. I may add that while he explains[2] that 'the consonantal combination of *cs* or *x*' is Gaulish, that is to say, Gallo-Brythonic, he says[3] that it is 'remarkable' that 'most of the early names with *x* belong to districts which have before been pointed out as non-Brythonic'. When we look for these districts, we find[4] that they were those of the Taexali, the Vacomagi, the Scottish Dumnonii, the Selgovae, and Cumberland. When we ask on what grounds the inhabitants of these districts had been 'pointed out as non-Brythonic', we find[5] that the Taexali and the Vacomagi were Pictish, that is to say 'no doubt' aboriginal; that the Dumnonii, according to the professor himself,[6] were 'undoubtedly Brythons', and remained so until, discovering perhaps that he had inadvertently given his case away, he changed them by his enchanter's wand into 'Goidels who adopted Brythonic speech';[7] and that the Selgovae are asserted to have been, like the Novantae, 'in a great measure... most likely a remnant of the aboriginal inhabitants.'[8] Why? Because they were afterwards included under the name *Atecotti*, which 'appears to have meant old or ancient', and was 'possibly given to them by the Brythons'.[9] Doubtless they were 'in a great measure' aboriginal, as were doubtless all the British tribes; but seeing that *Uxellon*, the name of a town in their country, is Gaulish, the natural conclusion is that their Celtic masters were not Goidels but Brythons.

3. Professor Kuno Meyer holds that 'no Gael ever set his foot on British soil save on a vessel that had put out from Ireland';[10] and his words are echoed by Dr. Macbain.[11] Professor Meyer points out that 'we have the concurrent testimony of Irish and Welsh tradition that from the second century of our era till the sixth a series of partial conquests of Britain took place'.[12] Dr. Beddoe[13] has indeed argued that it is extremely improbable that 'the Romans would have allowed the Irish Gael to acquire by violence possession of a large portion of one of their provinces'; and Professor Meyer, who admits the difficulty, says that he will not attempt to explain it away. He might have noted that the author of the panegyric which was addressed A.D. 296 to Constantius Chlorus[14] expressly affirms that

[1] *Celtic Britain*, 1904, p. 222.
[2] *Ib.*, p. 233.
[3] *Ib.*, p. 234.
[4] *Ib.*
[5] *Ib.*, pp. 164–6.
[6] *Ib.*, 2nd ed., 1884, p. 221.
[7] *Ib.*, 1904, pp. 223–4.
[8] *Ib.*, p. 222.
[9] *Ib.*, p. 223.
[10] *Trans. Hon. Soc. Cymmrodorion*, 1895–6, p. 69.
[11] W. F. Skene, *The Highlanders of Scotland*, 1902, p. 383.
[12] *Trans. Hon. Soc. Cymmrodorion*, 1895–6, pp. 65–6.
[13] *The Races of Britain*, p. 29.
[14] *Incerti Pan. Constantio Caesari*, c. 11 (published in XII *Panegyrici Latini* recensuit Aemilius Baehrens, 1874).—Britannia natio etiam tunc rudis, et solis Pictis modo et Hibernis assueta hostibus adhuc seminudis, facile Romanis armis signisque cessit. Prof. Haverfield (*The Romanization of Roman Britain*, p. 28) apparently disbelieves that there was any Irish invasion of Britain as early as the third century; but see *Y Cymmrodor*, xiv, 1901, p. 102.

such invasions did take place. Professor Meyer also points out that the Gaelic inscriptions which have been found in Southern Britain belong almost exclusively to South Wales, the quarter to which the invasions may be assumed to have been directed, very few having come to light in North Wales, Devonshire, and Cornwall.[1] On the other hand, it will be admitted that the record of these invasions is no proof that Goidels had not settled in Britain in pre-Roman times.

4. M. d'Arbois de Jubainville holds, as we have already seen,[2] that Goidels, or rather a people who spoke 'the Celtic dialect from which Goidelic was evolved',[3] were masters of the British Isles in the time of Pytheas, and that between his time and that of Caesar Britain was conquered by the Cymric Brittones. So far he is substantially in agreement with the view which, until a recent date, commended itself to Professor Rhys,[4] who, as the reader knows, now believes that there were two successive Brythonic invasions.[5] The more important differences between the two scholars lie partly in their views, which have been already examined, of the Pictish question; partly in the fact that M. d'Arbois is unable to accept the evidence which satisfies Professor Rhys that in Caesar's time and later Goidelic tribes still remained in Western and Northern Britain. He holds that many of them had been driven by the Belgae into Ireland, and that in Britain they only survived as a vanquished people who had been forced to adopt the language of their Gaulish conquerors.[6] I am inclined to believe, from the analogy of Gaul,[7] that in Caesar's time Goidelic was still spoken in remoter parts of the island.

5. Mr. Nicholson has recently attempted to prove that all his predecessors are entirely mistaken even on the few points on which they are agreed. According to him, the earliest Celtic invaders of the British Isles were Brythons, whom, however, he prefers to call Kymri; after them came a horde of Goidels; in the third century before Christ the Picts, who were also Goidels, invaded Scotland; and finally came the Belgae, who were Goidels too! The result was that 'apparently the great majority of the tribes inhabiting Roman Britain were Goidels',[8] although 'of the later Kymric recovery and victory in Wales and some other parts there is no manner of doubt'.[9] It will, at all events, be admitted that a victory, however late, gained by a small minority, was no mean achievement.

[1] *Trans. Hon. Soc. Cymmrodorion*, 1895–6, pp. 70–1.

[2] See pp. 411–2, *supra*; also M. d'Arbois's *Principaux auteurs de l'ant. à consulter sur l'hist. des Celtes*, p. 69.

[3] *Les Celtes*, p. 17.

[4] I say 'substantially' because M. d'Arbois, unlike Professor Rhys, holds that at the time of the Goidelic invasion the Celtic language was everywhere one and the same.

[5] See p. 445, *supra*.

[6] *Les Celtes*, p. 31.

[7] See p. 410, *supra*. M. d'Arbois rejects the analogy; but of course he would admit that the people of Gaul who remained behind belonged ethnologically to the same stock as those who, on his theory, invaded Britain and became the ancestors of British Goidels.

[8] *Keltic Researches*, p. 110. [9] *Ib.*, p. 111.

How does Mr. Nicholson set about proving this revolutionary theory? He tells us that 'on the map of Roman Britain' he can only see one 'certainly Kymric geographical name'[1]—Pennocrucium (now Penkridge) in Staffordshire. The long lists of Cymric names which have been drawn up by Professor Rhys, Dr. Whitley Stokes, M. d'Arbois de Jubainville, and Dr. Macbain do not move him at all. When he is confronted with geographical, tribal, or personal names belonging to Pictland—names such as *Argentocoxos*, *Epidii*, *Gartnait*, the *Ochil* Hills, and the prefixes *aber* and *pet*—he either ignores them or, as his opponents would say, explains them away.[2] Professor Rhys's list[3] is disposed of with the same breezy self-confidence. *Corstopiton*, *Epeiacon*, (Mons) *Graupius*, *Leucopibia*, *Maponi*, *Parisi*, *Petuaria*, *Prasutagos*, *Rutupiae*, *Toliapis*,—these names are either left out of account or explained as Goidelic by the simple method of affirming or 'suspecting' that the *p* in each case is 'Indo-European'.[4] The reader will form his own opinion if he can; only he will bear in mind that the weight of authority is all on one side. When doctors disagree, the patient must decide for himself which is the quack.

So much for the assertion that the Goidels, who, according to Professor Kuno Meyer and Dr. Macbain, were non-existent in Britain at the time when the Roman conquest began, formed then 'the great majority' of the population. What is the evidence for the theory that they came later than the Brythons?

There is no doubt that the Celts who first entered Gaul were Goidels[5] (assuming that Goidelic was then a distinct dialect[6]), and that the latest Celtic invaders of Gaul as of Britain were Belgae.[7] If the Belgae had been Goidels, we should then have to admit that Gaul was invaded first by Goidels, then by 'Cymri', and finally by Goidels again. Is this likely? And is it not likely that if Goidels were the first Celts who invaded Gaul, they were also the first who invaded Britain?

Mr. Nicholson offers the following arguments in favour of his theory. Remarking that the Menapii were a Belgic tribe, he says[8] that 'the Isle of Man(n) [which Caesar calls *Mona*] is called *Monapia* by Pliny (iv, 103)'; and that the Gaelic dialect which is spoken in the island is evidence that its inhabitants in Pliny's time were Goidels.

Now I ask, first, is it certain that Pliny's *Monapia*, rather than Caesar's *Mona*, was the name by which the Isle of Man was known to its own inhabitants? Is it not probable that the name *Monapia*, which is, at all events presumably, Brythonic, came to Pliny from

[1] *Keltic Researches*, pp. 110–1.
[2] *Ib.*, pp. 30, 37, 5, 63–5, 78, 175.
[3] *Celtic Britain*, 1904, pp. 229–31.
[4] *Keltic Researches*, pp. 19–20, 27, 16–7.
[5] Mr. Nicholson himself (*ib.*, p. 151) calls attention to the fact that the Gallic tribes whose Goidelic character he believes himself to have proved belonged, for the most part, to the west of Gaul.
[6] See pp. 410 and 449, *supra*.
[7] See Rhys's *Celtae and Galli*, p. 60.
[8] *Keltic Researches*, p. 9.

a Brythonic source ?[1] Secondly, assuming that the names *Monapia* and *Menapii* are etymologically connected, does it necessarily follow that *Monapia* was a name peculiar to the Belgae, seeing that the tribal name *Ceutrones* occurs not only in Belgic Gaul but in the Alps ?[2] Thirdly, is Mr. Nicholson prepared to prove that the Isle of Man was not colonized by Goidels after it had received the name *Monapia* from Brythons ? Lastly, since Mr. Nicholson himself affirms[3] that although the name *Aremorici* is ' certainly Kymric ', it nevertheless ' is no proof that the Aremoricans were Kymric ', why does he insist that the fact, if it is a fact, that *Monapia* was Goidelic proves that the Belgae were Goidels ?

Again, he says that the Parisi, who lived near the mouth of the Humber, were Belgae,[4] and he believes that ' their name preserves Indo-European *p* '.[5] But Caesar did not include the Gallic Parisii among the Belgae, and did include them among the Celtae.[6] Mr. Nicholson's belief, that the *p* in their name is Indo-European, is not shared by any other Celtic scholar.

Thirdly, he argues that the Atrebates, who were certainly Belgae, were Goidels ; for, he says,[7] ' With one exception, no ogam-inscription has ever been found in these isles outside territory which is known to have been once in Goidelic occupation. The single exception is that of the stone found at Calleva Atrebatum (Silchester).' But, according to Mr. Nicholson himself, ' the great majority ' of the British tribes were Goidelic : yet in only a small minority of their

[1] Cf. J. Rhys, *Celtic Britain*, 1904, p. 231.

[2] *B. G.*, i, 10, § 4 ; v, 39, § 1. Mr. Nicholson contends (*Keltic Researches*, pp. 9–13) that the Belgae also colonized Anglesey, where he finds various place-names of which *Bol* forms a part ; South Wales, where St. David's was formerly called Menew ; both banks of the estuary of the Forth, where he believes that he can find traces of the Irish stem *Manann* ; and Galway, Mayo, and other remote parts of Ireland, where the name *Mannin* is of frequent occurrence. The Belgae, or rather the Menapii, would certainly seem to have been not less enterprising as colonists than Mr. Nicholson as an etymologist. Without straining the elasticity of the words *Menapii* and *Belgae* more than he has already done, he could easily, with a little diligence and a good gazetteer, find traces of them all over the world. Surely they must have settled in *Bulg*aria. But, seriously, I would ask the reader to consider whether it is likely that they would have taken the trouble to go all the way to Connemara when there was plenty of good land open to them in this country. And, considering that they introduced the use of coins into Britain, is it not significant that no British coins have been found in Ireland, and hardly any in Scotland or Wales ?

Mr. Nicholson (*Keltic Researches*, pp. 11, 98–100) of course maintains that the Fir-Bolg of Ireland were Belgae, and that there is an etymological connexion between the two words. Professor Rhys, in a note to the second edition of his *Celtic Britain* (p. 280), which in the third is absent, affirmed that ' one thing is certain : neither the people [Belgae] nor its name had anything whatever to do with the Irish Fir-bolg '. At all events, MacFirbis and other Irish writers regarded the Fir-Bolg as having been found in Ireland and conquered by the Celtic invaders (J. Rhys, *Celtic Heathendom*, p. 120 ; W. C. Borlase, *Dolmens of Ireland*, iii, 1027–8).

[3] *Keltic Researches*, p. 151, n. 1.

[4] *Ib.*, p. 15.

[5] *Ib.*, p. 16.

[6] Cf. *B. G.*, i, 1, § 2, with ii, 1–4. See also J. Rhys, *Celtae and Galli*, 1905, p. 61.

[7] *Keltic Researches*, p. 16.

territories are ogam inscriptions forthcoming; and that minority, with the possible exception of the Atrebates, is in the west of England. What then is proved by the solitary inscription at Silchester? The individual who erected it was doubtless a Goidel:[1] but if it is to be regarded as a proof that the Atrebates were Goidels, then the existence of synagogues in Great Britain proves the truth of that widespread delusion which Professor Tylor[2] has described as 'abject nonsense',—the 'Anglo-Israel theory'.[3]

Fourthly, Mr. Nicholson remarks[4] that between the Parisi and the Iceni, the name of whose king, Prasutagus,[5] he regards 'as containing Ind.-Eur. *p*', while all other Celtic scholars regard it as Brythonic, dwelled the Coritani.[6] 'From their position on the coast,' he says, 'they should belong to the same Picto-Belgic family, and I submit that their name is simply Qṛtanoi, Cruitni.' In other words, Mr. Nicholson submits that a single tribe, which he assumes to have been Belgic, called itself by the same name which, on his own showing,[7] had been given to the entire population of Great Britain[8] long before the Belgae set foot in the land!

[1] I find that, in the judgement of Prof. Haverfield (*The Romanization of Roman Britain*, p. 29), 'the inscription . . . may be best explained as the work of some Western Celt who reached Silchester before its British citizens abandoned it in despair.'

[2] Chambers's *Encyclopaedia*, 1901, vol. i, p. 279.

[3] 'In case,' says Mr. Nicholson (*Keltic Researches*, p. 16, n. 2), 'any one should quote against me Eppillus, the name of a son of Commius the Atrebat, as derived from *epos* for *equos*, let me say that in that case it ought to have only one *p*.' No doubt it is remarkable that the *p* should be double (Rhys, *Celtic Britain*, 1904, p. 302); but *Epillos*, which is certainly the same word (A. Holder, *Alt-celtischer Sprachschatz*, i, 1445), occurs on Gallic coins of the Lemovices and on coins from Poitiers and the neighbourhood of Arles (E. Muret and M. A. Chabouillet, *Cat. des monnaies gaul. de la Bibl. nat.*, 4578, 4579, 4580). See also *Rev. celt.*, xxvi, 1905, p. 189.

[4] *Keltic Researches*, pp. 17–8.

[5] Tacitus, *Ann.*, xiv, 31.

[6] Ptolemy, *Geogr.*, ii, 3, § 11.

[7] *Keltic Researches*, pp. 25, 149. On page 26 (n. 1) Mr. Nicholson makes the curious suggestion that 'the Britons, strictly speaking, were the Kymric branch who painted themselves, as distinguished from the Goidelic who tattooed'. Is he prepared to argue that the Belgae, who, on his theory, were Goidels, and with whom (p. 110) he apparently identifies 'the original Brittones or Brittani', were not included among 'the Britons, strictly speaking'? Will he maintain, in the face of Caesar, from whom we learn that the Britons all 'painted themselves (*Omnes vero se Britanni vitro inficiunt*)', that 'the great majority' (p. 110) of the inhabitants of Britain did not paint? And, since Caesar undoubtedly included among the painted Britons the maritime tribes of the south-east, and also included them among the Belgae, does he not see the inconsistency into which he has fallen?

Bratuspantium, the name of a Belgic town mentioned by Caesar (*B. G.*, ii, 13, § 2), would to most minds prove that the Belgae spoke a Gallo-Brythonic dialect, not only by the *p* which it contains, but also by the *nt*, a non-Goidelic combination. Mr. Nicholson, however (*Keltic Researches*, p. 16, with which cf. A. Holder, *Alt-celtischer Sprachschatz*, i, 515), of course explains the *p* as Indo European.

[8] It may be worth mentioning that Professor Rhys has affirmed (*The Welsh People*, p. 13) that the language of the British Goidels shows more traces of having been influenced by contact with the language of the non-Aryan aborigines than that of the Brythons. This fact, if it were a fact, would obviously

6. I have set down the gist of the linguistic evidence which has been offered in support of the various theories about the Goidels and the Brythons in order that the reader may be able to form an independent judgement about its value. It goes without saying that on any particular question of Celtic etymology no opinion except that of a competent Celtic scholar is worth listening to: on most of the questions that concern us competent Celtic scholars differ widely among themselves: Professor Rhys differs from himself; and Mr. Nicholson, whose competence I neither affirm nor deny, differs from everybody. Even the lay reader who has studied the writings of Dr. Windisch, of Professor Rhys, of Dr. Whitley Stokes, of Dr. Macbain, of Mr. Nicholson, and of M. d'Arbois de Jubainville, and who has made much use of Alfred Holder's *Alt-celtischer Sprachschatz* cannot but see how few of the etymologies that relate to ethnology are to be accepted as certain. It would of course be absurd to sneer at the services which philology has rendered to ethnology and history; nevertheless the fact remains that on almost all the fundamental questions of Celtic ethnology the philologists agree to differ. And, at the risk of appearing flippant, I cannot help saying that when I read some of Mr. Nicholson's pages, when I see how M. Salomon Reinach demonstrates, with the approval of M. d'Arbois de Jubainville and of Professor Rhys, who for once find themselves in agreement, that *κασσίτερος*, the Greek word for tin, must be of Celtic derivation because the root *cassi-* is found in numerous Celtic names,[1] I ask myself whether some future philologist will not adduce the similarity between *Tamesis* and *Tamesi*, the name of a Mexican river, as a proof that the Celts once colonized Central America; whether he will not compare the name of Admiral Togo with that of the British prince, Togo-dumnos, and prove that 'the Japanese Nelson' was of Celtic extraction.[2]

7. Caesar, in a familiar passage, states that 'the maritime districts [of Britain are inhabited] by people who crossed over from Belgium to plunder and attack [the aborigines], almost all of them being called after the tribes from whom the invaders were an offshoot'.[3]

be a further argument, if such were needed, against the view that the Goidels were the latest Celtic invaders of Britain. One expects, of course, to find that the professor changed this view, which was published in 1900; and accordingly we read in the address which he delivered in the same year to the British Association (*Report*, &c., p. 896) that 'the syntax of insular Brythonic is no less non-Aryan than that of Goidelic'. Naturally in 1902 (*The Welsh People*, 3rd ed., p. 13) he repeated the former statement.

[1] See p. 494, *infra*.

[2] Philologists who have a sense of humour should read a truly delicious story told by M. H. Gaidoz (*Esquisse de la religion des Gaulois*, pp. 22–4) about a 'celtiste de premier ordre', who sent him for publication in the *Revue celtique* an elaborate study on the word *encina*, which he had discovered on the pedestal of a statuette and taken for a Celtic inscription, but which, as M. Gaidoz mercifully warned him, was simply the name of the engraver, M. Encina, 56, boulevard Montparnasse, Paris. 'Nous croyons utile,' M. Gaidoz gravely concludes, 'de protester par un exemple irréfutable contre l'abus qu'on semble faire actuellement de l'étymologie.'

[3] *B. G.*, v, 12, § 2.—maritima pars [Britanniae incolitur] ab iis qui praedae

It is, however, impossible to define the limits of the region which, in Caesar's time and during the period that elapsed between the date of his departure and that of the Claudian conquest, was occupied by the Belgae. The only tribal names that indicate their presence are those of the Catuvellauni,[1] who, about the commencement of the Christian era, occupied a territory of uncertain area round Verulamium, or St. Albans, which included Hertfordshire, Middlesex, and probably parts of Bedfordshire, Buckinghamshire, and Northamptonshire; the Atrebates, who possessed parts of Hampshire and Berkshire; and the Belgae, whose chief towns, according to Ptolemy,[2] were Aquae Calidae, or Bath, and Venta, or Winchester.[3] Caesar's words would certainly lead us to believe that the Cantii, the Trinovantes, and the Regni were also Belgic peoples, although their names do not occur in the list of the Belgic tribes of Gaul.[4] Professor Rhys indeed affirmed in the second edition of *Celtic Britain*[5] that 'there is no evidence that the Cantii . . . should be considered Belgic'; and this statement is repeated in the edition which has recently been published: one feels therefore that the evolution of the professor's views is quite normal when one reads in an intermediate volume, published two years ago,[6] that the earliest Belgic invaders of Britain were probably the Brittani,[7] and that the Brittani were probably the Cantii.

8. Finally, Dr. Macalister regards certain skeletons which have been found in the War Ditches of Cambridgeshire below layers that contained traces of late Roman occupation as Anglian[8]; and it may be that they testify to a pre-Roman immigration from Northern Germany.

ac belli inferendi causa ex Belgis transierunt, qui omnes fere isdem nominibus civitatum appellantur quibus orti ex civitatibus eo pervenerunt, &c.

[1] Ptolemy, *Geogr.*, ii, 3, § 11. Cf. my *Caesar's Conquest of Gaul*, 1899, pp. 450, 476–7.

[2] *Geogr.*, ii, 3, § 13.

[3] Not to mention Iscalis, the site of which is unknown.

[4] Professor Rhys (*The Welsh People*, 1902, pp. 88–9), observing that, according to Caesar (*B. G.*, ii, 4, §§ 6–7), Diviciacus, King of the Belgic Suessiones, had established his hegemony in (Southern) Britain, and (*ib.*, 3, § 5) that the territories of the Suessiones and the Remi were practically one, argues that 'we should expect to find both of them represented in Britain, though their names have not been detected. Now,' he continues, 'we know from . . . inscriptions that a god of the Remi was Camulos'; and he points out that the name of this god is preserved in *Camulodunum*, or *Colchester*, the name of the chief town of the Trinovantes. The argument is not decisive, because Camulos was worshipped by other Gallic tribes as well as the Remi, and his name appears also in that of Camulogenus, a chief of the Aulerci (*B. G.*, vii, 57, § 3), who were not Belgae: nevertheless the Professor's conclusion may be right.

[5] p. 43.

[6] *The Welsh People*, 1902, p. 6. On the next page the professor adds that 'the Belgae probably occupied the whole of the coast on the east and south . . . from the Isle of Wight to the Firth of Forth'. It is clear therefore that in 1902 the Cantii were 'considered Belgic', although in 1884 and in 1904 there was 'no evidence' for this view.

[7] See pp. 459–60, *infra*.

[8] *Proc. Cambridge Ant. Soc.*, N. S., iv, 1904, pp. 478–9.

X. CONCLUSION

For the sake of clearness I shall summarize the results which this inquiry has attained. No human remains, except those of Bury St. Edmunds and Cattedown, which can be certainly attributed to the Quaternary Period have been found in Britain; but it is probable that the earlier inhabitants belonged in part to the Neanderthal stock, and that towards the close of the Palaeolithic Age they were joined by immigrants akin to the Chancelade people of the Lozère valley. There is no conclusive evidence that the earliest neolithic invaders found this island inhabited; but it has not been demonstrated that even here there was a 'hiatus' between the Old and the New Stone Age. The source of the first neolithic influx was probably in France, in the southern parts of which at all events the latest palaeolithic and the earliest neolithic inhabitants were akin. The neolithic invaders who built the long barrows of Southern Britain and the chambered cairns of Scotland, and many of whom built round barrows also, were a branch of the 'Mediterranean' race, and likewise came from France, perhaps in some cases originally from the Spanish peninsula; but if they are to be called 'Iberian' the term must be regarded as conventional. There is no evidence that they were related more nearly to the Basques than to some other branch of the Mediterranean stock.[1] They certainly spoke a non-Aryan language; and so probably did the earlier brachycephalic invaders, of whom the first comers landed in Britain before the end of the Neolithic Age. These invaders—the principal builders of the round barrows and the short cists—continued to arrive in successive hordes during the earlier part of the Bronze Age, some probably from Gaul, some from the Low Countries and the valley of the Rhine, and others, who settled in Yorkshire and Northumberland and perhaps in Derbyshire, from Denmark or Danish islands and possibly also from the Scandinavian peninsula. The brachycephalic Round Barrow skulls fall under two different types. Some resemble those of the French Grenelle race—in other words, the so-called Alpine race of Central Europe—and, like them, belonged to individuals of low stature; although the general superiority of the Bronze Age Britons in this respect is so great as to preclude the supposition that men of the pure Grenelle type invaded Britain in considerable numbers: others illustrate the rugged and, in some cases, almost brutal type which Thurnam and Rolleston have so forcibly described; and some of those of Yorkshire, especially Rudstone, and Northumberland exhibit these characteristics in such a degree that they may almost be grouped apart. The majority would seem to show that people of the two types intermarried, as they certainly did with the dolichocephalic neolithic population. The first Celtic invaders were Goidels,

[1] It has indeed been conjectured, as we have seen (p. 400, n. 3, *supra*), that the Basques were a distinct race.

who certainly reached Ireland in the Bronze Age, and who may be supposed to have settled in Britain also before the time of Pytheas. The first Brythonic immigrants probably inaugurated the Iron Age in this country, and began to arrive a short time before the visit of Pytheas. They were succeeded by the Belgae, who, like them, came in successive hordes, the first probably in the third century before Christ. The Belgae and the other Brythons spread over the greater part of Southern Britain and many parts of Scotland. Both they and the Goidels were doubtless mixed with people of the 'Iberian' and Grenelle races with whom they had intermarried before they left the Continent; but the purer representatives of the two Celtic stocks—the descendants of the invaders who had introduced the Celtic languages into Gaul and of their continental kinsmen—belonged to a type different from both of the Round Barrow types, being not only tall and generally fair but dolichocephalic or mesaticephalic. A people characterized by dolichocephaly and low stature, who apparently were not descended from the Long Barrow race, but whose affinities are doubtful, were settled in the Early Iron Age in East Yorkshire, and, it would seem, nowhere else in Britain. The Picts of Romano-British history were a medley of tribes, among whom Celts were, as everywhere, predominant, but who probably included a greater proportion of the descendants of the neolithic and other pre-Aryan peoples than any other British group. It is possible that in the remoter parts of Pictland a non-Aryan dialect was still spoken when the Romans invaded Britain; but the pre-Aryan Picts as a whole had been Celticized, and the Celtic language had prevailed, although it had been largely modified by the speech with which it had come in contact. Everywhere in Britain the pre-Roman stocks have, in greater or less proportions, survived.[1] Few Englishmen, Welshmen, or Scotsmen, if their pedigrees could be traced back far enough, would not be found to count among their ancestors men of the type who were buried in long barrows, sturdy warriors of the Bronze Age, and Celts who fought against Caesar or were subdued by Agricola.

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The study of ethnology is as fascinating to its votaries, partly by reason of its very difficulty, as the attempt to determine the distances of the less remote stars must have been to Bessel, Henderson, and Struve; but I can sympathize with those to whom, in both cases, the quest of knowledge for its own sake appears equally unprofitable. They may well ask the ethnologist why he does not proceed to deduce from what he knows conclusions that would interest all students of history and of human nature. 'There are few fields,'

[1] The late Professor F. W. Maitland (*Domesday Book and Beyond*, p. 222) argues that post-Saxon British survivors could not have been very numerous, as the Celtic language left 'few traces of itself'; but the same argument might be used to show that when the Romans came to Britain the Celts were few. See F. J. Haverfield, *The Romanization of Roman Britain*, pp. 9–12.

says Professor Bury,[1] 'where more work is to be done or where labourers are more needed than the Celtic civilisations of Western Europe. In tracing from its origins the course of western history in the Middle Ages, we are pulled up on the threshold by the uncertainties and obscurities which brood over the Celtic world. And for the purpose of prosecuting that most difficult of all inquiries, the ethnical problem, the part played by race in the developement of peoples and the effects of race blendings, it must be remembered that the Celtic world commands one of the chief portals of ingress into that mysterious prae-Aryan foreworld, from which it may well be that we modern Europeans have inherited far more than we dream.' But when we have entered the 'prae-Aryan foreworld', how shall we map out its various provinces, and what clue shall we have gained to the solution of 'the ethnical problem'? That is as complex as the problem, which theoretically may not be insoluble, of forecasting remote meteorological as accurately as astronomical phenomena; and its solution is more hopeless still. We want to know what contributions the various British races which we have identified made to the formation of the British character, which is so obviously different from that of any other nation, and which is, so to speak, the generalized manifestation of the characters of the English, Scottish, and Welsh peoples, and, descending the scale, of the characters of the inhabitants of every district, and finally of every man.[2] Again, we want to trace the manifold sources from which the 'Celtic' character, with the idiosyncrasies of which we are all more or less familiar, is derived. But the Celtic character is not everywhere the same. Study it in Wales, in Man, in the Scottish Highlands, in Ireland, in Cornwall, in France, and you will find that while it is Celtic everywhere, everywhere it is different;[3] that everywhere it has become what it is because it is compounded, in different degrees, not only of Celtic, not only of pre-Celtic and pre-Aryan, but also of post-Celtic elements. And all these elements have been modified and moulded by different geographical and climatic influences and by adventitious circumstances too numerous to be particularized and too elusive to be estimated.[4] Those who

[1] *An Inaugural Lecture*, 1903, pp. 39–40.

[2] Although Matthew Arnold was almost absolutely ignorant of ethnology, I do not know any book which ethnologists would find more suggestive than his *Lectures on Celtic Literature*.

[3] It has been truly said (*Journ. Anthr. Soc.*, 1870, p. xxxvi) that 'between even the Welshman and the Irishman there is a want of sympathy . . . fully equal to that which exists between either . . . and the most Teutonic Briton'.

[4] Mr. Alfred Nutt (*Folk-Lore*, xv, 1904, p. 234), commenting on a statement in Mr. Nicholson's *Keltic Researches* (p. iv) that Buckinghamshire and Hertfordshire are as Celtic (I should say as pre-Celtic) as South Wales and Ulster, asks whether it does not 'demonstrate the absolute futility of statistics of "relative nigrescence", or . . . size of skulls. The Bucks peasant,' he continues, 'may be physiologically akin to the man from Kerry or Glamorganshire; psychically he differs profoundly.' Yes, but this does not discredit the methods of physical anthropology: it only illustrates what I have said in the text. Between a certain number of individuals in Glamorganshire and a certain number in Buckinghamshire there is, let us assume, physical kinship:

know Ireland well have observed that the character of Anglo-Irishmen, whose blood is neither more nor less Celtic than that of many Englishmen, has acquired a quasi-Irish tinge, which is discernible in their children even when they have been born and bred in England; and this sets us thinking, though we think in vain. We all know the passage in which Mommsen compares the Gauls to the modern Irish: the ethnologist knows enough to see that it is as misleading as it is brilliant; but he knows too little to attempt to rewrite it. Anthropologically speaking, the Gauls (I use the word in its most comprehensive sense) were very different from the modern and indeed from the ancient Irish; and if Mommsen's analogy were more than superficial, we should be forced to conclude that the character of the Gauls, as it is revealed in ancient writings, was that of the dominant Celts, perhaps mostly Gallo-Brythonic, alone; and that the character of the Irish is simply that of Celts, mostly Goidelic, who were once but have long ceased to be dominant. Who will attempt to differentiate the respective shares of the pre-Aryan Long Barrow race, of the few representatives of the pre-Aryan Grenelle race who settled in this land, of the tall harsh-featured Round Barrow people, of the Goidels, and finally of the Brythons in building up the character which was to be further modified by the Roman, the Saxon, the Dane, the Norman, the Fleming, and aliens of every nationality, who each and all contributed something to a result which, influenced by the Continent, by the Far West, and now by the Far East, is still in process of evolution?

if we could isolate those two sets of individuals and compare them, instead of hastily comparing the populations of Glamorganshire and Buckinghamshire as wholes, we might find that the psychical difference was not as profound as Mr. Nutt supposes. Probably it would still be noticeable. But why? Partly because the physical resemblance is combined with a physical difference due to cross-breeding, the degree and nature of which it would be impossible to ascertain; partly because the environment, social, geographical, and climatic, of the peasants of Glamorganshire has for many centuries been very different from that of Buckinghamshire. Let two plum-puddings be made of identical sets of ingredients, but in slightly different quantities, in different kitchens, and by different cooks. The results will be very different. Or suppose that a thousand Spanish immigrants settled in Britain, and intermarried only among themselves. At the end of a century their physical and psychical types would have been modified. Nevertheless, handled with due skill and judgement, statistics of nigrescence and of cranial measurements retain their value.

THE NAMES *ΠΡΕΤΑΝΙΚΑΙ ΝΗΣΟΙ*, *BRITANNI* AND *BRITANNIA*

Πρεττανοί, which (written with a single τ) is supposed to represent the Brythonic or the Gaulish equivalent of a Goidelic word *Qrtanoi*—the assumed progenitor of the Irish *Cruthni*[1]—is found in certain manuscripts of Strabo[2] instead of the more usual Βρεττανοί: Diodorus Siculus[3] (who derived part of his information about the British Isles indirectly from Pytheas[4]) Strabo, Ptolemy,[5] and Marcian,[6] appear to have described them as Πρετανικαὶ νῆσοι, for perhaps they were not responsible for the ττ which appears in manuscripts; and Stephanus of Byzantium speaks of Πρετανίας and Πρετανίδες.[7] According to Professor Rhys[8] and M. d'Arbois de Jubainville,[9] the form Βρεττανικαί (νῆσοι), which occurs in most of the manuscripts, is to be accounted for by the fact that *Brittani*, the Goidelic name of the Brythonic invaders of Britain, which had no connexion with Πρετανοί, was eventually confounded with it: 'the confusion,' says Professor Rhys, 'is to be detected in the ττ of Πρεττανική;'[10] and he attributes it to scribes. The questions connected with all these names are very difficult. The first puzzle is this:—if, as the professor says,[11] Πρετανικαὶ νῆσοι, 'under the influence of the name of the Brythons, Βρεττανοί, became at last Βρεττανικαὶ νῆσοι, that is to say "Brythonic isles',' why did Diodorus Siculus, Ptolemy, and Marcian persist in calling them Πρετανικαὶ νῆσοι? Again, the professor's views about the Brittani, who, he tells us, called themselves Brittones, have lately undergone a sweeping change. In 1902[12] he regarded 'the first of the Belgic peoples to cross over to this country' as an offshoot of 'the Brittani or Brittones whom Pliny seems to have found so called in the valley of the Somme' (and whom, by the way, Pliny,[13] whatever he may have found them called, called neither Brittani nor Brittones, but Britanni); and he considered that their name, 'from being exclusively that of the first settlers, came to be extended to the successive hordes, so that at the last it actually denoted all the settlers

[1] See pp. 411–21, *supra*.
[2] *Geogr.*, ed. C. Müller and F. Dübner, 1853, p. 948, note to p. 97, line 22.
[3] See F. Vogel's ed. of 1888.
[4] See p. 499, n. 2, *infra*.
[5] *Geogr.*, ed. C. Müller, i, 1883, p. 74, note.
[6] i, 8 (*Geogr. Graec. min.*, vol. i, 1855, ed. C. Müller). Cf. *Rev. celt.*, xiii, 1892, p. 399.
[7] Ed. A. Meineke, 1849, pp. 186, 534. Cf. Pauly's *Real-Encyclopädie*, vol. iii, part i, 1897, p. 860.
[8] *Scottish Review*, xviii, 1891, p. 137.
[9] *Rev. celt.*, xiii, 1892, pp. 398–403.
[10] *The Welsh People*, 1902, p. 76.
[11] *Celtic Britain*, 1904, pp. 241–2.
[12] *The Welsh People*, 3rd ed., p. 6.
[13] *Nat. Hist.*, iv, 17 (31), § 106.

here of Belgic descent.' But the Britanni who are mentioned by Pliny were a Belgic tribe of such small importance that Caesar either ignored or had never heard of them; and, granting that some of them invaded Britain, of which there is no evidence, it is to the last degree improbable that they rather than the Belgae, of whom they would only have formed an item, should have imposed their name upon the people of the whole island. In 1902 Professor Rhys maintained that the Brythonic invaders of Britain were all Belgae. In 1904 [1] he distinguished the Belgae from the other Brythons, maintained that they were the second group of Brythonic invaders, and gave the name *Brittones* not only to them but also to their predecessors. Whether or not he still holds that these two groups of Brittones derived their name from Pliny's Britanni, I cannot say. If so, it is somewhat puzzling that one tribe of the so-called Brittones were the Belgae, who presumably called themselves after the Gallic group of tribes which, as a whole, was designated by that name: the professor's theory would lead to the startling conclusion that while a single horde of the second group of Brythonic invaders were called *Belgae* after the entire nation of which they were an offshoot, the two Brythonic groups of invaders, Belgic and non-Belgic alike, were conjointly called after the most obscure tribe of the second group! He explains the name *Brittones* as connected with the Welsh *breithyn*, cloth, and concludes that 'the word Brython and its congeners meant a clothed or cloth-clad people', and that 'the race with which the Brythons contrasted themselves to their own satisfaction, when they began to give themselves that name, was probably some of the aboriginal tribes whose home they invaded on the Continent'.[2] But if so, it seems wellnigh inexplicable that none of the Continental Belgae, none of the other Gauls, were called either Brittones or Brittani, and that only one petty Belgic tribe, which was unknown to or unnoticed by Caesar, was even called Britanni. As Windisch says,[3] Professor Rhys's etymology has to contend with serious difficulties; and it must, I think, be admitted that if the Brythons were called either Brittani or Brittones, the mention by Pliny of the Belgic Britanni throws no light upon the origin of the name. It is perhaps conceivable that, as Dr. Macbain [4] has suggested, 'the tribe on the Somme were some returned emigrants from Britain.'

Britain, says Professor Rhys, is traceable to *Britannia*, and *Britannia* to *Britanni*,—'the Latin name of the people'. He observes that the Greek form of *Britanni* was Βρεττανοί, and he adds that 'the practical identity between the Latin and Greek forms makes it probable that it was from or through the Greeks of Marseilles that the Romans first heard of these islands. This,' he continues, 'is not all, for the Latin *Brittanni*, and especially the Greek Βρεττανοί, have their exact counterpart in the Medieval Irish plurals Bretain,

[1] *Celtic Britain*, 3rd ed., p. 4.
[2] *Ib.*, pp. 211–4.
[3] *Allgemeine Encyklopädie der Wissenschaften*, &c., 35. Theil, 1884, p. 141.
[4] W. F. Skene, *The Highlanders of Scotland*, 1902, p. 384.

genitive Bretan, which had at times to function as the name both of the Brythons and of the island. It is to be noticed that neither Βρεττανοί or Britanni, nor the Irish Bretain has anything corresponding to it in the dialects of the Brythons themselves. From whom, then, did the Greeks hear the word which served as the basis of their names for Britain and its people? It cannot have been from the Brythonic peoples of the south-east of the island, or any, perhaps, of the Gauls of the Continent: it was probably from the natives of the south-west who brought their tin to market, and in whose country the only Celtic speech in use was as yet Goidelic. When, however, the Romans came to Britain they learnt the name which the Brythons gave themselves in the south-east of the island, and this was not Britanni, or Brettani, but Brĭttŏnes.'[1] On the other hand, Dr. Macbain[2] suggests that the 'Greek form Prettania [or rather *Pretania*, the form which is assumed to have been derived from *Priten* or its older equivalent, and indirectly from *Qrtanoi*] gave rise to the name Britain,—a bad Latin pronunciation'. Mr. Nicholson objects that 'in neither Greek nor Latin is *p* known to pass into *b*'.[3] Is it possible that the Latin pronunciation, if it was bad, may have been traceable to a bad Greek pronunciation, which gave rise to Polybius's[4] Βρεττανικαί (*νῆσοι*), and which was itself due to a defect not in pronunciation but in hearing?

THE BIRTHDAY OF RELIGION

Those who, like Professor Tylor, reject the theory that certain savage tribes have no religious belief would probably accept the evidence which Lord Avebury[5] adduces in its favour: only they attach to the word Religion a meaning different from his. Indeed he himself, in one passage,[6] uses the word in Professor Tylor's sense; for he remarks that 'one of the lowest forms of religion is that presented by the Australians, which consists of a mere unreasoning belief in the existence of mysterious beings'; and he admits that religion, in this sense, 'is general to the human race.'[7] Dr. Frazer, however, would apparently refuse to make even this concession. He is, or was, inclined to believe that 'faith in magic is probably older than a belief in spirits';[8] for 'magic is nothing but a mistaken application of the very simplest . . . processes of the mind, namely, the association of ideas by virtue of resemblance or contiguity', while 'religion assumes the operation of conscious or personal agents, superior to man, behind

[1] *Celtic Britain*, 1904, pp. 208–9.
[2] W. F. Skene, *The Highlanders of Scotland*, 1902, p. 384.
[3] *Keltic Researches*, p. 25, n. 1.
[4] iii, 57, § 3.
[5] *Prehist. Times*, 1900, pp. 540–51; *Origin of Civilisation*, 1902, pp. 220–4, 340–5.
[6] *Ib.*, p. 537.
[7] *Ib.*, p. 219.
[8] *Golden Bough*, 1900, i, 73, n. 2.

the visible screen of nature. Obviously,' he continues, 'the conception of personal agents is more complex than a simple recognition of the similarity or contiguity of ideas.'[1] I can only say that to me this is not obvious; the fancy of a primitive savage that fire, running water—everything that moves—is alive, is doubtless a less rudimentary mental act than the fear of a horse that a traction-engine is a formidable monster, but the difference is only one of degree.[2] And Dr. Frazer's definition of magic is singularly narrow: magic and religion were rooted in the same soil; and their branches intertwined.[3]

To M. Salomon Reinach also 'it appears evident that the true primitive savage . . . does not believe himself to be surrounded by spirits; he is in the state which Herbert Spencer calls *passive atheism* . . . The most backward primitive savages whom we know are in the neolithic age . . . The superstition (δεισιδαιμονία, dread of demons) which dominates their whole existence . . . is . . . the outcome of a long evolution.'[4] But did not the process begin when the primitive savage, conscious of life, fancied that sun and stars, flood and fire were also alive? And how can M. Reinach make it 'evident' that there ever was a savage so primitive that he had no such fancy? It is not true that the most backward savages whom we know, or at least have known, are in the Neolithic Age. The Tasmanians, a hundred years ago, were in their Palaeolithic Age, but they believed themselves to be surrounded by spirits.[5] Lord Avebury indeed affirms that 'some races entirely disbelieve in the survival of the soul after the death of the body';[6] nevertheless, if they believe in spiritual beings, they have the germ of religion.

For M. Reinach[7] religion was born at the moment when man, finding himself constrained to do what he feared might offend malignant spirits, began to devise means of conciliating them. But may it not be said with equal truth that the birthday of religion was when man began to form the conception, on which religion, in the ordinary sense, is based, that spiritual beings exist?

M. Reinach has recently pronounced that 'fire-worship preceded the use of fire, just as the worship of cereals preceded and prepared the way for their cultivation'.[8] One must infer that the 'true primitive savage', who, according to M. Reinach, was in a state of 'passive atheism', and therefore had not begun to worship fire, had not found out how to produce it. If M. Reinach is right, the 'passive atheist' must have been primitive indeed.

Professor Robertson Smith held that 'religion in the only true sense of the word' began 'not with a vague fear of unknown powers, but with a loving reverence for known gods who are knit to their

[1] *Golden Bough*, 1900, i, 70.
[2] See Sir A. Lyall's *Asiatic Studies*, ii, 1899, p. 236. [3] See p. 58, *supra*.
[4] *L'Anthr.*, xiii, 1902, p. 534.
[5] *Journ. Anthr. Inst.*, xxiii, 1894, p. 151.
[6] *Origin of Civilisation*, 1902, p. 391.
[7] *L'Anthr.*, xiii, 1902, p. 533. [8] *Ib.*, xvi, 1905, p. 658.

worshippers by strong bonds of kinship '.[1] But it was in the ' vague fear ' that the ' loving reverence ' had its germ.

Dr. J. G. Frazer, in a recent article [2], argues that the Australian aborigines have no religion : but by religion he means ' a propitiation or conciliation of the higher powers ' ; [3] and he admits that some Australian tribes ' have a notion of spiritual beings who can help or injure them '.[4] In other words, their belief fulfils Professor Tylor's ' minimum definition of religion ' ; and Professor A. C. Haddon justly remarks that ' it is doubtful whether more than a few anthropologists of repute would deny the term religion to the beliefs and practices of the Arunta ' of Central Australia.[5]

DUMBUCK, LANGBANK, DUNBUIE

I have said nothing in the first part of this book about the famous ' crannogs ', or pile-dwellings (so called), which were discovered a few years ago at Dumbuck and Langbank in the estuary of the Clyde, the hill-fort of Dunbuie by Dumbarton Castle, and the remarkable objects which they contained, because it is admitted that they belong to a period several centuries later than the Roman conquest of Britain ; but, for a reason which will presently be apparent, they must not be ignored. Everything worth reading that has been written upon the subject is included in two recent books—*Archaeology and False Antiquities*, in which Dr. Robert Munro contends that the disputed objects are spurious, and the *Clyde Mystery*, in which Mr. Andrew Lang endeavours to show that the difficulty of regarding them as forgeries is at least as great as the difficulty of maintaining their authenticity, and that, if they are genuine, they prove the survival of ritual and magical ideas that must have belonged to the Stone Age.

It may be premised that Professor Boyd Dawkins,[6] after a careful examination of certain engraved oyster shells, which were a part of the finds at Dunbuie, reported that he ' had satisfied himself that two of the shells were American blue points ', and, as he somewhat superfluously added, ' consequently of very modern date.' Mr. Lang, admitting this, suggests that, as Dunbuie was left unguarded for several months, the shells were introduced by some local wag.[7] At the same time he argues that if the disputed objects were not genuine, either the forger must have been a man of extraordinary erudition, who had studied the archaeology of England, America, France, Germany, Italy, Spain, Portugal, and Australia, or, by a coincidence which is incredible, he produced objects which are found in all those countries. I would suggest, however, that he may himself have

[1] *The Religion of the Semites*, 1901, pp. 54–5.
[2] *Fortnightly Rev.*, July, 1905, pp. 162–73.
[3] *Ib.*, p. 162.
[4] *Ib.*, p. 171.
[5] *Man*, vi, 1906, No. 49, p. 78.
[6] *Archaeol. Journal*, lx, 1903, pp. 209–10.
[7] *The Clyde Mystery*, pp. 138–9.

been a person of quite ordinary education, who was either employed by an archaeologist with a peculiar sense of humour or learned what was necessary for his purpose from some one more erudite than himself. Mr. Lang reminds us, further, that if the disputed objects have been found in Britain only in the basin of the Clyde, certain painted pebbles, similar to those of Mas d'Azil, which have been found in Scottish brochs, are also unique in Britain, and yet are disputed by nobody; and he might have said the same of the strange objects of Mycenaean type which were found in a barrow on Folkton Wold.[1]

Dr. Munro, on the other hand, can see no resemblance whatever between the disputed objects and the genuine productions of Australia or certain other questionable 'antiquities' that recently startled the explorers of a Portuguese dolmen.[2] The reader, as Mr. Lang says, must decide for himself; and I doubt whether he will see eye to eye with Dr. Munro.

The doctor also insists that if the Scottish objects are survivals, 'we ought to find, at least somewhere in Britain, decided and undisputed evidence of the existence of a phase of culture in the Stone Age in which the prototypes . . . would be the prevailing forms in general use. But,' he adds, 'of such archaic remains there is not a vestige.'[3] No; but the earth has not yet given up all the vestiges of the Stone Age: the first discovery of a Scottish interment of the Early Iron Age has been made within the present century,[4] and the doctor will admit that it is probably not unique; besides, do not the brochs and Folkton Wold suggest an answer to his argument?

Mr. Lang, concluding that at present the only position which the impartial *savant* can reasonably assume is a seat upon the proverbial fence, admits that 'the very strong point against authenticity is this: *numbers* of the disputed objects were found in sites of the early *Iron Age*. Now,' he continues, 'such objects, save for a few examples, are only known—and that in non-British lands—in *Neolithic* sites. The theory of survival may be thought not to cover the *number* of the disputed objects.'[5] May it not also be said that as an ignorant or sportive forger undoubtedly carved the oyster shells, so the disputed objects may have been smuggled into the sites by a forger who was well informed?[6]

[1] See p. 199, *supra*.
[2] *Archaeology and False Antiquities*, pp. 170, 259–60.
[3] *Ib.*, p. 245.
[4] See p. 435, *supra*.
[5] *The Clyde Mystery*, p. 141.
[6] See *Archaeology and False Antiquities*, pp. 255–6, and cf. pp. 229–30 with *The Clyde Mystery*, pp. 132–4.

INHUMATION AND CREMATION

Dr. R. Munro[1] says, on what authority I do not know, that the object of cremation was 'to liberate the spirit more quickly'. Is it then to be concluded that in cases where inhumation and cremation were practised simultaneously in the same barrow,[2] it was intended that certain spirits should be liberated quickly and others slowly?

Mr. W. C. Borlase[3] remarks that 'the transformation which would have taken place when incineration was introduced . . . would . . . have . . . been from a cult which was probably filthy and material to one which was pure and spiritual'. We have seen that the 'probably filthy' and the 'spiritual' cult were practised simultaneously by the same people; are we to assume that when inhumation was reintroduced in the Early Iron Age filth and materialism were revived?

Professor Boyd Dawkins[4] insists that cremation was introduced into Britain by 'the bronze-using Celtic tribes'; and Dr. Munro[5] apparently agrees with him. Putting aside the fact that most of the tribes to which the professor refers were not Celtic,[6] there is no evidence that cremation was first introduced by bronze-using tribes: if it was, the long barrows in which primary cremation interments have been found must have been erected in the British Bronze Age! It may or may not be true that, as Canon Greenwell suggests,[7] some of the Yorkshire round barrows were erected in the Stone Age; but at all events they were later than the long barrows of the same county. Those long barrows, according to Dr. Munro and Professor Boyd Dawkins, must have been erected after a bronze-using people had introduced cremation into Britain. How then would the professor and the doctor explain the fact that in the round barrows of the Yorkshire Wolds there was a reaction in favour of inhumation, seeing that Canon Greenwell[8] found in them 301 interments of unburnt and only 78 of burnt bones?

Dr. Munro[9] remarks further that, 'so far as available evidence has been adduced, it would appear that the only sepulchral remains, proved to have been older than the custom of cremation, are the chambered cairns in the south-west of England. When, however, the analogous cairns of Argyllshire, Caithness, and the Orkney Islands were constructed, the religious wave had already enveloped Northern Britain. Hence, though generally destitute of bronze relics, these structures were generally contemporary with the Bronze Age burials elsewhere in Britain . . . The explanation . . . is that in

[1] *Prehist. Scotland*, p. 474.
[2] See pp. 110, 185–6, *supra*.
[3] *Dolmens of Ireland*, iii, 743.
[4] *Early Man in Britain*, p. 366.
[5] *Prehist. Scotland*, pp. 476–80.
[6] See pp. 429–40, *supra*.
[7] *Brit. Barrows*, p. 409.
[8] *Ib.*, pp. 19–20, 22.
[9] *Prehist. Scotland*, pp. 478–9.

out-of-the-way localities . . . the Stone Age civilisation lingered longer than in those on the main routes of commercial intercourse.' Certainly; but no sepulchral remains in Britain are '*proved* to have been older than the custom of cremation'. Inhumation preceded cremation in Cornwall;[1] but there is no evidence that when inhumation was first practised there cremation was not practised in other parts of Britain. Though cremation was very rare in the chambered long barrows of Gloucestershire and Wiltshire, it was not unknown:[2] it was almost universal in the unchambered long barrows of Yorkshire; and it cannot be proved that they were later than the chambered long barrows of Wiltshire and Gloucestershire.[3] The chambered cairns of Scotland were not only 'generally' but absolutely 'destitute of bronze relics'. Very likely some of them may have been erected after the Bronze Age had commenced in Southern Britain; but even this can hardly be proved. What has been proved is that even in the Palaeolithic Age in the caves near Mentone cremation was already practised side by side with inhumation.[4]

[Since the rough draft of this note was written Professor Boyd Dawkins[5] has asserted that the long chambered barrow of Stoney Littleton belonged to the Bronze Age, while he admits, apparently because it did not contain cremated interments, that the long chambered barrow of Rodmarton was neolithic.[6]]

Professor Ridgeway's views, which are expounded in his well-known chapter, 'Cremation, Inhumation, and the Soul,' have been noticed in the first part of this book. In regard to Western usage he blunders in a way which makes me hesitate to accept his statements about archaeological details that I have not myself studied. He says that 'in Dorsetshire . . . the extended position seems to be the prevalent one',[7] a remark which I have already noted[8] as an instance of the danger of relying upon second-hand evidence; he implies that the invaders who 'conquered Dorset, Wiltshire, and Cornwall' in the Bronze Age were Belgae;[9] and he states that 'in France inhumation was universal before the age of metal',[10] which, as I have shown,[11] is contrary to fact.

[1] W. C. Borlase, *Dolmens of Ireland*, ii, 445.
[2] See p. 110, *supra*, and W. Greenwell, *Brit. Barrows*, pp. 448–51.
[3] *Ib.*, p. 536.
[4] See p. 204, *supra*.
[5] *Vict. Hist. of . . . Somerset*, i, 189.
[6] *Ib.*, p. 187.
[7] *Early Age of Greece*, i, 502.
[8] See p. 188, n. 2, *supra*.
[9] *Early Age of Greece*, i, 503.
[10] *Ib.*, p. 504.
[11] See p. 110, n. 1, *supra*.

SEPULCHRAL POTTERY

Some antiquaries have maintained that drinking-cups, food-vessels, incense-cups, and urns were not specially made for sepulchral purposes, but were merely ordinary domestic vessels.[1] On the other hand, it has been urged that most of them were too fragile to stand rough usage; that many are so contracted at the bottom that they would have been ill adapted to serve as table or culinary ware; that the food-vessels and the drinking-cups were too porous to hold fluid long, while the shape of most of them would have made them inconvenient for any ordinary purpose; and that all are wholly unlike the domestic pottery which has actually been found in hut-circles, forts, barrows, and the Heathery Burn Cave.[2] Mr. J. R. Mortimer[3] replies that drinking-cups and food-vessels were quite strong enough for domestic use; that 'the form of the typical drinking-cup is well chosen for the purpose its name implies, and most of the food-vessels are the prototypes of our . . . porringers, jars', &c. It may be admitted that some few food-vessels, for instance the one figured by Thurnam in *Archaeologia*, xliii, 381, are, apart from their decoration, not unlike domestic bowls; but what about incense-cups? The truth perhaps lies between the opposing views; for drinking-cups and food-vessels have been exhumed from pit-dwellings near Taplow;[4] and Pitt-Rivers,[5] speaking of an urn which was found on the bottom of the ditch of the camp in South Lodge Park in his estate, observes that 'it is more probable that the urn would be found in the ditch thrown away as refuse if it was in ordinary use, than if it were only fabricated for ceremonial purposes'. He remarks further that 'the large quantities of pottery of the same quality . . . afterwards found in different parts of the Camp, confirms this opinion [that sepulchral pottery was used for domestic purposes], as it could not all have been used for funeral urns'.[6] Moreover, fragments of ornamental pottery of the drinking-cup type were found by Pitt-Rivers in a pit in Martin Down Camp.[7] Still, the fact remains that only a very small proportion of the pottery which is commonly called sepulchral has been found outside sepulchres; and even it may have been intended for sepulchral use.

[1] *Archaeol. Cambr.*, 3rd ser., xiv, 1868, p. 291; *Archaeol. Journal*, xxvii, 1870, p. 156; W. Greenwell, *Brit. Barrows*, pp. 103–6.
[2] *Ib.*, pp. 103–8.
[3] *Forty Years' Researches*, pp. lxvii–lxviii.
[4] *Archaeologia*, xliii, 1871, p. 338; *Proc. Soc. Ant. Scot.*, xxxviii, 1904, pp. 335–6.
[5] *Excavations in Cranborne Chase*, iv, 30.
[6] *Ib.*, pp. 4–5.
[7] *Ib.*, p. 189.

STONEHENGE

Stonehenge has exercised the minds of many generations of antiquaries. An exhaustive bibliography, filling 169 pages and containing the titles of 947 books and articles, was published in the *Wiltshire Archaeological and Natural History Magazine* for 1901: but nearly all the works therein enumerated are obsolete; and any one who wishes to form an independent judgement will find all the necessary materials in the volumes which will be referred to in this article.

I. Modern opinion has for some time been tending to the conclusion that Stonehenge was erected, or at least began to be erected, in the Bronze Age. Excavation has proved that it did not exist before the use of copper or bronze, however uncommon it may have been, was known in this country;[1] and the arguments of Rickman,[2] James Fergusson,[3] and others who contend for a Roman or post-Roman date have been or can be demolished. To refute them in the text of this article would be useless; for no competent archaeologist now regards them as worth discussion.[4]

Dr. Arthur Evans maintains that Stonehenge was built in the earlier half of the third century before Christ, although some parts of it may be of later date; that 'sun worship was at most a secondary object in its structure'; and that it was 'one of a large series of primitive religious monuments that grew out of purely sepulchral architecture'. Let us first consider the question of date.

Dr. Evans has no difficulty in establishing, what has already been

[1] *Archaeologia*, lviii, 1902, p. 84.

[2] *Ib.*, xxviii, 1840, pp. 399–419.

[3] *Rude Stone Monuments*, pp. 8, 82–3. Of this famous book the Rev. W. C. Lukis (*Archaeol. Review*, i, 1888, p. 353) says that 'every copy should be committed to the flames'.

[4] See Lord Avebury's *Prehist. Times*, 6th ed., 1900, pp. 112–4, 122, and *Wilts. Archaeol. and Nat. Hist. Mag.*, xxiii, 1887, pp. 245–54. The theory of Mr. Edgar Barclay (*Stonehenge and its Earthworks*, 1895, pp. 40–1, 127–30), which ascribes the construction of Stonehenge to the time of Agricola, has been confuted by Professor Haverfield (*Classical Review*, x, 1896, pp. 74–5). The argument which Mr. Barclay (*op. cit.*, pp. 50–1) directs against the received view that it was pre-Roman, is based upon the fanciful assumption that it was designed in accordance with 'an ancient astrological figure', which rests upon the further assumption that 'all the salient measurements of Stonehenge may truly be said to result from an observation' of the sun. His argument (*ib.*, p. 88) that 'we have the testimony of an eye-witness, John Webb, that an iron spike was dug up near one of the trilithons from a depth of 3 feet', and that the circle must therefore have been erected after the close of the Bronze Age, would hardly impose upon a beginner. There is no evidence that this 'spike' (John Webb, *A Vindication of Stone Heng Restored*, 1665, p. 128) was made of iron: the circumstances in which it was found are not known: and, as we shall presently see (p. 477, n. 5, *infra*), an object manufactured in the nineteenth century has recently been unearthed within the precincts of Stonehenge at a depth much greater than three feet.

demonstrated in Part I of this book,[1] that 'Stonehenge was at least begun before the close of the Wiltshire "Round-Barrow" Period'.[2] At the same time he holds that 'its foundation belongs to the conclusion of this period'. He points out that 'of 36 disk-shaped barrows [in the neighbourhood of Stonehenge] 35 contained cremation interments . . . The number of glass beads contained in these barrows is also,' he continues, 'evidence of their comparatively late date[3] . . . The general inference which we draw from the intimate structural connexion between Stonehenge and these disk-shaped barrows is, that the great stone circles themselves were erected towards the close of the Round-Barrow Period. The proportionately frequent occurrence of gold relics in barrows in the immediate neighbourhood of Stonehenge, 4 out of 5 such discoveries having been made within half-a-mile of this monument, points in the same direction.' And if it should be argued that the barrows may have been built after the erection of Stonehenge, his answer would be that 'the barrows themselves, with the exception of the two within its own area, are disposed without any reference to Stonehenge, and do not in any way cluster about it, as we might reasonably have expected them to do had the bulk of them been reared after the Stone Circle'. Dr. Evans then observes that an amber collar 'found in one of the Lake barrows about two miles from Stonehenge . . . is of a form and arrangement identical with the amber necklaces found in the great cemetery at Hallstatt, and from the similar character of the boring of the beads must in all probability have come from the same centre of manufacture'; and he endeavours to show that we may infer from recent discoveries that 'a large proportion of the Hallstatt remains reach down to the period between the approximate dates of 450–300 B.C.'[4] On the other hand, Late Celtic antiquities, which began to appear in Britain 'at least as early as the second century B.C.', are absent from the barrows of Wiltshire; and the latest date which can be assigned to these barrows is about 250 B.C. Dr. Evans concludes that 'we may approximately refer the foundation of Stonehenge to the end of the fourth or beginning of the third century'.[5]

Some of these arguments do not appear to have much weight. Dr. Evans himself admits that there is a great structural distinction between Stonehenge and the disk-shaped barrows:[6] in the latter the surrounding ditch is *inside* the bank; in the Stonehenge vallum it is *outside*. Those barrows at all events in which chippings of the stones were found were later than Stonehenge;[7] and whatever con-

[1] See pp. 215–6, *supra*.
[2] *Archaeol. Review*, ii, 1889, pp. 320–2.
[3] Cf. p. 183, *supra*.
[4] The Hallstatt period is now believed to have ended about 400 B.C. See p. 229, *supra*.
[5] *Archaeol. Review*, ii, 1889, pp. 322–3, 324–5.
[6] *Ib.*, p. 321.
[7] Pottery was unearthed in 1802 close to the 'Altar Stone' from a depth of 5 feet or more by Sir R. C. Hoare's collaborator, W. Cunnington, who described it as 'similar to the rude urns found in the barrows' (W. Long, *Stonehenge and its Barrows*, 1876, p. 86).

clusion may be drawn from the arrangement of the barrows, their number is so great as to suggest the inference that many of them were erected there because Stonehenge was regarded as a holy place. In a recent article,[1] however, Dr. Evans has reinforced his argument, pointing out that the circle called the Rollright Stones, which stands on a hill overlooking the valley of the Warwickshire Stour, also stands 'in immediate relation to a large group of [disk-shaped] sepulchral barrows', and giving additional and conclusive evidence as to the late date of these particular monuments. He would not, however, I believe, now assign to Stonehenge quite so early a date as the 'beginning of the third century'; for the commencement of our Early Iron Age is commonly referred to about 400 B.C.[2] Of the two barrows in which Hoare found chippings of the Stonehenge stones one was bell-shaped,[3] the other belonged to the kind which he called 'flat',[4] but which, as Thurnam points out,[5] is simply a variety of the bowl-barrow; but Thurnam has given reasons for believing that many barrows of this form may not have been earlier than disk-shaped mounds.[6]

In 1901 excavations were made at Stonehenge, but only in 'a fraction of the whole site', under the superintendence of Professor W. Gowland.[7] The principal objects discovered were chippings from the 'sarsens' and 'blue-stones'; more than one hundred stone implements, many of which were of flint, and had evidently been used for dressing the softer stones of the monument, while others consisted of 'the hard quartzite variety of sarsen'; bones of domestic animals; 'splinters of antlers of deer'; 'a portion of a large antler with its lowest tine worn away,' apparently from its having been used as a pick; and Roman coins, which, however, were only found in the superficial layers.[8] 'The layers of the excavations,' says Professor Gowland, 'in which the flint and stone tools were found was absolutely undisturbed ground';[9] and the chippings were found as far down as the surface of the bed rock. Only one trace of copper or bronze was visible, namely a stain, described by the formula $CuCO_3$, on a sarsen block, seven feet below the surface. The work of trimming the stones appears to have been done with stone implements only. The copper stain, however, proves that copper or bronze must have been in use at the time when the builders of Stonehenge were at work. Professor Gowland[10] affirms that the stain 'can only have been produced by prolonged contact with some very small object of copper or bronze or some material containing copper . . . It may perhaps have been an ornament, but cannot possibly

[1] *Folk-Lore*, vi, 1895, pp. 6–51, and especially 14–6.
[2] See p. 232, *supra*.
[3] *Ancient Wilts*, i, 127.
[4] *Ib.*
[5] *Archaeologia*, xliii, 1871, p. 291.
[6] *Ib.*, p. 301. One of the two barrows in which chippings of the Stonehenge stones were found contained a bronze 'spear-head' or dagger, and a bronze pin.
[7] See *Man*, ii, 1902, No. 6, pp. 7–11, and *Archaeologia*, lviii, 1902, pp. 37–118.
[8] *Ib.*, pp. 51, 53, 55, 57, 62, 65–6, 71–2, and fig. 24.
[9] *Man*, ii, 1902, No. 16, p. 25.
[10] *Ib.*, p. 24; *Archaeologia*, lviii, 1902, p. 84.

have been an implement.' He argues, further, that 'even if metal tools were of no use for this particular work, it is difficult to believe that, if the monument were of the Bronze Age, no bronze implement would have been lost in the course of its erection'.[1] I, on the other hand, would remind him that no bronze has been found in the hut-circles of Dartmoor or in various cemeteries which undoubtedly belonged to the Bronze Age,[2] and I suggest that if the workmen who built Stonehenge had no use for bronze tools when they were building it, they were not more likely to lose them on the site than the masons who built St. Paul's Cathedral to drop their table knives within the area of the churchyard. However, Professor Gowland does not pin his faith upon this argument. He points out [3] that many of the flint implements which he discovered at Stonehenge closely resemble those which were discovered by Canon Greenwell at Grime's Graves, and which were attributed by him to the close of the Neolithic Age, or, at the latest, to a period when bronze had not come into general use.[4] But nobody who has learned the ABC of archaeology needs to be told that stone implements were used long after the introduction of bronze;[5] and no expert sees anything improbable in the theory that such tools were used in constructing Stonehenge towards the end of the Bronze Age. Besides, as Professor Gowland admits, Dr. Maskelyne has pointed out that 'bronze tools would not work sarsens';[6] assuming, then, that Stonehenge was erected in the Bronze Age, how could the sarsens have been dressed except with implements of stone? But the discovery upon which Professor Gowland lays the most stress is that of the deer-horn pick. Similar picks were found in large numbers at Grime's Graves. The one which Professor Gowland found, if it really was a pick, must have been used for excavating the pits in which the stones of Stonehenge were erected; and Professor Gowland argues that if bronze tools had been in use at the time, 'it would seem not unreasonable to assume that they would have been employed, as they would have been so much more effective for such work than the picks of deer's horn.'[7] But no bronze pick has ever been found in this country; and deer-horn picks have been found in interments of the Bronze Age,[8] and even in a Romano-British deposit in the village of Woodyates on Cranborne Chase.[9] Professor Gowland provisionally assigns the date 'about 2000–1800 B.C.' for the erection of Stonehenge; and he adds that

[1] *Man*, ii, 1902, No. 6, p. 9.
[2] See p. 215, *supra*, and *Guide to the Ant. of the Bronze Age* (Brit. Museum), pp. 46–7.
[3] *Archaeologia*, lviii, 1902. pp. 63–5.
[4] Dr. Evans, however, insists (*Man*, ii, 1902, No. 16, p. 22) that 'amongst all the stone implements discovered [by Prof. Gowland] there was nothing distinctly neolithic'.
[5] See pp. 71–2, 129, 131, *supra*.
[6] *Man*, ii, 1902, p. 10.
[7] *Archaeologia*, lviii, 1902, p. 86.
[8] W. Greenwell, *Brit. Barrows*, pp. 37, n. 1, 258, 304, 329, 432; *Archaeologia*, liv, 1895, p. 89.
[9] A. Pitt Rivers, *Excavations in Cranborne Chase*, iii, 135.

Sir Norman Lockyer's astronomical calculation 'gives an approximate date . . . of 1680 B.C., with a margin of error ± 200 years'.

More than one attempt has been made to determine the date of Stonehenge from the orientation of its axis; and these attempts have been founded upon the assumption that one, at all events, of the objects which the builders had in view was the worship of the sun. 'The chief evidence,' as Sir Norman Lockyer and the late Mr. F. C. Penrose have observed, 'lies in the fact that an "avenue" . . . formed by two ancient earthen banks, extends for a considerable distance from the structure, in the general direction of the sunrise at the summer solstice.'[1] On the avenue, 100 feet from the so-called Slaughter Stone, stands a large monolith, called the 'Friar's Heel', or the 'Heel Stone'. At one time it was generally assumed that on Midsummer Day, at the time when Stonehenge was built, an observer, standing on or behind the 'Altar Stone', could see the sun rising above the tip of the Heel Stone. At the present time, however, as Mr. Arthur Hinks points out, 'the sun rises further south than it has done for the last ten thousand years'; and yet, from the point of view of an observer standing behind the Altar Stone, 'it still rises north of the stone.'[2] In fact 'it is some seven days before or after midsummer day when it rises directly over the stone'.[3] Moreover, as Professor Flinders Petrie[4] says, the 'skew position' of the Altar Stone would seem to show that it is not now in its original position. Accordingly Sir Norman Lockyer felt obliged to leave the Friar's Heel out of his calculations, and to confine himself to attempting to determine the orientation of the avenue.[5] The method which he and his colleague adopted was to peg out as accurately as possible 'the central line between the low and often mutilated banks' of the avenue, and then to measure 'the bearings of two sections of this line near the beginning and the end'.[6] 'The resulting observations,' he tells us, 'gave for the axis of the avenue nearest the commencement an azimuth of 49° 38′ 48″, and for that of the more distant 49° 32′ 54″.'[7] But neither of these measurements was adopted by Sir Norman. He found, or thought that he found, that the mean between the two values which he had obtained, namely, 49° 35′ 51″, was 'confirmed by the information, supplied by the Ordnance Survey, that from the centre of the temple [Stonehenge] the bearing of the principal bench mark on the ancient fortified hill, about eight miles distant, a well-known British encampment named . . . Sidbury, is 49° 34′ 18″; and that the same line continued through Stonehenge to the south-west strikes another ancient fortification, namely, Grovely Castle, about six miles distant, and at practically the same azimuth, viz., 49° 35′ 51″. For the above reasons,' he says, '49° 34′ 18″ has been adopted for the azimuth of the avenue.'[8]

[1] *Nature*, Nov. 21, 1901, p. 55.
[2] *Nineteenth Century*, June, 1903, p. 1003.
[3] *Ib.*, p. 1002.
[4] *Stonehenge*, 1880, p. 18.
[5] *Nature*, Nov. 21, 1901, pp. 55-6.
[6] *Nineteenth Century*, June, 1903, p. 1008.
[7] *Nature*, Nov. 21, 1901, p. 57.
[8] *Ib.*

Having regard to the rate of change in the obliquity of the ecliptic,[1] he concluded that the date of the foundation of Stonehenge was 1680 B.C.; but he admits that this date 'may possibly be in error by ±200 years'.[2]

It would appear then that, if Sir Norman Lockyer's calculations are well founded, Stonehenge was erected at some time between 1880 and 1480 B.C. Certainly the conclusion does not err on the side of excessive precision. But the foundation upon which the calculations rest has been shown by Mr. Hinks to be rotten. To begin with, the assumption that Sidbury Hill was connected with the erection of Stonehenge is absurd. Does Sir Norman Lockyer mean to suggest that the bench mark was prehistoric? 'In our climate,' says Mr. Hinks, 'Sidbury is probably not visible from Stonehenge at sunrise once in twenty years.'[3] In point of fact it is never so visible: only the trees on the top of the hill are to be seen. Furthermore, as Mr. Hinks points out, Sir Norman Lockyer has assumed that 'for [the temple of] Karnak the moment of sunset was when the sun's centre had just reached the horizon; for Stonehenge sunrise was the moment when the first tip of the sun appeared above the hill. It was necessary to adopt these precise yet different phases for the two cases, because any other assumptions would have led to results obviously absurd.'[4] Finally, Sir Norman Lockyer is obliged to

[1] This phenomenon is explained in Sir N. Lockyer's *Elementary Lessons in Astronomy*, 1889, §§ 549-54.

[2] *Nature*, Nov. 21, 1901, p. 57.

[3] *Nineteenth Century*, June, 1903, p. 1009. See also *Nature*, lxviii, 1903, p. 180. (On the 22nd of June, 1903, a correspondent of the *Times* wrote from Salisbury, 'For the first time for nearly ten years visitors at Stonehenge yesterday morning saw the sun rise.')

[4] *Nineteenth Century*, June, 1903, p. 1009. I cannot deny myself the pleasure of quoting Mr. Hinks's criticism of Sir Norman Lockyer's argument. 'The authors [Sir Norman and Mr. Penrose] are trying,' he says, 'to find the place of a prehistoric sunrise by assuming that the avenue pointed to it. They measured the direction of the avenue, and found that the measures agreed so very nearly with the Ordnance Survey measure of the direction of their mark—presumably on the highest point—at Sidbury camp, that they adopted the latter measure rather than their own; in other words, they agreed that the avenue is directed very exactly to Sidbury. Henceforward one cannot leave Sidbury out of the argument . . . There are two courses open to us. On the one hand we may suppose that the avenue was drawn to lead over the down to Sidbury camp, and had no intentional relation to the place of sunrise. On the other hand we may suppose that Sidbury is in the sunrise line not by accident but by design; that it forms an integral part of the solar temple of Stonehenge. And since the camp occupies the summit of a steep and isolated hill, while Stonehenge lies on a wide and gently sloping down, it is plain that the camp end of the Stonehenge-Sidbury line must have been fixed first, and the site of the temple determined by prolonging the line sunrise-Sidbury till it struck a suitable place on the down. There is nothing impossible in this; the question is, Can it be said to be so probable that one is justified in fixing a date for Stonehenge from the direction of the line so drawn? Which is the greater improbability, that the Stonehenge-sunrise line was laid out so that it passed over the peak of Sidbury hill . . . so nearly invisible from Stonehenge by reason of an intervening down that Sir Norman Lockyer thought that the latter formed the local horizon, and makes no mention of having seen Sidbury over its top . . . or that the line of an avenue setting out from Stonehenge happens to point to

assume that the builders of Stonehenge could tell the exact day on which the midsummer solstice occurred. The utter improbability of this assumption must be apparent to any one who remembers that the astronomer who constructed the Julian calendar miscalculated the dates both of the summer and of the winter solstice.[1]

Mr. E. J. Webb, whose brilliant article in the *Edinburgh Review* of October, 1894, demolished Sir Norman Lockyer's theory as to the orientation of the Egyptian temples,[2] has written me a letter in which the futility of attempting to determine the date of Stonehenge by astronomical reasoning is explained with a clearness which leaves nothing to be desired. 'As,' he writes, 'the sun in our latitudes does not rise at right angles to the horizon, but with a considerable slant, it follows that the place where his upper rim begins to appear is appreciably further towards the north than the place where his centre appears, and this again than the place where he is first seen fully risen,—that is, where his lower edge touches the horizon. Now I think myself that, even if we could credit the builders with complete accuracy, attempts to get the date of the building astronomically would be vain, because (1) we do not know the exact place (if such there was) at which the observer's eye was supposed to be placed. (Flinders Petrie does to some extent get over this difficulty by supposing that the observer took up a position from which the point of the Heel Stone appears exactly level with the horizon. I doubt, however, whether we have a right to be sure that the point is exactly where it was at first. Some of the stones have leaned over considerably, and why not this? But the difficulty is much greater for Lockyer, who takes no account of the Heel Stone.) (2) We do not know whether the ancients would have understood by the moment of sunrise the moment when the sun's upper rim appears (*A*), or the moment when his centre appears (*B*), or the moment when his lower rim appears (*C*). (3) Even if we did know this, yet, as every one who has watched the sun rise must admit, it is practically quite impossible to be certain when any one of these moments occurs. Lockyer tacitly admits this when he arbitrarily takes as the moment of first appearance the time when 2′ (about $\frac{1}{16}$) of the sun's disc are risen.

'It is clear that (*A* being assumed) when Stonehenge was built, an

the place where the sun rose at a date which is perhaps as likely as any other for the foundation of the building . . . ?

'If preference be given to the first alternative, and we assume that Stonehenge really was so placed that Sidbury marked the point where the sun rose on midsummer morning, the question still remains, Was it done so accurately that it is worth measuring accurately now, and drawing from the measures an exact statement of date? It may well be objected that in our climate Sidbury is probably not visible from Stonehenge at sunrise once in twenty years, and that the likelihood of a long delay in drawing out the plan of so great a work would very soon have induced the builders to adopt a line near enough for their purposes though not for ours . . . And lastly there is the grave difficulty that everything depends upon guessing right what is to be considered the critical phase of the sunrise or sunset,' &c.

[1] *Journal of Philology*, xxix, 1903, pp. 94, 113.

[2] This article is mentioned *honoris causa* by Mr. Hinks (*Nineteenth Century*, June, 1903, p. 1005).

observer looking along Flinders Petrie's line of sight would see the Friar's Heel considerably to the south of the place of sunrise, inasmuch as, though that place has ever since been moving southwards, we see it slightly to the south even now. Lockyer therefore puts the Friar's Heel out of his theory altogether, in the belief that a stone which did not exactly mark the place of the most northerly sunrise could be of no use. I think, on the other hand, that a stone placed a little too far south would probably suit what is likely to have been the purpose in view even better than one which exactly marked the solstitial sunrise. For if, instead of asserting that Stonehenge was roofed over, and a beam of light admitted at the moment of sunrise to its darkened sanctuary[1]—all of which is pure guesswork—we suppose that the builders were contemplating merely such bonfires and rejoicings as, by Lockyer's own admission, certainly have taken place in various parts of the world, may we not ask how people in those ancient days knew when these festivities were to be held. For the later days, of which we have knowledge, the answer is easy enough: then people had the Julian calendar, according to which St. John's Day, or whatever day was selected, always recurred at the same place in the solar year, whether at, or before, or after the solstice. But we do not know whether in pre-Roman times the inhabitants of Wiltshire had any settled calendar at all; and if they had, it is probable that, as in almost all ancient calendars, the days of the month, and therefore most likely the festivals, were reckoned by the moon.[2] The fifteenth, let us say, of a particular month meant the day when a particular moon was fifteen days old; and if this day should coincide in one year with the solstice, it would not coincide with it the next year, and could not have coincided with it the year before. How then could people tell when the Midsummer festival ought to be held? I answer that they might have very easily done so some time beforehand by the aid of a stone set up so as to mark, not the solstitial sunrise itself, but an earlier—and therefore of course also a later—one. If the Friar's Heel stood, as on Lockyer's theory it did, some little way to the south of the place of the midsummer sunrise, then the sun must have risen over it twice—first towards the end of his journey north, just before the solstice, and secondly on his return southward, just after the solstice. Now if the Stonehenge people looked out for the morning on which the sun first seemed to rise over the stone, and counted the days to a morning when he seemed to rise there again on his return journey, they could, by halving this number, obtain the time of the solstice with as much accuracy as they could have required. After doing this once, they could in following years always know, by watching the sun's first approach to the Friar's Heel, for what day to appoint the midsummer rejoicings. That these rejoicings took place at sunrise I do not assert. Bonfires, at least in these times, usually take place at night . . . I do not think we have any right to say with certainty that any solstitial festival ever took place at Stonehenge or near it. For even granting that,

[1] *Nature*, Nov. 21, 1901, p. 55.

[2] See p. 296, *supra*.

as seems not unlikely, Stonehenge was orientated more or less closely to the solstitial sunrise, and that the Friar's Heel was really used for the observation of the sun, it does not follow that Stonehenge was a "solar temple" any more than Milan Cathedral, which is orientated more or less closely to the equinoctial sunset, and has had a meridian line traced upon its pavement. And even if we knew that it was a solar temple, we should have no right to infer what kind of worship went on there.'[1]

Although Dr. Evans's arguments are not all equally strong, there can be little doubt that his view as to the date of the erection of Stonehenge is approximately correct. The stones were certainly not standing when round barrows were first erected on Salisbury Plain; for one is contained within the *vallum*, which, moreover, encroaches upon another.[2] Mr. F. R. Coles has shown that 'so far as direct evidence has been obtained by rightly conducted excavations, the outstanding feature of all the Scottish circles that have been investigated is the presence within them of interments of the Bronze Age'.[3] That Stonehenge was erected before the close of

[1] Professor Montelius accepts and endorses Sir Norman Lockyer's conclusions. Most of the barrows near Stonehenge belong, he says (*Archiv für Anthr.*, N. F., ii, 1904, p. 140) to the earliest period of the Bronze Age, which, in the south of England, began, in his opinion, about 2000 B.C. He goes on to speak of the chippings of the Stonehenge stones which have been found in two of the surrounding barrows, and affirms that to those who know the epoch to which Stonehenge belongs it is evident that it was a temple, for sepulchral monuments 'have a different appearance' (*sahen nicht so aus*); and finally he mentions the results at which 'some of England's greatest astronomers have arrived'.

Alas that a great archaeologist should meddle with what he does not understand! 'Some of England's greatest astronomers' is presumably a rhetorical synonym for Sir Norman Lockyer: at all events the results which so appeal to Professor Montelius's and Sir Norman's imagination stand to Sir Norman's credit alone. Let me recommend the professor to read the article in the *Nineteenth Century* of June, 1903, in which another astronomer has demolished them. The only novelty in the professor's article is the implied statement that the barrows in which the chippings were found are not much later than 2000 B.C. On this point he is of course entitled to a respectful hearing: but the mere amateur who remembers that the two barrows in question are assigned by one of England's greatest archaeologists to about 300 B.C. will, I fear, shrug his shoulders; and Mr. Abercromby (see p. 183, *supra*) has proved that many of the Wiltshire barrows were later than 800 B.C. The remark that sepulchral monuments have a different appearance from Stonehenge is not helpful, seeing that Stonehenge is unique among megalithic circles. I can only repeat that many such circles have been proved to be sepulchral monuments; and, as I shall show presently, there is evidence that Stonehenge was a scene of sepulchral rites.

[2] Sir R. C. Hoare, *Anc. Wilts*, i, 144–5; W. Long, *Stonehenge and its Barrows*, p. 86.

[3] *Proc. Soc. Ant. Scot.*, xxxiv, 1900, p. 197. See also xxxv, 1901, pp. 194, 219; xxxvi, 1902, pp. 131, 579; and Joseph Anderson, *Scotland in Pagan Times: the Bronze and Stone Ages*, p. 118. As I have already remarked (p. 207, *supra*), the date of many English circles remains uncertain; and I admit that some may be of locally late neolithic age, though I doubt whether any were erected before the oldest bronze implement was introduced into Southern Britain. Mr. H. St. George Gray (*Archaeologia*, lviii, 1903, pp. 461–98) regards the well-known monument of Arbor Low, near Bakewell in Derbyshire, as

this period, or at all events before the dawn of the Iron Age in Wiltshire, is certain; and, as it was the most elaborate and highly finished of all the stone circles of Great Britain, we may fairly infer that it was one of the latest of them all.

II. The most interesting pages of Dr. Evans's article are those in which he attempts to trace the pedigree, so to speak, of Stonehenge, and to divine the purpose of its builders. He cites instances to show that 'wherever the meaning of these great stone monuments has been clearly revealed to us, we find them connected either directly or indirectly with sepulchral usage'.[1] He contends that in the most characteristic examples 'the Circle is an enlarged version of the ring of stones placed round the grave-mound; the Dolmen represents the cist within it; the Avenue is merely the continuation of the underground gallery, which in our earliest barrows leads to the sepulchral chamber'.[2] But is there any evidence that interments ever did take place within the precincts of Stonehenge? General Pitt-Rivers remarked that the question could be definitely settled by excavation;[3] but scientific excavation, as we have seen, has hitherto been confined within a small area. The evidence amounts to this:—a vessel, which Dr. Evans calls an incense-cup, was discovered by Inigo Jones,[4] and incense-cups have never been found except in association with interments;[5] while the numerous bones of domestic animals which have been exhumed, along

belonging to 'the period of transition from stone to bronze'. This circle has been excavated to a considerable extent. No metal was discovered, nor any pottery that could be assigned to the period of construction; but a barbed and tanged arrow-head was found on the bottom of the ditch. Arrow-heads of this kind were probably first manufactured later than the non-barbed varieties (see p. 81, *supra*), although many specimens of the latter were contemporary with the former. As I have already pointed out, the mere absence of bronze in a circle is not sufficient to prove that it did not belong to the Bronze Age: the excavation of Arbor Low was necessarily incomplete; and all that can be said with certainty is that it is not older than the period to which Mr. Gray ascribes it. The reasons which he gives (*Man*, vi, 1906, No. 101, p. 159) for presuming that the Stripple Stones in Cornwall were of the same date appear to me equally inconclusive.

[I find that Mr. Gray (*Report of . . . the Brit. Association*, 1902 [1903], p. 465) admits that 'Arbor Low has not been disproved to be of Early Bronze Age date', and that his conclusion rests 'on somewhat meagre evidence'. It has, however, been pointed out (*ib.*, p. 466) that 'a Bronze Age tumulus was certainly constructed out of material *derived from a portion of the original structure* of the earthwork enclosing the stone circle', and therefore that 'it is reasonable to assign the date of construction of the circle to a period *not later* than the *early* Bronze Age'.]

[1] *Archaeol. Review*, ii, 1889, p. 313. See pp. 211–2, *supra*.

[2] *Ib.*, pp. 313–4. [3] *Journ. Ethn. Soc.*, ii, 1870, p. 2.

[4] *Archaeol. Review*, ii, 1889, p. 322.

[5] Professor Gowland (*Archaeologia*, lviii, 1902, p. 85) holds that the discovery of the 'incense-cup' proves nothing, 'as the nature of the ground and the conditions under which it was found are not given . . . In Excavation VI', he dryly remarks, 'I dug up a modern preserved meat tin from a much lower layer than the stone implements in the neighbouring undisturbed ground.' Dr. Evans, however, who apparently anticipated this objection, holds (*Archaeol. Review*, ii, 1889, p. 322), that if the cup had not been originally deposited in the place where it was found, it would have been broken. I cannot find any

with charcoal and fragments of pottery, from the interior circle,[1] point to the conclusion that Stonehenge was the scene of sepulchral rites such as we know to have been performed in barrows.[2] Furthermore, the older monument of Avebury contains two smaller stone circles, within each of which are the remains of a stone chamber, which, Dr. Evans argues, 'there can be little doubt once contained interments.'[3] But Dr. Evans is at no great pains to argue that Stonehenge was itself a cemetery: it is on its connexion, close or distant, with sepulchral usage that he lays stress. While he points out that 'in the case of the Chambered Barrows the [surrounding] stones may be said still to fulfil an original structural function', he holds that 'in the case of the Circles they bear a more purely ritual signification. In some cases,' he adds, 'we find transitional examples in which the stone circle is actually seen in the act as it were of separating itself from the earth barrows. Thus in the great monument of New Grange [in Ireland] the stone circle is separated by an interval of some twenty feet from the central mound.' Then, going to the Far East for an illustration, he tells us that, while the stone circles and dolmens which are still erected by the Khasis of Assam 'are in themselves non-sepulchral', they 'are reared as a propitiation either to the departed Spirits of their own ancestors or to any other Spirit'.[4] But Dr. Evans does not deny that Stonehenge was also a solar temple: he admits, indeed, that its orientation 'certainly seems to associate the Sun in the religion of the spot'.[5] This theory is supported by observations made by Professor Gowland in Japan. 'There,' he tells us, 'on the seashore at Futa-mi-gaura . . . the orientation of the shrine of adoration is given by two gigantic rocks which rise from the sea as natural pillars. The sun, as it rises over the mountains of the distant shore, is observed between them, and the customary prayers and adorations made. . . the point from which the sun is revered is marked by a structure of the form of a trilithon,'[6] &c.

But although some evidence has been collected in support of the theory that certain stone circles in the British Isles and elsewhere

proof that the so-called incense-cup was an incense-cup, in the sense in which archaeologists use the term, at all. It is described by John Webb (*A Vindication of Stone Heng Restored*, 1665, pp. 127–8) as 'the *Cover* highly probable of a *Thuribulum* . . . It was of Stone, light in comparison, the more by being hollow, and extream hard.' Now incense-cups were not made of stone (though fragments of stone were often mixed with the clay of which they were baked), and they hardly ever had covers (*Archaeologia*, xliii, 1871, p. 383; W. Greenwell, *Brit. Barrows*, p. 164, note; *Guide to the Ant. of the Bronze Age* [Brit. Museum], pp. 61–3). The word 'Stone' may have been used incorrectly; but if the 'Thuribulum' was really stone, it was perhaps of late date. Cf. Sir J. Evans's *Anc. Stone Implements*, 1897, p. 471.

[1] W. Long, *Stonehenge and its Barrows*, p. 86; *Archaeol. Review*, ii, 1889, p. 318. See p. 469, n. 7, *supra*.

[2] See pp. 202, 212, n. 2, *supra*.

[3] *Archaeol. Review*, ii, 1889, p. 315.

[4] *Ib.*, p. 318.

[5] *Ib.*, pp. 315–6. See also *Trans. Ethn. Soc.*, iv, 1866, pp. 251–3.

[6] *Archaeologia*, lviii, 1902, p. 88.

were orientated more or less closely to the Midsummer sunrise, it does not necessarily follow that they were solar temples ;[1] and a scientifically conducted examination of the circles of Kincardineshire and Aberdeenshire has shown that their main diameters ' are in scarcely any instance oriented (*sic*) to any point of the compass as we understand the term ' ;[2] while Mr. W. C. Lukis, pointing out that on Dartmoor and in Cornwall circles are to be found in clusters, and that there are three circles quite close to one another at Stanton Drew, asks, ' if they were temples, why should the worshippers have been gathered into separate congregations ? '[3] The only answer which I can suggest is that while each of these circles was probably erected either for sepulchral purposes or in honour of a dead ancestor, the rites which were from time to time solemnized within them may have been connected with the worship of the sun. There is no reason to believe that in any megalithic circle in the British Isles solar worship was more than incidental.

About forty years ago the late distinguished archaeologist, Professor Nilsson, wrote an article,[4] the main object of which was to prove that Stonehenge was a temple of Phoenician origin, consecrated to the worship of Baal ; but the evidence upon which he relied was so unsubstantial that no useful purpose would be served by summarizing his arguments, which, indeed, are virtually obsolete.

Professor Flinders Petrie [5] argues that certain parts of Stonehenge are much later than others ; and Dr. Evans, who agrees with him, remarks that ' this is strongly shown by the fact that each of the Stone Circles as well as the Earth Circle has a different centre '.[6] Dr. Evans also points out that, in the case of the circles which are still erected in the East, ' the huge blocks are not all put up at one time but in batches of an equal number of stones at intervals of time.'

Professor Gowland has shown that the sarsen stones in the outer circle must have been erected before the trilithons, and the trilithons before the blue-stones.[7] ' That the stones,' he remarks, ' of the central trilithon were erected from the inside of the circle has been conclusively demonstrated by the excavations ; hence the " bluestones " in front cannot have been erected before them. Moreover, the " bluestone ", No. 68, the base of which was laid bare in Excavation V, was found to be set in the rubble which had been used to fill up the foundation of No. 56, and further, in a lower layer than its base, there were two . . . blocks of sarsen with tooled surfaces. . . . If [the outer sarsens were set up] from the inside [of the circle], their erection must have preceded that of the trilithons and hence of the " bluestones ". On the other hand, should the outer sarsens have been reared from the outside, it would not be possible for the " bluestones " to have been placed in position before them,

[1] See pp. 210-2, *supra*.
[2] *Proc. Soc. Ant. Scot.*, xxxiv, 1900, p. 196.
[3] *Proc. Soc. Ant.*, 2nd ser., ix, 1881-3, p. 348.
[4] *Trans. Ethn. Soc.*, iv, 1866, pp. 244-63.
[5] *Stonehenge*, pp. 21, 32-3.
[6] *Archaeol. Review*, ii, 1889, p. 319.
[7] *Archaeologia*, lviii, 1902, pp. 83-4.

as they would then have seriously interfered with, if not altogether prevented the erecting operations.' Mr. William Cunnington, however, observes that 'the fact that specimens of all the varieties of rocks which constitute the inner circle of Stonehenge have been found in the mixed substance at the base of . . . [the stump of one of the blue-stones] proves that they were all on the spot when the inner ellipse was erected';[1] and Professor Gowland, who confirms this view, concludes that 'no long interval of time separated the erection of the sarsen and the "bluestone" monoliths, although the work must have occupied a considerable period'.

III. Unwarned by the *Edinburgh Review* and Mr. Hinks, Sir Norman Lockyer published in *Nature*[2] a series of 'Notes on Stonehenge', which might be safely ignored if his authority had not made converts, even among archaeologists and men of science who happen to be ignorant of certain essential facts. He now maintains that the sarsens 'and above all the trilithons of the magnificent naos represent a re-dedication and a re-construction of a much older temple'; and, further, that 'the older temple dealt, primarily but not exclusively, with the May year', while 'the newer temple represented a change of cult, and was dedicated primarily to the solstitial year'. It is unnecessary to examine in detail the process by which he has endeavoured to establish these conclusions; but I shall give a few specimens of his work.

'Acting,' says Sir Norman, 'on a very old tradition, the people from Salisbury and other surrounding places go to observe the sunrise on the longest day of the year at Stonehenge. We therefore,' he concludes, 'are perfectly justified in assuming that it was a solar temple.'[3] Not improbably it was—from one point of view; but how old is the tradition? The earliest extant mention of Stonehenge is in the *Historia Anglorum*[4] of Henry of Huntingdon, who lived in the twelfth century, but who does not refer to the tradition. Stonehenge, according to Sir Norman Lockyer, was rebuilt in 1680 B.C. It is therefore impossible to prove that the tradition originated even as early as two thousand nine hundred years after the alleged date of the alleged second dedication of Stonehenge. Tentatively I would suggest that it may have arisen after 1771, when the astronomical theory was anticipated by a Dr. John Smith.[5]

Among the 'considerations' to which Sir Norman would 'direct

[1] *Wilts. Archaeol. and Nat. Hist. Mag.*, xxi, 1884, p. 146.

[2] vol. lxxi, 1904–5, pp. 297–300, 345–8, 367–8, 391–3, 535–8.

[3] *Ib.*, p. 298.

[4] Ed. T. Arnold, 1879, p. 12 (lib. i, c. 7). Sir Norman Lockyer (*Stonehenge*, 1906, p. 51), quoting the well-known passage in which Hecataeus of Abdera, a contemporary of Pytheas, describes a circular temple in the island of the Hyperboreans (Diodorus Siculus, ii, 47, § 1), says that 'Stonehenge alone can by any probability be referred to'. Is it not possible that if the romancer was serious, he was referring to the far larger circle of Avebury?

[5] In a work entitled *Choir Gaur . . . commonly called Stonehenge . . . astronomically explained*, &c.

attention' in support of his theory the fifth[1] runs as follows:—'It is quite possible that the rebuilding of the temple in 1680 B.C. was part of a very large general plan which could only have been undertaken by a large, powerful and comparatively civilized tribe or people under strict government, commanding the services of skilled mathematicians, for Stonehenge, Old Sarum, and Grovely Castle occupy the points of an equilateral triangle of *exactly* six miles in the sides, and the three sides are continuations of the entrances at Stonehenge and Old Sarum and of a ditch running through the centre of Grovely Castle, and the line Stonehenge—Old Sarum passes *exactly* through Salisbury Spire, which again is exactly two miles from Sarum. We ought to restore the old name, Solisbury.'

'Skilled mathematicians' on Salisbury Plain in 1680 B.C., a thousand years before the dawn of mathematics in Greece,[2] busily engaged in forming, for some recondite religious purpose, gigantic equilateral triangles! Sir Norman italicized the word 'exactly'. Evidently then he wished to impress upon us, in proof of the mathematicians' skill, not only that they made their triangle equilateral, but that each side measured six miles,—no more and no less. Is it not a remarkable coincidence that the unit of measurement in the British Bronze Age was the English statute mile? I confess that I cannot grasp the significance of the prolongation of 'the line Stonehenge—Old Sarum' to Salisbury Spire, or of the fact that this additional section was 'exactly two miles long', unless the builders of Stonehenge were Christians as well as mathematicians and Salisbury Spire was standing in 1680 B.C. Nor indeed, it should seem, can Sir Norman himself: at all events in *Stonehenge and other British Monuments Astronomically considered*—a book which is, in the main, a reproduction of his 'Notes'—the passage which I have quoted disappears: equilateral triangle and skilled mathematicians are left to the kindly obscurity of *Nature*. But if 'the line Stonehenge—Old Sarum' and the line Stonehenge—Grovely Castle have lost all significance, why persist in staking a hopeless case upon the imaginary importance of the line Stonehenge—Sidbury Hill?

Sir Norman Lockyer has not restricted his researches to Stonehenge, sun-worship, and the solstitial year. He has discovered instances in which stone circles have been used for the observation not of the sun but of the stars, and in which, 'on account of the change in a star's place due to precession,' 'the sight line has been changed in the Egyptian manner.'[3] Among these astral temples were 'the three circles of the Hurlers, near Liskeard' and 'the circles at

[1] *Nature*, lxxi, 1904–5, p. 391.

[2] See W. W. Rouse Ball, *Short Account of the Hist. of Math.*, 3rd ed., 1901, pp. 2, 6, 14.

[3] *Nature*, lxxi, 1904–5, p. 535. Sir Norman Lockyer has discovered new uses for dolmens and barrows. 'The dolmens,' he says (*ib.*, p. 298), 'have, I am convinced, been in many cases not graves originally, but darkened observing places to observe along a sight-line'; and, he adds (*ib.*, lxxii, 1905, p. 272), 'I have always held that . . . long and chambered barrows were for the living and not for the dead.'

Stanton Drew'. After an interesting calculation he announces that 'we have the following declinations approximately :—

The Hurlers.	Lat. 50° 31′	Stanton Drew.	Lat. 51° 10′
Dec. N.	38½°	Dec. N.	37°
,,	38°	,,	36½°
,,	37°		

Here then,' he observes, 'we have declinations to work on, but declinations of what star? Vega is ruled out as its declination is too high.' He concludes that the star which 'the astronomer-priests' observed was Arcturus, and that 'the approximate dates of the use of the three circles at the Hurlers' are 1600 B.C. for the southern, 1500 for the central, and 1300 for the northern circle; and at Stanton Drew 1260 B.C. for the great circle and 1075 B.C. for the south-western circle.[1]

Once more I am puzzled. Sir Norman remarks that all these circles are considerably older than Stonehenge.[2] Stonehenge, he says, was in use as a solar temple in 1680 B.C. and a good deal earlier: none of the older circles began to be used as an astral temple until 1600 B.C. Why? Surely not because Arcturus, Capella, and Vega all refuse to fit in with 'the sight lines' which Sir Norman has discovered except at inconveniently late dates? Again, 'Vega is ruled out as its declination is too high.' But the present declination of Vega happens to be exactly 38½°. 'In other words,' as Mr. Webb writes to me, 'there exists between the circle and one of the brightest stars in the sky a perfect correspondence, which is nevertheless, beyond all possibility of doubt, wholly accidental.' Why did Sir Norman omit to mention this significant fact?

But second thoughts or kind friends have once more come to Sir Norman's rescue. In his book 'Vega is ruled out as its declination *was* too high'[3] (the italics are mine). 'He had become aware,' remarks the lynx-eyed Mr. Webb, 'of the damaging fact that the present declination of Vega actually *is* 38½° N., in other words that, on his own principles, we can prove that the Hurlers were set up to-day.'

[1] *Nature*, lxxi, 1904–5, pp. 536–8.
[2] *Ib.*, p. 536.
[3] *Stonehenge*, p. 137.

THE CASSITERIDES, ICTIS, AND THE BRITISH TRADE IN TIN

I. THE CASSITERIDES

I. The identity of 'the tin-islands', which ancient writers called the Cassiterides, is still a matter of dispute. Professor Haverfield, indeed, has affirmed that 'the recent researches of Usener [for which read Unger], Rhys, and others, have made it almost certain that the Cassiterides were off N.W. Spain'.[1] Professor Rhys shall speak for himself. 'M. Reinach,' he says,[2] 'argues, convincingly as it seems to me, that the Cassiterides meant the Celtic islands, or, as I may call them, the British Isles.' And, if anything relating to this question is certain, it is that the islands off North-Western Spain, which are supposed to have been the tin-islands, have never produced any tin at all.[3]

One group of scholars insists that all the ancient writers who mentioned the Cassiterides associated them with Spain. But what if the ancient writers were misinformed, or misunderstood their informants? Another group insists that for the metallurgists of ancient Europe the sole source of tin was the British Isles; and with this pronouncement they would apply the closure to the debate. But the British Isles were not the sole source;[4] and the debaters persist in wrangling. If the only question were, From what parts of Europe did the Greeks and Romans derive tin, it could be answered in a sentence:—from Galicia in Spain, Cornwall, and, possibly, the Scilly Islands.[5] But this is not the only question. What we want to know is, Were the ancient writers misled into believing that the Cassiterides were islands? If they were not misled, were they all thinking of the same islands? Or did they attempt to indicate the position of the Cassiterides by simply guessing? If they were misled, was the district to which their informants alluded Galicia or Cornwall, or did they refer to both? Did the ancient writers fancy that islands

[1] *Archaeol. Journal*, xlix, 1892, p. 178, n. 1. In a more recent article (*Proc. Soc. Ant.*, 2nd ser., xviii, 1900, p. 119) Professor Haverfield refers to 'the tin-trade of N. W. Spain, where we must place the famous and fabulous Cassiterides'. The word 'fabulous' seems to suggest that he here withdraws his former view that the Cassiterides were islands 'off' N.W. Spain.

[2] *Academy*, xlviii, Oct. 5, 1895, p. 273.

[3] W. C. Borlase, *Tin Mining in Spain*, 1898, p. 21.—'In the island of Ons alone, near the mouth of the river Pontevedra . . . some indications of tin-quartz were found, so Cornide tells us,' &c. Ons is not one of the group of islands with which the Cassiterides have been identified.

[4] *Ib.*, pp. 24, 28, &c. Mr. Borlase's investigations only confirm the statements of Diodorus (v, 38, § 4), of Strabo (iii, 2, § 9), and of Pliny (*Nat. Hist.*, xxxiv, 16 [47], § 156), who all agree in saying that Spain produced tin.

[5] See p. 490, n. 5, *infra*.

used as depots for tin were the places in which the mines were situated? Did those who professed to inform them intentionally mislead them?

The theories which have recently held or still hold the field are, first, that the Cassiterides were a group of islets off the north-western coast of Spain; secondly, that they were headlands of the same coast; thirdly, that they were the Scilly Islands; fourthly, that they were Cornwall, which is supposed by some writers to have been regarded either as an island or as a group of islands, separated by estuaries, which were erroneously believed to be channels; and, lastly, that they were the British Isles.[1]

II. Diodorus Siculus,[2] after stating that tin was produced in Britain and in many parts of Iberia, goes on to say that there are many tin mines in the islands called Cassiterides, which are situated in the ocean, off the coast of Iberia and above the country of the Lusitani.[3] Strabo mentions the Cassiterides four times. In the first passage[4] he says that the extremity of the Pyrenees is opposite the western parts of Britain, and that the Cassiterides, which are situated in the same latitude as Britain, are in the open sea, opposite to and north of the Artabri. In the second[5] he mentions both the Cassiterides and the British Isles, clearly distinguishing the two groups. In the third[6] he says that, according to Posidonius, tin was produced in the country beyond [that is to say, north of] the Lusitani, and also in the Cassiterides; that tin was conveyed from the British Isles to Massilia, and that, according to the same authority, tin, silver, and gold were produced in the country of the Artabri, the most remote tribe of Lusitania, who face the north-west. In the fourth[7] he says that the Cassiterides

[1] De Mortillet's identification of the Cassiterides with the islands off the coast of Brittany is not worth discussing. Tin was apparently worked in the Morbihan in the Bronze Age (W. Boyd Dawkins, *Early Man in Britain*, pp. 403–4), but not in any of the Breton islands; nor is there any evidence that Gallic tin was ever an object of foreign commerce.

[2] *Bibl. hist.*, v, 21, § 2; 22, §§ 1–2; 38, § 4.

[3] *Ib.*, 38, § 4.—*ὑπεράνω γὰρ τῆς τῶν Λυσιτανῶν χώρας ἔστι μέταλλα πολλὰ τοῦ καττιτέρου, κατὰ τὰς προκειμένας τῆς Ἰβηρίας ἐν τῷ ὠκεανῷ νησίδας τὰς ἀπὸ τοῦ συμβεβηκότος Καττιτερίδας ὠνομασμένας.*

[4] *Geogr.*, ii, 5, § 15.—*τούτοις δὲ* [i.e. the extremity of the Pyrenees] *τὰ ἑσπέρια τῆς Βρεττανικῆς ἀντικεῖνται πρὸς ἄρκτον, ὁμοίως δὲ καὶ ταῖς Ἀρτάβροις ἀντίκεινται πρὸς ἄρκτον αἱ Καττιτερίδες καλούμεναι νῆσοι πελάγιαι, κατὰ τὸ Βρετανικόν πως κλίμα ἱδρυμέναι.*

[5] *Ib.*, ii, 5, § 30.—*πρόκεινται δὲ νῆσοι τῆς Εὐρώπης, ἃς ἔφαμεν, ἔξω μὲν Στηλῶν Γάδειρά τε καὶ Καττιτερίδες, καὶ Βρεττανικαί*, &c.

[6] *Ib.*, iii, 2, § 9.—*τὸν δὲ καττίτερον οὐκ ἐπιπολῆς εὑρίσκεσθαί φησιν* [Ποσειδώνιος] *. . . ἀλλ' ὀρύττεσθαι· γεννᾶσθαι δ' ἔν τε τοῖς ὑπὲρ τοὺς Λυσιτανοὺς βαρβάροις καὶ ἐν ταῖς Καττιτερίσι νήσοις, καὶ ἐκ τῶν Βρεττανικῶν δὲ εἰς τὴν Μασσαλίαν κομίζεσθαι. ἐν δὲ τοῖς Ἀρτάβροις, οἳ τῆς Λυσιτανίας ὕστατοι πρὸς ἄρκτον καὶ δύσιν εἰσίν, ἐξανθεῖν φησιν τὴν γῆν ἀργυρίῳ, καττιτέρῳ, χρυσίῳ λευκῷ.*

[7] *Ib.*, iii, 5, § 11.—*Αἱ δὲ Καττιτερίδες δέκα μὲν εἰσί, κεῖνται δ' ἐγγὺς ἀλλήλων, πρὸς ἄρκτον ἀπὸ τοῦ τῶν Ἀρτάβρων λιμένος πελάγιαι· μία δ' αὐτῶν ἔρημός ἐστι, τὰς δ' ἄλλας οἰκοῦσιν ἄνθρωποι μελάγχλαινοι, ποδήρεις ἐνδεδυκότες τοὺς χιτῶνας, ἐζωσμένοι περὶ τὰ στέρνα, μετὰ ῥάβδων περιπατοῦντες, ὅμοιοι ταῖς τραγικαῖς Ποιναῖς· ζῶσι δ' ἀπὸ βοσκημάτων νομαδικῶς τὸ πλέον. μέταλλα δὲ ἔχοντες καττιτέρου καὶ μολύβδου κέραμον ἀντὶ τούτων καὶ τῶν δερμάτων διαλλάττονται καὶ ἄλας καὶ χαλκώματα*

are ten in number and lie close together in the open sea, north of the harbour of the Artabri; that one of them is uninhabited; and that the inhabitants of the rest wear black robes reaching down to their feet, and walk about with staves in their hands, 'like the Furies in tragedy.' They are, he says, nomadic, and live upon flesh meat; and they barter tin and hides with merchants for pottery, salt, and articles of bronze. Formerly, he adds, the Phoenicians monopolized the trade from Gades, or Cadiz, with the islanders; and they kept the route a close secret, which, however, the Romans, after numerous attempts, succeeded in discovering. Finally, Publius Crassus sailed across (*διαβάς*) to the islands, ascertained that the tin lay near the surface, and indicated the route for the benefit of traders, 'although the passage was longer than that [from the continent] to Britain.' In another passage[1] Strabo says that the Artabri dwell in the neighbourhood of the north-western promontory of Iberia, which he identifies with the Nerian promontory.[2] Pomponius Mela,[3] himself a Spaniard, immediately after speaking of Baetica and Lusitania, and immediately before mentioning the island of Sena, which was off the coast of Brittany, states that the Cassiterides are situated *in Celticis*. Pliny[4] says that the Cassiterides, so called from the abundance of tin which they produce, are situated over against Celtiberia, and that opposite the promontory of the Arrotrebae are 'the six islands of the Gods', which some call 'the Fortunate Isles'. In another passage[5] he says that tin was first fetched from 'the island Cassiteris' (or from 'the tin island') by Midacritus, whom M. d'Arbois de Jubainville,[6] wrongly, according to M. Salomon Reinach,[7] identifies with 'Melkarth, who personified the Phoenician race'. In a third passage[8] Pliny says that tin has been fabulously reported to

πρὸς τοὺς ἐμπόρους. πρότερον μὲν οὖν Φοίνικες μόνοι τὴν ἐμπορίαν ἐστελλον ταύτην ἐκ τῶν Γαδείρων, κρύπτοντες ἅπασι τὸν πλοῦν· τῶν δὲ Ῥωμαίων ἐπακολουθούντων νανκλήρῳ τινί, ὅπως καὶ αὐτοὶ γνοῖεν τὰ ἐμπόρια, φθόνῳ ὁ ναύκληρος ἐκὼν εἰς τέναγος ἐξέβαλε τὴν ναῦν, ἐπαγαγὼν δ' εἰς τὸν αὐτὸν ὄλεθρον καὶ τοὺς ἐπομένους, αὐτὸς ἐσώθη διὰ ναναγίου . . . οἱ Ῥωμαῖοι δὲ ὅμως πειρώμενοι πολλάκις ἐξέμαθον τὸν πλοῦν· ἐπειδὴ δὲ καὶ Πόπλιος Κράσσος, διαβὰς ἐπ' αὐτούς, ἔγνω τὰ μέταλλα ἐκ μικροῦ βάθους ὀρυττόμενα . . . ἐκ περιουσίας ἤδη τὴν θάλατταν ἐργάζεσθαι ταύτην τοῖς ἐθέλουσιν ἐπέδειξε, καίπερ οὖσαν πλείω τῆς διειργούσης εἰς τὴν Βρεττανικήν.

[1] *Ib.*, iii, 3, § 5.—*Ὕστατοι δ' οἰκοῦσιν Ἀρταβροι περὶ τὴν ἄκραν, ἣ καλεῖται Νέριον, ἣ καὶ τῆς ἑσπερίου πλευρᾶς καὶ τῆς βορείου πέρας ἐστί.*

[2] *Ib.*, iii, 1, § 3.—*τρίτον ἐστὶ τὸ ἑσπέριον πλευρὸν . . . μέχρι τῆς πρὸς Ἀρτάβροις ἄκρας, ἣν καλοῦσι Νέριον.*

[3] *Chorographia*, iii, 6, § 47.—in Celticis aliquot sunt [insulae], quas quia plumbo abundant uno omnes Cassiteridas appellant.

[4] *Nat. Hist.*, iv, 22 (36), § 119.—Ex adverso Celtiberiae complures sunt insulae Cassiterides dictae Graecis a fertilitate plumbi, et e regione Arrotrebarum promunturi Deorum VI, quas aliqui Fortunatas appellavere. G. F. Unger (*Rheinisches Museum*, xxxviii, 1883, p. 167) holds that both Pliny and Mela, in locating the Cassiterides, followed Roman, and therefore recent authorities.

[5] *Ib.*, vii, 56 (57), § 197.—plumbum ex Cassiteride insula primus adportavit Midacritus.

[6] *Les premiers habitants de l'Europe*, i, 1889, p. 196, n. 2. Cf. K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, p. 211.

[7] See p. 514, *infra*.

[8] *Nat. Hist.*, xxxiv, 16 (47), § 156.—Pretiosissimum hoc, Graecis appellatum

have been obtained from islands in the Atlantic. 'Now,' he continues, 'it is certainly known to be produced in Lusitania and Galicia.' By 'islands in the Atlantic' Pliny certainly did not, as Professor Ridgeway supposes,[1] consciously mean the British Isles; for in his geographical system the northern limit of the Atlantic was marked by the north-western promontory of Spain.[2] Ptolemy[3] says that the Cassiterides are ten in number, and are situated in the western Ocean. Finally, Dionysius Periegetes[4] says that 'the western isles', which produce tin, and are situated below the Sacred Promontory, or Cape St. Vincent, are inhabited by Iberians.

From a comparison of these statements it is clear, first, that the ancient geographers who mentioned the Cassiterides regarded them as distinct from the British Isles; secondly, that they believed them to be situated somewhere off the coast of Spain (although, as we shall presently see, the words of Strabo are not inconsistent with the theory that he identified them with the Scilly Islands, or even, unconsciously, with the British Isles); thirdly, that of those who attempted to define their position one associated them with the south-western, the others with the north-western coast; fourthly, that one writer mentioned *an* island, Cassiteris, from which tin was fetched; and, lastly, that this same writer, having affirmed that the tin islands were opposite Celtiberia, nevertheless denied that any islands in the Ocean which extended as far north as the north-western promontory of Spain produced tin.

III. Assume, for the sake of argument, that the Cassiterides were off the north-western coast of Spain. What, then, are the islands with which they are to be identified?

1. Unger[5] remarks that it may be inferred from Ptolemy's statement that they were south of the Nerian promontory and off the western coast of Galicia. Strabo, it is true, places them northward of the northern coast: but, says Unger,[6] Strabo is wrong; for on the northern coast there are no islands distant more than one German mile [or between four and five English miles] from the shore, whereas Strabo himself says that the Cassiterides were further from the continent than Britain. Dionysius Periegetes was, by common consent, mistaken. Let us see then what islands we can find off North-Western Spain. The coast between Cape Ortegal and the mouth of the Douro is broken by several inlets or fiords, which are called Rias. East of Cape Finisterre, in

cassiterum fabuloseque narratum in insulas Atlantici maris peti vitilibusque navigiis et circumsutis corio advehi. Nunc certum est in Lusitania gigni et in Gallaecia, &c.

[1] *Academy*, xlviii, Dec. 14, 1895, p. 524.

[2] *Nat. Hist.*, iii, 1 (2), § 6; iv, 19 (33), § 109; 22 (35), § 114.

[3] *Geogr.*, ii, 6, § 73.—Ἐν δὲ τῷ Δυτικῷ Ὠκεανῷ αἱ Καττιτερίδες δέκα τὸν ἀριθμόν, ὧν τὸ μεταξὺ ἐπέχει μοίρας δ′ με′ ⌊″ (4° 45° 30′) καὶ αἱ τῶν θεῶν νῆσοι δύο τὸν ἀριθμὸν δ′ γο″ μγ′ γ″ (4° 40′ 43° 30″).

[4] *Orbis Descriptio*, 561–4.—αὐτὰρ ὑπ' ἄκρην | Ἰρήν, ἣν ἐνίπουσι κάρην ἔμεν Εὐρωπείης, | Νήσους Ἐσπερίδας, τόθι κασσιτέροιο γενέθλη, | Ἀφνειοὶ ναίουσιν ἀγαυῶν παῖδες Ἰβήρων.

[5] *Rheinisches Museum*, xxxviii, 1883, p. 166.

[6] *Ib.*, pp. 166–7.

the Ria de Corcubion, are three very small islands; and off the south-eastern entrance of this Ria, by Cape Minarzo, are six tiny islets. About 20 miles south-east of Cape Finisterre is the Ria of Muros and Noya: on the west of Mount Louro, which dominates the entrance of this fiord on its northern side, are the four small islands of Bruyos; and there are others within the fiord itself and south of it. Unger[1] remarks that the small size of all these islands harmonizes with the word νησῖδες, or 'islets', which Diodorus applies to the Cassiterides; and that the smallest of them may have been left out of account when the number was given as ten. Off Pontevedra Bay, which is north of Vigo Bay, are the islands of Ons and Orcela.

Strabo says that the harbour of the Artabri, north of which he places the Cassiterides, was formed by a gulf on which were situated numerous cities.[2] Mela[3] describes a gulf in the country of the Artabri as having a narrow entrance and a wide circuit, and adds that four streams flowed into it; and Ptolemy,[4] having first mentioned a harbour of the Artabri, immediately south of the Nerian promontory, speaks, in the next section but one, of 'the Great Harbour', on the shore of which he places Brigantium. The gulf mentioned by Mela and Ptolemy's 'Great Harbour' correspond with the Ria of Betanzos and Ferrol, which is between Cape Finisterre and Cape Ortegal; but there are no islands north of this harbour. The identification of the harbour of the Artabri which Ptolemy places immediately south of the Nerian promontory depends of course upon the identification of the promontory itself. The latter is generally identified with Cape Finisterre; and if this view is correct, the harbour must have been the Ria de Corcubion. Unger, however, identifies the Nerian promontory with the bluff of land, between Cape Finisterre and Corunna, from which project the headlands of Punta del Roncudo, Punta de Nariga, and Cape de S. Adrian;[5] and if he is right, Ptolemy's harbour was the Ria of Corme and Lagos. But, as Unger points out, there are no islands off this harbour or north of it.[6] It is clear, then, that if the Cassiterides really lay in Spanish waters and on the north of a harbour of the Artabri, that harbour must be looked for further south. Now Unger[7] observes that Posidonius, as quoted by Strabo,[8] makes the territory of the Artabri extend southward as far as the river Douro; for he says that their territory produced gold; and, says Unger, in the country of the Artabri, in the narrower sense, there are no auriferous streams. Accordingly, Unger identifies the

[1] *Ib.*, p. 170.

[2] *Geogr.*, iii, 3, § 5.—Ἔχουσι δὲ οἱ Ἀρταβροι πόλεις συχνὰς ἐν κόλπῳ συνοικουμένας, ὃν οἱ πλέοντες καὶ χρώμενοι τοῖς τύποις Ἀρτάβρων λιμένα προσαγορεύουσιν.

[3] *Chorographia*, iii, 1, § 13.—In Artabris sinus ore angusto admissum mare non angusto ambitu excipiens Adrobricam urbem et quattuor amnium ostia incingit, &c.

[4] *Geogr.*, ii, 6, § 2.—Ἀρτάβρων λιμὴν ϵ′ γ″ με′ (5° 20′, 45°) Νέριον ἀκρωτήριον ϵ′ δ″ με′ ς″ (5° 15′, 45° 10′); § 4—ἐν τῷ Μεγάλῳ λιμένι Φλαούιοι Βριγάντιον ς′ ⌊″ δ″ με′ (6° 45′, 45°).

[5] *Rhein. Mus.*, xxxviii, 1883, p. 165, n. 2.

[6] *Ib.*, p. 168.

[7] *Ib.*

[8] See p. 484, n. 6, *supra*.

harbour for which he has been searching with the Puerto de Bayona,—the southernmost inlet north of the Douro; and from this harbour he maintains that Crassus sailed to the Cassiterides, which he identifies with the islands of Bruyos. He points out that the distance from the northern entrance of the harbour, opposite the island of Bayona, to the islands of Bruyos is eight German miles. This distance exceeds that of the shortest passage between Britain and the continent; and accordingly Unger insists that it harmonizes with the statement of Strabo. But the point which he most strenuously labours is that 'in this case only is explicable the circumstance, strikingly calculated to cause such a mistake as Strabo made, that Crassus sailed northward from a harbour on the west coast [of Galicia], and yet sailed in the open sea'.[1]

Mr. Cecil Torr,[2] on the other hand, insists that 'unless it can be shown that there were tin *mines* on the islands [near Vigo], the story [of the voyage of Crassus] cannot be used to show that Crassus visited those islands'. Strabo, he adds, states precisely that the Cassiterides 'lay to the north of Ἀρτάβρων λιμήν', which 'is obviously the gulf that now holds Ferrol and Corunna'. Here, as we have seen, there are, *in Spanish waters*, no islands; and Mr. Torr argues that 'Strabo is so very accurate in his description of this part of Spain that his account of the Cassiterides cannot be explained away as an inaccurate description of the islands at Vigo . . . It must be a bit of downright fiction repeated in good faith.'[3] To this latter argument it might be replied, first, that just as Strabo was mistaken in supposing that the direction of the Pyrenees was from north to south, so he may have been mistaken in supposing that the Cassiterides were on the north of Spain; and, secondly, that the other writers whose testimony has been quoted place them off the west coast. No other reply, indeed, could be made by those who hold, like Unger, that the Cassiterides were in Spanish waters. But, for reasons which shall presently be given, I agree with Mr. Torr that Ἀρτάβρων λιμήν must have been 'the gulf that now holds Ferrol and Corunna', and also that Strabo's 'account of the Cassiterides cannot be explained away as an inaccurate description of the islands at [or rather near] Vigo'. Only I believe that he is wrong in regarding that account as 'fiction'. Strabo's Cassiterides *were not in Spanish waters at all: they were, as he says, in the open sea, and far to the north of Corunna.* Mr. Torr's other objection rests upon the fact that tin was never produced in any island off the coast of Spain, except, possibly, in Ons,[4] which, for reasons obvious to any one who consults the map of Spain, Unger does not include among the Cassiterides. The only possible answer to this objection has been already suggested in this

[1] *Rhein. Mus.*, xxxviii, 1883, p. 170.
[2] *Academy*, xlviii, Nov. 23, 1895, p. 438. [3] *Ib.*, Dec. 21, p. 547.
[4] See p. 483, n. 3, *supra*. Unger (*Rhein. Mus.*, xxxviii, 1883, p. 171) says that after the discovery made by Crassus the mines on the islands must have been speedily worked out. But this is pure fancy: the mines on these islands were never worked out, for they never existed.

article : it is that the islands may have served as depots to which the tin was conveyed from the mainland opposite, and that they may have been confounded with the districts in which the tin was actually produced. This suggestion, however, leaves unexplained the definite statement of Strabo, that Crassus sailed across to the islands and found that the islanders worked the tin easily because it lay near the surface. There remain three other objections, which, unless Strabo's authority is to be absolutely discarded, appear insuperable. First, Strabo, I repeat, distinctly states that the islands were 'in the open sea' (πελάγιαι); and of the islands which Unger identifies with the Cassiterides not one is more than four statute miles from the mainland, while the nearest is not more than two. Secondly, as they are all within sight of land, their situation could never have been kept secret. Lastly, since it was unnecessary for those who desired to reach them to sail from the harbour of Vigo, and easy to sail across from the neighbouring Ria of Muros and Noya, it is difficult to understand why Strabo should have said that the distance which separated them from the mainland was greater than the distance from Gaul to Britain. It is true that the islanders, according to Strabo, dressed in black, and that, according to the same authority,[1] the inhabitants of Lusitania did likewise ; but any one who regards this as an argument for identifying the Cassiterides with the islands near Vigo must make up his mind to reject nearly all the details which are given by Strabo, and to pin his faith to the undoubted fact that the Cassiterides are placed by most of the ancient authorities off the coast of Spain. M. Salomon Reinach, however, with whom I agree, argues that 'the fact that numerous [ancient] writers place the Cassiterides in geographical connexion with Spain only proves—what we knew before—that Phoenician Spain had commercial relations with those islands'.[2]

2. M. Hans Hildebrand[3] thinks that the Cassiterides were headlands of the Galician coast. He argues that if they are to be located 'in England', the name Cassiterides must be applied to headlands in Cornwall ; accordingly, he says, 'je demande la même concession pour ma théorie espagnole, savoir que ce nom désigne des caps.'[4] But Cornwall is part of an island which is itself one of a group of islands : Spain is not an island at all. M. Hildebrand's theory can by no ingenuity be defended except on the assumption that the ancient writers were misled by the fact that in the language of the Phoenicians, from whom the earliest notions about the Cassiterides may be supposed to have been derived, there was no word which specially denoted islands ; and if it is accepted, not only must all the statements of those writers which relate to the situation of the islands, their number, their inhabitants, the mode in which the tin was extracted, and the voyage of Crassus, be rejected as absolutely

[1] *Geogr.*, iii, 3, § 7.—Μελανείμονες ἅπαντες.
[2] *L'Anthr.*, iii, 1892, pp. 275–6. See also K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, p. 92, n. *, and Unger in *Rhein. Mus.*, xxxviii, 1883, p. 163.
[3] *Congrès internat. d'anthr. et d'archéol. préhist.*, 1874, i, 579–84.
[4] *Ib.*, p. 579.

fictitious, but it is utterly impossible to conceive how they should have originated.[1]

3. The old-fashioned view, which identified the Cassiterides with the Scilly Islands, has even of late years had adherents of high reputation, such as Dr. von Gutschmid,[2] Emil Hübner,[3] and Mommsen.[4] Although the bulk of the tin which supplied the wants of ancient Europe came from Cornwall and Spain, it is nevertheless not improbable that some came from the Scilly Islands.[5] If so, the real Cassiterides were the Scilly Islands and 'the adjacent island' of Great Britain. But of the ancient writers there were only two of whom it can be maintained that when they referred to the Cassiterides they were thinking either of the Scilly Islands or of Cornwall,—Festus Avienus and Strabo.

Festus Avienus was a writer of the fourth century, whose *Ora maritima* was based either upon a Greek version of the Carthaginian account of the voyage of Himilco,[6] or, as seems more probable, upon a Greek poem, which had itself been compiled from two distinct Greek narratives of different dates, the latter being assignable to the period between 240 and 150 B.C.[7] After describing the rocky peninsula,

[1] The theory of Mr. Cecil Torr, who holds that the Cassiterides never existed, is virtually identical with that of M. Hildebrand. 'In the Phoenician language', he remarks, 'the word for island is the same as in the Hebrew . . . and this word אי is used repeatedly in the Bible for places beyond the sea, as well as . . . islands. Most probably the Phoenicians used this word when speaking of the Cassiterides, meaning thereby that these were places beyond the sea [which he identifies with 'the north-west corner of Spain']: but the Greeks understood it in another sense, and thus turned these places into islands' (*Academy*, xlviii, Oct. 26, 1895, pp. 342-3). To the objection that Publius Crassus reached the Cassiterides by sea, Mr. Torr replies that 'there is nothing to show that his destination was an island' (*ib.*, Nov. 9, p. 390); but Mr. Talfourd Ely (*ib.*, Nov. 16, p. 414) pertinently asks whether the word διαβάς, which Strabo uses in describing the voyage of Crassus, can be used of coasting from one point to another on the same shore. Moreover, it is absurd to contend that 'there is nothing to show' that Crassus sailed to an island; for Strabo says that the Cassiterides were ten islands; and Mr. Torr is therefore forced, as we have seen (p. 488), to make the incredible assumption that Strabo's account of the voyage of Crassus is pure fiction.

[2] *Ency. Brit.*, xviii, 1885, p. 806; *Lit. Centralblatt*, 1871, p. 528.

[3] Pauly's *Real-Encyclopädie*, iii, part i, 1897, pp. 860, 863.

[4] *Hist. of Rome*, v, 1894, p. 63 (*Röm. Gesch.*, iii, 1889, p. 269).

[5] 'The ancient workings for Tin, in the Scilly Islands, are neither deep, nor many, nor large' (Wm. Borlase, *Observations on the Ant. . . . of . . . Cornwall*, 1754, p. 30). [In St. Nicholas Island] 'we found a row of shallow Tin-pits . . . These are the only Tin Pits which we saw, or are any where to be seen, as we were informed, in these Islands' (*ib.*, *Observations on the . . . Islands of Scilly*, 1756, p. 45). 'Some Tin might have been found in the low grounds washed down from the Hills . . . There may be also Tin-veins in those Cliffs which we did not visit . . . as the *Gwêl-Hill* of BREHAR, *Gwêl* Island, the name *Gwêl* (or *Hwêl*) in Cornish signifying a Working for Tin' (*ib.*, pp. 73-4). 'I have been lately informed that, under one of the Cliffs of ANNET, there is a Load, in which there is the appearance of Tin, and that it looks as if it had been work'd' (*ib.*, p. 73, note m). 'Tin is found in several of the islands . . . but there are now no mines in work' (D. and S. Lysons, *Magna Britannia*, iii, 1814, p. 337). See Addenda, p. 740.

[6] See H. d'Arbois de Jubainville, *Principaux auteurs de l'ant. à consulter sur l'hist. des Celtes*, &c., p. 42.

[7] See F. Marx's article in *Rhein. Mus.*, l, 1895, pp. 321-47.

Oestrymnis, he says that in the gulf formed by it lie the islands called Oestrymnides, which are widely scattered and rich in tin.[1] He does not mention the Cassiterides at all. The Oestrymnides, however, are generally, and, if Festus was right in saying that they produced tin, necessarily identified with the Cassiterides: the peninsula is rightly identified with Brittany,[2] or, more strictly speaking, with the promontory formed by Finistère; and therefore the gulf is either the Bay of Biscay, or the gulf in which lie the Channel Islands. After describing the Oestrymnides, Festus goes on to say that 'from here it is two days' sail to the Sacred Island', that is to say, Ireland (*hinc duobus in sacram sic insulam Dixere prisci solibus cursus rati est*[3]); and then, remarking that 'the island of the Albiones', or Britain, is near, he says that the Tartesii used to resort for trade to the Oestrymnides, and that the Carthaginians also used to sail 'these seas'.[4]

The question of the identity of the gulf is discussed by Friedrich Marx in an article on the *Ora maritima*,[5] of which a summary has been given by Mr. W. H. Stevenson.[6] Marx, says Mr. Stevenson, 'explains the *sub vertice* [of the promontory or peninsula, Oestrymnis] of Avienus[7] as referring to the maps of antiquity, and as having the sense of "northwards of", so that the Tin Islands are conceived of as north of the promontory of Finistère . . . The Tin Islands must therefore be the mainland of Britain and the Isle of Wight (which Marx considers to be included among the *laxe iacentes insulae* of Avienus), and cannot be explained as the Scilly Islands, which have nothing beyond their insular nature to favour the identification.' But why 'therefore'? What has the Isle of Wight beyond *its* 'insular nature' to 'favour the identification'? If Marx is right in his interpretation of *sub vertice*, the gulf (*sinus*) lay north of 'the promontory of Finistère'; and since it can hardly be maintained

[1] *Ora maritima*, 90–8.—

Et prominentis hic iugi surgit caput,
(Oestrymnin istud dixit aevum antiquius,)
Molesque celsa saxei fastigii
Tota in tepentem maxime vergit Notum.
Sub huius autem prominentis vertice
Sinus dehiscit incolis Oestrymnicus,
In quo insulae sese exserunt Oestrymnides,
Laxe iacentes, et metallo divites
Stanni atque plumbi.

[2] See K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, p. 91.

[3] *Ora maritima*, 108–9.

[4] *Ib.*, 110–6.—

Haec inter undas multa caespitum iacet,
Eamque late gens Hibernorum colit.
Propinqua rursus insula Albionum patet.
Tartesiisque in terminos Oestrymnidum
Negotiandi mos erat: Carthaginis
Etiam coloni, et vulgus, inter Herculis
Agitans columnas, haec adibant aequora, &c.

[5] *Rhein. Mus.*, l, 1895, p. 335.

[6] *Eng. Hist. Rev.*, xix, 1904, pp. 139–40, n. 5.

[7] *Ora maritima*, 94.

that this gulf was the English Channel, it must have been the gulf in which lie the Channel Islands. If it was the English Channel, Marx can hardly venture to argue that 'the mainland of Britain' is *in* the Channel. If any conclusion can be drawn from the words of Festus, it must be either that the *laxe iacentes insulae* were the Channel Islands and the *sinus* the gulf in which they are situated, or that the *sinus* was the Bay of Biscay and the *insulae* Ushant and the adjacent islets. But I agree with Mr. Stevenson that 'the Tin Islands [of Avienus] . . . cannot be explained as the Scilly Islands', unless the indications which Avienus gives of their situation are utterly misleading.

Strabo, as we have seen, says that the Cassiterides were in the open sea northward from the harbour of the Artabri; and Mr. H. F. Tozer[1] argues that, according to Strabo's 'idea of the relative position of these countries [Spain and Britain] this would place them a great distance to the west of the Scilly Islands'. This objection, however, assumes that Strabo was aware that the Scilly Islands were comparatively close to the Land's End. Strabo imagined that the direction of the Pyrenees was from north to south; that the coast of Gaul extended in a straight line from the northern extremity of the Pyrenees to the mouth of the Rhine; and that the southern coast of Britain extended from a point nearly opposite and close to the northern extremity of the Pyrenees, parallel with the coast of Gaul.[2] He expressly states that the Cassiterides were in the same latitude as Britain; and therefore, if he had intended to identify the Cassiterides with the Scilly Islands, it would have been quite natural for him to say that they lay north of the harbour of the Artabri.[3] Müllenhoff[4] indeed dismisses the claims of the Scilly Islands with contempt; but all that he has to say against them is that they never produced tin, and that they are small. The former objection is, as we have seen, unfounded; the latter is irrelevant, for small islands may contain mines, and the islands off the coast of Spain are smaller still.

But I am not concerned to argue that the sailors from whom the ancient writers, directly or indirectly, derived their information intended to convey that the Cassiterides were the Scilly Islands and the Scilly Islands alone; for, although the Scilly Islands did produce some tin, by far the greater part of the British supply of that metal doubtless came from Cornwall. Professor von Gutschmid indeed

[1] *Hist. of Anc. Geogr.*, p. 37.

[2] Similarly Appian (*De rebus Hisp.*, 1) says that the voyage from Spain to the British Isles occupied half a day!

[3] This page was written before I had read the relevant passage in Kiepert's *Formae orbis antiqui*, quoted on p. 493, *infra*. Dr. H. Berger maintains (*Gesch. der wissenschaftlichen Erdkunde der Griechen*, iv, 1893, pp. 24–5) that Strabo's error was due to a misunderstanding of statements about islands situated on the route which the ships engaged in the tin trade followed; but I cannot conceive how such a misunderstanding could have been suggested by the narrative of Crassus.

[4] *Deutsche Altertumskunde*, i, 1890, p. 92 and note *; ii, 1887, p. 317.

explains that 'the tin was supposed [by the ancient writers] to be produced where it was exchanged,—a very common case'; [1] and although the place where the Cornish tin was exchanged by the merchants who used the overland route was Ictis, or St. Michael's Mount,[2] the Phoenicians may possibly have found it convenient to occupy one of the Scilly Islands.[3] But it seems to me safer to conclude that the Scilly Islands may have been originally included with Britain under the designation, *Cassiterides*.

The late distinguished geographer, H. Kiepert, maintained that although the name, *Cassiterides*, had been originally used by the Greeks to denote the tin-producing districts of Britain, it was erroneously applied by Strabo to the Scilly Islands. 'Only to this group', he insists, 'can Strabo's account of the discovery of the ten small *Cassiterides*-islands on the north of Hispania by . . . Publius Crassus refer, as there are no other islands in this part of the ocean.' [4]

4. George Smith, for whom the Cassiterides represented simply the Cornish peninsula,[5] observed, anticipating a similar argument of Mr. Cecil Torr,[6] that 'the Hebrew, Phoenician, and cognate languages had no terms which distinctly specified islands, peninsulas, &c.; one word being used to signify islands, sea-coasts, and even remote countries. In these languages the whole coast of Cornwall and Devonshire might be termed island or islands.' It may be objected that the very same argument might be used to show that the name, *Cassiterides*, really denoted the headlands on the coast of Galicia.[7] But it is easier to conceive how the misconception should have arisen in the case of Cornwall, part of a remote island in the northern ocean and close to the Scilly Islands, than in the case of Galicia; and, moreover, the Galician theory leaves the story of Crassus's voyage unexplained. But the problem of the Cassiterides cannot be satisfactorily solved by the simple statement that they were Cornwall.

5. Müllenhoff,[8] M. Salomon Reinach, and various other writers identify the Cassiterides with the British Isles. According to M. Reinach,[9] 'the whole question resolves itself into this:—what islands in western Europe produce tin? The British Isles alone fulfil this condition; therefore we must recognize in them the archipelago of the Cassiterides.' 'If,' he adds, 'Strabo does not identify them with the British Isles, though he mentions both the one group and the other, this is because in the different chapters [of his

[1] *Lit. Centralblatt*, 1871, pp. 528–9; *Ency. Brit.*, xviii, 1885, p. 806.
[2] See pp. 500–7, *infra*.
[3] See also *L'Anthr.*, x, 1899, p. 401, n. 2.
[4] *Formae orbis antiqui,—insulae Britannicae*, 1893.
[5] *The Cassiterides*, pp. 52–3. See also p. 80, n. *, and pp. 107–8, where Smith makes an ingenious but hardly successful attempt to account for the statement of Strabo, repeated by Ptolemy, that the Cassiterides were ten in number.
[6] See p. 490, n. 1, *supra*.
[7] See pp. 489–90, *supra*.
[8] *Deutsche Altertumskunde*, i, 1890, pp. 91–2.
[9] *L'Anthr.*, iii, 1892, pp. 275–6.

work] he follows different authorities, some of whom allude to the Cassiterides from hearsay evidence collected in Spain, while the others describe the British Isles from experience derived on the spot.' Then, remarking that the alleged derivation of κασσίτερος (the Greek word meaning 'tin') from a Sumerian word and from an Assyrian word have been proved to be fanciful, he argues that κασσίτερος did not, as most ancient and modern writers have supposed, give its name to the Cassiterides, but on the contrary derived its name from theirs. Similarly, he points out, at least four names of metals have been derived from the names of places which produced them, namely, copper from Cyprus; silver (in Gothic *silubr*) from the town of Salybe in Pontus;[1] bronze from Brundisium; and *Kalay*, the Turkish word for tin, from Kalah in the peninsula of Malacca. M. Reinach goes on to argue that as the Greeks derived their knowledge of the Cassiterides from the Phoenicians, the termination ιδες must have been added by them. There remains therefore *cassiteros*, of which the first part is found in numerous Celtic words, for example, *Cassi*, *Cassi*-vellaunus, Velio-*casses*, &c. M. Reinach gives reasons, which appear to me unsatisfactory, for the conjecture that *Cassiterides* means the same as *insulae extimae* ('the remotest isles'); and he holds that the name was given to the British Isles by the Celts of Western Gaul.[2]

Whatever M. Reinach's argument may be worth, he and Müllenhoff are unquestionably right in one sense: the British Isles, taken as a whole, were the only islands from which the ancients derived tin. But this truism did not require demonstration. The question is, whether the identification of the Cassiterides with the British Isles can be reconciled with what was written about them by the ancient geographers.

IV. The story which Strabo tells about Publius Crassus presents some difficulty. As we have seen, he says that after the Romans had discovered the route to the Cassiterides in spite of the efforts which the Phoenicians made to conceal it, Crassus sailed across to the islands, ascertained that the tin lay near the surface, and indicated the new route for the benefit of traders. The first question is, who was Crassus? Unger[3] maintains that he was the consul of 95 B.C. who conquered the Lusitanians. If so, he must have sailed from the mainland to the islands near Vigo which Unger identifies with the Cassiterides. But, as I have already pointed out, these islands are quite close to the coast: their distance from the mainland is not greater, but many times less than the distance of Britain from the Continent; their whereabouts could never have been kept secret; and they have never produced tin. Therefore, if Publius Crassus was the consul of 95 B.C., Strabo's story is utterly untrustworthy.

[1] Σαλύβη, ὅθεν ἀργύρου ἐστὶ γενέθλη. Homer, *Il.*, ii, 857.

[2] *L'Anthr.*, iii, 1892, pp. 277–80. For unfavourable criticisms of M. Reinach's view see O. Schrader, *Reallexicon der indogermanischen Altertumskunde*, p. 993, and *Eng. Hist. Rev.*, xix, 1904, p. 140.

[3] *Rhein. Mus.*, xxxviii, 1883, p. 164.

Mommsen[1] holds that Crassus was Caesar's lieutenant of that name, and that he sailed from Gaul to the Scilly Islands before Caesar's first invasion of Britain.[2] How then are we to account for the ignorance of Caesar, who tells us that tin was produced 'in the midlands' (*in mediterraneis regionibus*[3]) of Britain? Professor Ridgeway, who believes that the Cassiterides were the islands near Vigo, also identifies Crassus with Caesar's lieutenant, who, as he reminds us, invaded Aquitania—the south-western division of Gaul—in 56 B.C. 'He is all the more likely,' writes Professor Ridgeway,[4] 'to have passed into Northern Spain, inasmuch as the people of that region had given great assistance to the Aquitani . . . (*B. G.*, iii, 23). Without doubt he was fully aware of the mineral wealth of that country, as is shown by Caesar's remark (iii, 21) on their skill in defending cities, in consequence of their having numerous copper mines and other works in that region. As is plain from Strabo's words, the Romans already knew how to reach the tin islands by sea, coasting round from the Mediterranean and up from Gades on the old Phoenician track. Crassus, then, by opening up a far shorter route, that of a short sea voyage from the Cassiterides to the coast of Gaul (possibly to the Garonne), at once developed this trade. The ore lay near the surface. The distance by sea was greater than that across the English Channel, but the readiness with which the tin was obtained, combined with the shorter land transit, more than compensated this. Strabo is evidently contrasting the rival tin-producing regions when he introduces the allusion to Britain . . . From this achievement of Crassus and its results we can now understand in its proper light the famous expression of Pytheas, that "the northern parts of Iberia are more accessible towards Keltiké than for those who sail by the ocean" . . . He found, as Publius Crassus found three centuries later, that the mineral regions and islands of North-Western Spain were far more accessible for the Massaliotes by a land journey across Gaul and a short sea voyage than by the long and perilous route round by Gibraltar.' But Professor Ridgeway mistranslates 'the famous expression of Pytheas',—*τὰ προσαρκτικὰ μέρη τῆς Ἰβηρίας εὐπαροδώτερα εἶναι [τοῖς] πρὸς τὴν Κελτικὴν ἢ κατὰ τὸν ὠκεανὸν πλέουσι.*[5] He fails to see that the word *πλέουσι* refers to *πρὸς τὴν Κελτικήν* as well as to *κατὰ τὸν ὠκεανόν*. The passage simply means that it is easier to sail along the northern coast of Iberia (Spain) from west to east in the direction of Keltiké (Gaul) than to sail along the southern coast from east to west in the direction of the Atlantic.[6]

[1] *Hist. of Rome*, v, 1894, p. 63 (*Röm. Gesch.*, iii, 1889, p. 269).

[2] George Smith (*The Cassiterides*, p. 80) remarks that if Crassus was Caesar's lieutenant, his discovery of the Cassiterides 'must have taken place after the time of Julius Caesar'. But Smith forgets that this Crassus died in 53 B.C.

[3] *B. G.*, v, 12, § 5.

[4] *Folk-Lore*, i, 1890, pp. 91–2.

[5] Strabo, iii, 2, § 11.

[6] Groskurd (*Strabonis Erdbeschreibung*, i, 1831, p. 249) translates the passage: 'dass Iberiens nördliche Küsten gegen Keltike leichtere Vorbeifahrt haben, als wenn man dem Ocean entgegenschiffe.' C. Müller, however, in his edition of Strabo (p. 953), rejects Groskurd's attempt to defend the common text, and

This, as Müllenhoff[1] observes, is perfectly true, owing to the set of the current and the prevalence of westerly winds. Moreover, Professor Ridgeway does not seem to be aware that there are no 'tin islands' off the coast of Spain: he does not explain how Crassus could have found time in 56 B.C. to make the 'short sea voyage' of five hundred miles or more from the mouth of the Garonne to the neighbourhood of Vigo, when he was campaigning in Aquitania until the approach of winter;[2] nor, finally, does he explain how the Massaliotes would have gained by conveying tin five hundred miles from the neighbourhood of Vigo to the mouth of the Garonne, and then considerably more than three hundred miles across Gaul to Massilia, instead of overland across Spain. Mr. Tozer[3] disposes of the difficulty by simply discrediting Strabo's account. 'There is no reason,' he says, 'to doubt that Crassus made such an expedition; but whatever the place was to which he went, his account is quite untrustworthy, because he represents the Cassiterides as producing tin, whereas that metal is not found in any of the groups of islands which lie off the coasts of Gaul, or Britain, or Spain. The explicit character of his statements, however, seems to have deceived his contemporaries, and Strabo among them.' But what theory can Mr. Tozer frame to account for the gratuitous mendacity which he imputes to Crassus, who, by the way, was not Strabo's contemporary?[4] Strabo's story is, in any case, obviously inaccurate:[5] but I agree with Mr. Tozer that it contains a kernel of truth; and I can only suppose that Crassus, when he was in Brittany in 57–56 B.C.,[6] was directed by Caesar to visit and report upon the tin-producing districts of the British Isles.[7] And if I am asked how I account for the mistake which Caesar made when he said that tin was produced in

holds that we should read *τὰ προσαρκτικὰ μέρη τῆς Ἰβηρίας εὐπαροδώτερα εἶναι τοῖς πρὸς τὴν Κελτικὴν κατὰ τὸν ὠκεανὸν πλέουσι*, mentally supplying after *εὐπαροδώτερα* the words *τῶν νοτίων*, if indeed they were not in Strabo's manuscript; and he gives good reasons for believing that Pytheas meant to say what I have stated in the text.

[1] *Deutsche Altertumskunde*, i, 1890, p. 370.

[2] *B. G.*, iii, 27, § 2.

[3] *Hist. of Anc. Geogr.*, pp. 38–9.

[4] Unless a child born in 1888 could have been called Mr. Gladstone's contemporary. Strabo was born about 63, and Crassus died in 53 B.C.

[5] As Sir George Cornewall Lewis pointed out (*Hist. Survey of the Astronomy of the Ancients*, 1862, p. 452), 'the Romans . . . were not likely to attempt voyages beyond the Pillars of Hercules before . . . 146 B.C., whereas after that time the Carthaginians had no ships or factories; Gades had been sixty years in the hands of the Romans; and ever since the end of the Second Punic War the Romans had been able to extort the secrets of the Carthaginians . . . The story doubtless originated in the known commercial jealousy of the Carthaginians,' &c.

M. Salomon Reinach (*L'Anthr.*, x, 1899, p. 400) holds that the Romans were anxious to ascertain the maritime route to the Cassiterides because it was cheaper than the overland route. But is it certain that a voyage of more than 2,000 miles would have been cheaper than a land journey of 600?

[6] *B. G.*, ii, 34; iii, 7, § 2.

[7] Cf. H. Berger, *Gesch. der wissenschaftlichen Erdkunde der Griechen*, iii, 1891, pp. 29, 34.

the interior of Britain, I offer the following suggestion. Crassus may have contented himself with landing on the coast, perhaps at or near St. Michael's Mount, where the tin was delivered to the merchants :[1] if so, he was doubtless informed that the tin was actually won in the interior, as, in literal truth, it of course was ;[2] and Caesar may have hastily concluded from his report that the tin mines were far from the coast. As to the details with which Strabo embellished his story, it would be idle to conjecture from what source they were obtained. We may be sure that he did not invent them ; but he may have confused items of information furnished by different authorities.[3]

V. The conclusion of the matter is this. The statements of Strabo are most satisfactorily explained on the hypothesis that those from whom he, directly or indirectly, derived his information referred to the Scilly Isles and probably also the Cornish peninsula, or (which is less probable) to islands off the coast of Brittany, at which trading vessels may have touched on the voyage. All the other ancient writers, except perhaps Polybius, undoubtedly associated the Cassiterides with Spain. In so doing they were mistaken ; for no islands in Spanish waters, except Ons, which is out of the question, have ever produced tin. The real Cassiterides—the ' tin islands ' which were known to the mariners from whom the ancient writers ultimately derived their notions—were, speaking generally, the British Isles, and particularly, the tin-producing districts of Cornwall and perhaps also the Scilly Islands. It is possible that Polybius[4] may have held this view ; for he does not mention the Cassiterides, and names the British Isles as the source of tin.

How the ancients came to entertain such vague notions about the Cassiterides, is not difficult to conceive. Evidently, when they first heard of them, all that they could learn was that they were somewhere in the western ocean. Knowing that Gades was the centre of the tin trade, they would naturally assume that they were in Spanish waters[5] ; and even when they learned that tin came from Britain and from Galicia, they would cling to the idea that it came also from islands, the geographical position of which the crafty Phoenicians had striven to keep secret. Mr. Tozer[6] may possibly be right in

[1] See pp. 500–7, *infra*.

[2] H. Berger (*op. cit.*, p. 29) affirms that, according to Strabo (iii, 5, § 11), Crassus saw ' with his own eyes ' the tin-mining actually going on ; but Strabo does not say this.

[3] Berger (*op. cit.*, pp. 34–5) points out that Crassus's description [was it his ?], reproduced by Strabo, puts us in mind of that of Diodorus (pp. 499, 506, *infra*), and may have been suggested to Crassus by a perusal of Diodorus's authority. R. Zimmermann, on the contrary, argues (*Hermes*, xxiii, 1888, pp. 121–3) that the passage in Strabo is based upon Posidonius. Obviously not the part which relates to Crassus.

[4] *Ἴσως γὰρ δή τινες ἐπιζητήσουσι, πῶς . . . οὐδὲν ἐπὶ πλεῖον εἰρήκαμεν . . . περὶ τῶν Βρεττανικῶν νήσων, καὶ τῆς τοῦ καττιτέρου κατασκευῆς, ἔτι δὲ τῶν ἀργυρείων καὶ χρυσείων τῶν κατὰ τὴν Ἰβηρίαν*, &c. (iii, 57, §§ 3–4).

[5] See E. H. Bunbury, *Hist. of Anc. Geogr.*, i, 1879, p. 12.

[6] *Hist. of Anc. Geogr.*, p. 38.

suggesting that ' when the nations about the Mediterranean obtained more accurate information concerning the north-western coasts of Europe, it was natural that they should affix the name to one or other of the groups of islands with which they found the trade to be associated'. ' Thus,' he continues, ' by some writers it may have been attached to the Oestrymnides, by others to the islands of the Galician coast, and even the Scillies may in some cases have been intended.' But is it not likely that the writers in question, when they attempted to locate the Cassiterides, were not identifying them with any group of islands the existence of which was certainly known to them, and the whereabouts of which they knew ? M. Salomon Reinach puts the matter well, though he fails to perceive that Strabo was not referring to islands in Spanish waters. ' There were two traditions,' he says, ' relating to the tin islands,—one Phoenician, of which the starting-point was Southern Spain ; the other Greek, which originated at Marseilles. With that respect for the written word which characterized them, the ancients accepted the two traditions side by side . . . Even after the expedition of Crassus . . . Pliny dared not reject the geographical legend which connected the islands with Spain ; and a century later Ptolemy persisted in the same error.' [1]

Mr. W. H. Stevenson explains that Müllenhoff, of whose conclusions respecting the Cassiterides he gives a lucid summary, holds that they ' were marked by guess-work on the early Greek maps . . . off the north-west coast of Spain . . . and that they there remained on the maps (much like the mythical island of Brazil in fifteenth-century maps), although they had been known since the time of Pytheas, under the names of Britannia, Albion, Ierne, &c., without their identity being suspected. In a precisely similar manner the Electridae, which had been put into the maps by guess-work, were retained long after it was known that amber came from the shores of the Baltic, and not from islands in the North Sea.' [2]

Thus the important point to bear in mind is that the name *Cassiterides*, which must, as Kiepert says, have been originally applied to the British Isles, was afterwards misapplied to imaginary islands, and applied by Strabo, not perhaps without some foundation in fact, to the Scilly group.

[1] *L'Anthr.*, x, 1899, p. 401. Cf. H. d'Arbois de Jubainville, *Les premiers habitants de l'Europe*, i, 1889, pp. 45–6.

[2] *Engl. Hist. Review*, xix, 1904, p. 140, note.

II. ICTIS AND THE BRITISH TRADE IN TIN

Let us now consider the British trade in tin.

I. Diodorus Siculus[1] states, on the authority of Timaeus, who derived his information on this matter from Pytheas,[2] that tin was conveyed by the people of Belerium (the Land's End) in wagons at low tide from the British mainland to an island called Ictis; purchased there by merchants from the natives; carried to Gaul; and transported on pack-horses to the mouth of the Rhône,[3] the overland journey lasting thirty days. In another chapter[4] he says, following Posidonius, that tin was carried from Britain to Gaul, and then conveyed on horseback to Massilia and to Narbo. Pliny[5] states,

[1] *Bibl. Hist.*, v. 22, § 2.—Κομίζουσιν εἴς τινα νῆσον προκειμένην τῆς Πρεττανικῆς, ὀνομαζομένην δὲ Ἶκτιν· κατὰ γὰρ τὰς ἀμπώτεις ἀναξηραμένου τοῦ μεταξὺ τόπου, ταῖς ἁμάξαις εἰς ταύτην κομίζουσι δαψιλῆ τὸν κασσίτερον. Ἐντεῦθεν δ' οἱ ἔμποροι παρὰ τῶν ἐγχωρίων ὠνοῦνται καὶ διακομίζουσιν εἰς τὴν Γαλατίαν· τὸ δὲ τελευταῖον πεζῇ διὰ τῆς Γαλατίας πορευθέντες ἡμέρας ὡς τριάκοντα κατάγουσιν ἐπὶ τῶν ἵππων τὰ φορτία πρὸς τὴν ἐκβολὴν τοῦ Ῥοδανοῦ ποταμοῦ.

[2] See K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, pp. 471–2; H. Berger, *Gesch. der wissenschaftlichen Erdkunde der Griechen*, iii, 1891, pp. 34–5; and Pauly's *Real-Encyclopädie*, iii, part i, 1897, p. 860. Müllenhoff justly remarks that the account which Diodorus gives in v, 22 of the mode in which the tin trade was conducted must have been derived from an eye-witness; and that of all the ancient writers Pytheas was the only one who saw with his own eyes what went on at Ictis. Professor Ridgeway assumes that Diodorus's account of Ictis was borrowed from Posidonius; but the descriptions which Elton (*Origins of Eng. Hist.*, 1890, pp. 30–1, 34–5, 92) and Professor Rhys (*Celtic Britain*, 1904, pp. 45–6) have published of the visit of Posidonius to Britain are purely imaginary; for there is absolutely no evidence that he ever crossed the Channel. Elton refers to a passage in the *Solutiones* of Priscian of Lydia, a writer of the sixth century *quaest.* vi, p. 571 of F. Dübner's edition), which proves nothing about Posidonius. See J. Bake, *Posidonii Rhodii reliquiae doctrinae*, 1810; *Fragm. hist. Graec.*, ed. C. Müller, iii, 1849, pp. 245–96; R. Scheppig, *De Posidonio*, 1869, p. 7; *Rev. celt.*, vii, 1886, p. 378; and M. Dubois, *Examen de la géogr. de Strabon*, 1891, p. 327.

[3] Professor Rhys (*Celtic Britain*, 1904, p. 45) says that, according to Diodorus, the tin was brought 'to the outlet of the Rhone, that is to say, to the meeting of the Rhone and the Saone', &c. But πρὸς τὴν ἐκβολὴν τοῦ Ῥοδανοῦ ποταμοῦ can only mean 'to the mouth of the Rhône'. Ἐκβολή sometimes means the issue of a river from a mountainous country: it cannot mean that part of a river where it is joined by an affluent; and I doubt whether the professor would seriously maintain that 'the outlet of the Rhône' is at Lyons.

[4] v, 38, § 5.—Πολὺς δὲ καὶ ἐκ τῆς Πρεττανικῆς νήσου διακομίζεται πρὸς τὴν κατ' ἀντικρὺ κειμένην Γαλατίαν, καὶ διὰ τῆς μεσογείου Κελτικῆς ἐφ' ἵππων ὑπὸ τῶν ἐμπόρων ἄγεται παρά τε τοὺς Μασσαλιώτας καὶ εἰς τὴν ὀνομαζομένην πόλιν Ναρβῶνα.

[5] *Nat. Hist.*, iv, 16(30), § 104.—Timaeus historicus a Britannia introrsum sex dierum navigatione abesse dicit insulam Mictim in qua candidum plumbum proveniat; ad eam Britannos vitilibus navigiis corio circumsutis navigare. E. H. Bunbury (*Hist. of Anc. Geogr.*, i, 1879, p. 603, n. 9) remarks that 'it is impossible to say what sense we are to attach to the word "introrsus", upon which the interpretation of the whole passage, in a geographical sense, depends'. I shall show presently (p. 505, *infra*) that only one sense which is not nonsense can be attributed to *introrsum*.

Müllenhoff (*Deutsche Altertumskunde*, i, 1890, p. 471) holds that Pliny confused the distance of Ictis from Britain with that of Thule, which, as he says in an earlier passage (*Nat. Hist.*, ii, 75 [77], § 187), was 'six days' sail northward from Britain' (*sex dierum navigatione in septentrionem a Britannia*). See p. 505, *infra*.

quoting Timaeus as his authority, that there was an island called Mictis, six days' sail from Britain, which produced tin, and to which the Britons sailed in coracles. Strabo tells us that Corbilo in the estuary of the Loire[1] 'was formerly an emporium'; and, as we learn from Polybius, who couples it with Narbo and Massilia, that in the time of Scipio Aemilianus it was one of the principal towns of Gaul,[2] it is probable that it was at one period the Gallic port to which British tin, destined for the Mediterranean markets, was conveyed.[3]

II. Now the first thing to do is to identify Ictis or Mictis; for it is admitted that they were the same.[4] According to Elton[5] and Professor Rhys,[6] Ictis was the Isle of Thanet. 'The important point', says Elton, 'remains that the tin . . . was stored at some place, which was supposed to have lain at six days' voyage from the mineral district; and it seems reasonable to identify it with the Isle of Thanet, at which the marts were established from which the merchants made the shortest passage to Gaul.' But there is no evidence that 'the marts were established' in the Isle of Thanet, or that 'the merchants made the shortest passage to Gaul'; nor is there one word in Pliny (whose statement shall be considered presently) to justify Elton in stating as a 'fact' that the tin was 'stored at some place which was supposed to have lain at six days' voyage from the mineral district'.[7] The view that Ictis was the Isle of Thanet is absolutely untenable. 'If,' says Professor Ridgeway,[8] 'it was Thanet, it

[1] The geographical position of Corbilo cannot be fixed. Desjardins (*Géogr. de la Gaule rom.*, i, 1876, p. 288) was originally inclined to place it near Beslon in the peninsula of Guérande, because the neighbourhood is 'rempli de souvenirs celtiques'. Beslon is no more on the Loire than Margate is on the Thames; and if the tin had been landed there, it would have been necessary either to tranship it and carry it across the Loire, or to take the pack-horses by a round-about route up the valley of that river. Afterwards (*ib.*, ii, 1878, pp. 139, 484–5, 485, n. 1) Desjardins changed his mind, and identified Corbilo with St.-Nazaire: 'cet emplacement', he remarked, anticipating one of the objections which I have just made against his former view, 'cet emplacement s'accorde-t-il beaucoup mieux que celui de Beslon avec le texte de Strabon, qui porte cet ancien port sur la Loire, et non sur la mer.' He relied mainly upon the investigations of an engineer, M. René Kerviler, who, 'ayant eu l'occasion de faire des travaux d'approfondissement à Saint-Nazaire, y a découvert des substructions qui avaient fait vraisemblablement partie de l'ancien port de *Corbilon*.' See *Rev. arch.*, nouv. sér., xxxiii, 1877, pp. 145–53, 230–9, 342–53. M. Kerviler himself identified the remains with those of the *Brivates portus* of Ptolemy, *Geogr.*, ii, 8, § 1.

[2] *Geogr.*, iv, 2, § 1.—*πρότερον δὲ* Κορβιλὼν *ὑπῆρχεν ἐμπόριον ἐπὶ τούτῳ τῷ ποταμῷ, περὶ ἧς εἴρηκε* Πολύβιος, *μνησθεὶς τῶν ὑπὸ* Πυθέου *μυθολογηθέντων, ὅτι* Μασσαλιωτῶν *μὲν τῶν συμμιξάντων* Σκιπίωνι *οὐδεὶς εἶχε λέγειν οὐδὲν μνήμης ἄξιον ἐρωτηθεὶς ὑπὸ τοῦ* Σκιπίωνος *περὶ τῆς* Βρεττανικῆς, *οὐδὲ τῶν ἐκ* Νάρβωνος *οὐδὲ τῶν ἐκ* Κορβιλῶνος, *αἵπερ ἦσαν ἄρισται πόλεις τῶν ταύτῃ.*

[3] Cf. *Folk-Lore*, i, 1890, pp. 85–6, and H. F. Tozer, *Hist. of Anc. Geogr.*, p. 36.

[4] Cf. K. Müllenhoff, *Deutsche Altertumskunde*, i, 1890, p. 471, and D. Detlefsen in W. Sieglin's *Quellen und Forschungen*, &c., Heft 9, p. 77.

[5] *Origins of Eng. Hist.*, 1890, p. 34.

[6] *Celtic Britain*, 1904, p. 46.

[7] Mr. Alfred Tylor blunders even more hopelessly than Elton. 'The transhipment of tin', he says (*Archaeologia*, xlviii, 1885, p. 233), 'was described by ancient writers as taking place at Vectis, six days' sail from Cornwall.'

[8] *Folk-Lore*, i, 1890, pp. 95–7.

follows that the tin was brought all the way from Devon, which was impossible, as the great forest of Anderida stretched right from Hampshire into Kent.' Formerly the professor held that 'the only difficulty in identifying Ictis with the Isle of Wight is the statement of Diodorus . . . that the tin was conveyed across to the island at low water'; for 'geologists maintain that Wight could not have been joined to the mainland in historic times'. Geologists, however, as we shall presently see, have changed their minds; and accordingly Professor Ridgeway has changed his. I shall therefore only take account of those parts of his argument which are not obsolete. 'Mr. Elton,' he observes, 'seems to forget that if the Britons brought the tin a six days' voyage from Cornwall to Thanet, there would be no need to bring it overland by waggons across the estuary at low water . . . Diodorus and Timaeus are substantially agreed that there was an island where the tin came to market, and that its name was Ictis or Mictis . . . The tin could not be carried overland on account of the forests, and they certainly would not convey it all round the south and south-east coasts to the Straits, and then round the coast of Gaul to Corbilo, if it was at all possible to get across at a nearer point. The passage from the Isle of Wight to the Channel Islands, and thence to Armorica and Corbilo, would best attain this object.' Professor Ridgeway then invokes numismatic evidence. He states that Gallic coins of a peculiar type have been found in the southern and western parts of England, in the Channel Islands, and in the territories of the Turones, Pictones, Redones, Namnetes, all the tribes of the Armorican peninsula, and the Volcae Tectosages. 'Follow the peoples enumerated above on the map,' he says, 'and we shall find them all lying in the basins of the Garonne and Loire . . . This evidence, then, points unmistakably to a route direct from Armorica to the southern coast of Britain, or, in other words, supports strongly the doctrine that the Isle of Wight was the island called Ictis.'[1]

Professor Ridgeway's arguments, as directed against the theory of Elton and Professor Rhys, are conclusive. Ictis was certainly not Thanet. But the argument which he adduces from numismatic evidence in favour of its identification with the Isle of Wight rests upon the assumption that the coins in question could not have found their way to the Channel Islands except in the course of the tin trade. The Dumnonii, in whose country the tin was produced, had no coinage of their own, and apparently made little use of money:[2] the coins to which Professor Ridgeway alludes were far later than the time of Pytheas; and the professor himself affirms that in the time of Posidonius, whom he wrongly regards as Diodorus's authority for the description of Ictis, the route from Ictis to Corbilo had been abandoned. Nor is it easy to understand why the traders who conveyed tin from Cornwall to Marseilles should have needlessly added between 300 and 400 miles to the length and a corresponding amount to the expense of the journey. Professor Ridgeway has himself

[1] *Ib.*, pp. 98–101.

[2] See pp. 250, 359–60, *supra*.

made use of this very argument to prove that Ictis was not the Isle of Thanet: can he not see that it tells with equal force against his own theory, that Ictis was the Isle of Wight?[1]

Mr. Alfred Tylor[2] insists that 'St. Michael's Mount', which was formerly identified with Ictis, 'is a steep rock, and does not form a harbour at all.' What if it is a steep rock? Does not Thucydides[3] tell us that the Phoenicians 'fortified headlands on the sea-coast [of Sicily], and settled in the small islands adjacent, for the sake of trading with the Sicels'?[4] Nobody who knows St. Michael's Mount will contend that there would have been the slightest difficulty in conveying tin on to the small plain on its landward side,[5] or in loading with tin vessels moored beneath it. Diodorus Siculus does not mention any harbour in connexion with Ictis; but, as a writer who knew every inch of the Cornish coast long ago pointed out, St. Michael's Mount afforded perfect shelter for shipping.[6] 'It still,' says Sir Charles Lyell,[7] 'affords a good port, daily frequented by vessels, *where cargoes of tin are sometimes taken on board, after having been transported, as in the olden time, at low tide across the isthmus.*[8]

[1] Mr. Alfred Tylor (*Archaeologia*, xlviii, 1885, p. 233) argues, in favour of the identification of Ictis with the Isle of Wight, that 'Stans Ore Point is said to be named from Stannum (tin)'; and Elton (*Origins of Eng. Hist.*, 1890, p. 230) thinks that 'the course of the metal-trade may be indicated by the names of places on the coast-road leading eastward from the Exe, as . . . Stans Ore Point'. Now, as O. Schrader points out (*Prehist. Ant. of the Aryan Peoples*, 1890, p. 217), *stannum* probably did not get the meaning of 'tin' before the fourth century A.D.; and even if the derivation in question could be established, it would not prove that Ictis was the Isle of Wight. Tin was doubtless conveyed eastward from Cornwall; but not for the supply of the Mediterranean markets.

[2] *Archaeologia*, xlviii, 1885, p. 236.

[3] vi, 2, § 6.—Ὡκουν δὲ καὶ Φοίνικες περὶ πᾶσαν μὲν τὴν Σικελίαν ἄκρας τε ἐπὶ τῇ θαλάσσῃ ἀπολαβόντες καὶ τὰ ἐπικείμενα νησίδια ἐμπορίας ἕνεκεν τῆς πρὸς τοὺς Σικελούς.

[4] B. Jowett, *Thucydides translated into English*, i, 1881, p. 409.

[5] *Report of . . . the Brit. Association*, 1865 (1866), p. 71.

[6] *Trans. Roy. Geol. Soc. Cornwall*, iii, 1828, pp. 91–4.

[7] *Principles of Geology*, i, 1875, pp. 546–7.

[8] The italics are mine. Müllenhoff (*Deutsche Altertumskunde*, i, 1890, pp. 471–2) asserts that 'Ictis can only be looked for at the promontory of Belerium' [the Land's End], and that 'it is undoubtedly one of the small islands off the Land's End, which are marked on the Ordnance Map (sheets 32 and 33)'. It must be presumed that Müllenhoff came to this singular conclusion because Pytheas landed at Belerium. But there is no reason to suppose that he landed at the precise spot which we call the Land's End; and if he did he certainly went on to visit the tin mines. If Müllenhoff had known the Cornish coast, or even studied the map carefully, he would have seen that tin could not have been conveyed in carts down the cliffs opposite the small islands to which he refers, and that, as Dr. Barham says (*Trans. Roy. Geol. Soc. Cornwall*, iii, 1828, p. 91), 'there is not . . . any other island [besides St. Michael's Mount] on the Cornish, or any neighbouring shores to which carts can pass at low water; there is no other spot, at all answering to the description of Diodorus, which becomes alternately an island and a peninsula with the changes of the tide.'

George Smith (*The Cassiterides*, p. 114) points out that 'twelve miles to the west of St. Michael's Mount, and eighteen miles to the east of it, comprehend almost the whole of the ancient tin mining district'. Professor Rhys, on the other hand, states (*Celtic Britain*, 1904, p. 44) that the tin districts 'in ancient times were chiefly Dartmoor, with the country around

Colliers of 500 tons' burden can now enter the harbour, which is on the landward or sheltered side of the Mount.'

But the Isle of Wight has recently found a new champion,—the eminent geologist, Mr. Clement Reid.[1] He affirms that at the time when tin was shipped at Ictis, 'St. Michael's Mount must have been an isolated rock rising out of a swampy wood.' By an interesting process of reasoning, based upon evidence which he collected while revising 'the geological map of the northern part of the Isle of Wight', and afterwards while mapping 'the whole of the adjacent parts of the mainland', he arrives at the conclusion that about 100 B.C. a limestone causeway, over which wagons could pass at low tide, extended from the western side of the river Yar to the coast of Hampshire opposite Pennington Marshes. He explains that the tin was transported by this causeway to the Isle of Wight instead of being shipped in one of the Hampshire harbours because the latter 'are all more or less exposed to the prevalent south-west wind, and are sheltered by no high land', and, moreover, 'the harbours outside the Solent were probably always rendered dangerous by bars of sand and shingle.' Finally, he contends that the identification of Ictis with the Isle of Wight shows that 'the ancient writers can be literally depended on, and that their descriptions are thoroughly in keeping with each other'. Pliny was right in saying that Mictis 'is distant inwards from Britain six days' voyage', for 'six days' coasting from the mouth of the Exe would amply suffice to bring boats to the Isle of Wight'; and since 'a coasting trade of this sort would go direct to the Isle of Wight side of the Solent', Pliny's account, which is based on Timaeus, naturally makes 'no mention of the causeway alluded to by Diodorus, writing at a later date'. (Mr. Reid presumably means, not that Diodorus wrote later than Pliny, but that Posidonius, whom he assumes to have been Diodorus's

Tavistock, and that around St. Austell, including several valleys looking towards the southern coast of Cornwall'; and he adds that 'in most of the other districts where tin existed it is supposed to have lain too deep to have been worked in early times'. I do not know whether among these 'other districts' he includes the one near St. Michael's Mount; but it is certain that the tin in this district was worked in early times. It was the district of Belerium, where the tin-workers mentioned by Diodorus lived; and he says that there were veins of tin in the hard rock near the surface (*αὕτη δὲ πετρώδης οὖσα διαφυὰς ἔχει γεώδεις, ἐν αἷς τὸν πόρον κατεργαζόμενοι καὶ τήξαντες καθαίρουσιν* [v, 22, § 2]. Cf. Strabo, iii, 5, § 11, and *Ency. Brit.*, 9th ed., vi, 425). Mr. P. W. Flower (*Hist. of the Trade in Tin*, 1880, p. 26) tells us that from pre-Roman days 'Cornish men have been sinking deeper and deeper in their search for cheaper metal'; while Prof. Haverfield (*Proc. Soc. Ant.*, 2nd ser., xviii, 1900, p. 122), after remarking that 'the tin districts of Dartmoor [were] worked largely in the middle ages', says, 'The Dartmoor tin is, I believe, far more difficult to work than the Cornish, and this fact may explain the Roman neglect of it.' See also, for evidence that Cornish tin was won in the Bronze Age, *Archaeologia*, xvi, 1812, p. 137, pl. 10; xlix, 1885, p. 181; and *Archaeol. Journal*, xxxi, 1874, pp. 53, 60. I am astonished to find that M. Salomon Reinach (*L'Anthr.*, xvii, 1906, pp. 235–6), noticing a paper the writer of which maintains that no tin was worked in Britain until after the date of Domesday Book, says, 'Cette manière de voir, bien que contredite par les textes, mérite réflexion.'

[1] *Archaeologia*, lix, part ii, 1905, pp. 281–8.

authority, wrote later than Timaeus.) Caesar is right in saying that tin was found in the interior, ' for he refers to the British part of the trade-route,' that is to say, the (assumed) overland journey from Cornwall to the Hampshire coast. Diodorus is right because the limestone causeway answers to his description.

I submit that whoever is right, Mr. Clement Reid is wrong, because the only equipment which he brings to the discussion is the special knowledge of the geologist. Doubtless he has proved the former existence of a causeway between Hampshire and the Isle of Wight; but it does not follow that the Isle of Wight was Ictis unless it can be proved that ' St. Michael's Mount must have been an isolated rock rising out of a swampy wood '.

Can this be proved ? I have searched all the relevant geological and geographical literature, and have failed to find any evidence in support of Mr. Reid's assertion. The testimony of geologists, except Mr. Reid, is all the other way. Sir Charles Lyell,[1] Mr. Pengelley,[2] and Mr. Ussher [3] of the Geological Survey all hold that since the time when tin was shipped at Ictis, St. Michael's Mount has undergone no sensible change. But Mr. Reid has recently been revising the old geological survey of Cornwall; and he tells me that he reached his conclusion by calculating the rate at which the sea washed away alluvium which once connected St. Michael's Mount with the mainland. Moreover, although he does not actually rely upon the hoary fable, demolished by Max Müller, of ' the Hoar Rock in the Wood ', he laid stress in conversation with me upon the prevalence in Cornwall of a tradition which supported his conclusion,—a tradition which, Max Müller's readers know, is simply worthless.[4]

Now I would ask geologists whether it is not dangerous to strive after chronological precision in geological inquiries by reasoning which assumes that nature worked during a long period of remote time at a uniform rate of speed. The calculations by which Sir Archibald Geikie laboured years ago to estimate the time which the Thames occupied in excavating its valley,[5] the calculations which geologists have made as to the time required for the deposition of the layers of stalagmite in caves,[6] have been proved to be futile. This much at all events is certain: if Mr. Reid's calculation is accurate, it stultifies the testimony of the ancient authors to whom he appeals.

For I would ask Mr. Reid how he proposes to reconcile his own statement, ' that the ancient writers can be literally depended on,' with the assumption, which he admits that he is compelled to make in order to show ' the perfect consistency of the accounts ', that ' Mictis and Ictis were the same island as Vectis '. Is he not aware that in

[1] *Principles of Geology*, i, 1875, pp. 543–4.
[2] *Report of . . . the Brit. Association*, 1865 (1866), p. 71.
[3] *Geol. Mag.*, 1879, pp. 74–5.
[4] *Chips from a German Workshop*, iii, 1870, pp. 330–57. Elton, even in his second edition (*Origins of Eng. Hist.*, 1890, p. 37), repeated the obsolete argument alluded to in the text.
[5] See p. 31, *supra*.
[6] See p. 222, *supra*.

Pliny's *Natural History*[1] [M] ictis and Vectis are distinguished ? If he had studied Müllenhoff's great work, he would not have attempted to reconcile Pliny's account of the six days' voyage to [M]ictis with Diodorus's account, which 'mentions only the causeway to Ictis', by assuming that the writer whom Diodorus followed lived two centuries later than Timaeus. For Diodorus's account was not, as Mr. Reid fancies, based upon Posidonius ; he also, like Pliny, derived his information immediately from Timaeus, ultimately from Pytheas. Not less hopeless is Mr. Reid's attempt to explain Pliny's account of the voyage to [M]ictis. How could the Isle of Wight be described as 'distant inwards from Britain six days' voyage' ? Because, says Mr. Reid, 'the Isle of Wight and more easterly parts of the south of England were politically part of Gaul perhaps even at that early date [300 B.C.] ; the tin-producing "Britain" was apparently outside the dominion of the Belgae, and must have been Devon and Cornwall.' This argument rests upon a doubtful 'perhaps', an obscure 'apparently', a desperate 'must have been', and the baseless assumption that the Belgae had established dominion in Britain in the time of Pytheas : it leaves the word 'inwards' unexplained ; and it is pulverized by the mere fact that in the very chapter from which Mr. Reid is quoting and everywhere else Pliny uses the word Britain not in the sense of 'Devon and Cornwall', but simply in the sense of Britain. To any man who is not obliged to distort the plain meaning of words it is clear that, from Pliny's point of view, Ictis was six days' sail from Britain, and that by 'inwards' he meant, speaking from the standpoint of an Italian, 'northward.' Thus London might be intelligibly described as fifty-two miles 'inwards' from Brighton ; but to say that Brighton is a day's sail 'inwards' from Portsmouth would be gibberish. As Müllenhoff has pointed out, Pliny confounded the distance of Ictis from Britain with that of Thule.[2]

Enough of Mr. Reid's attempt to reconcile the irreconcilable. Like Professor Ridgeway, he does not explain why men of business preferred to pay the cost of the long voyage from the Isle of Wight to the mouth of the Loire, when they need only have paid for the shorter voyage from Cornwall, or why they chose to saddle themselves with the cost of the overland transport from Cornwall to Hampshire. Nor does he explain why this imaginary and expensive overland transport was substituted for the imaginary coasting voyage. Nor again does he explain how wagons, loaded with tin (for Diodorus does not speak of pack-horses except in connexion with the journey across Gaul), were able to travel two hundred miles along unmetalled trackways. The rate at which they crawled, the numerous breaks down, the curses of the drivers, and the wear and tear of the cattle I leave to Mr. Reid's imagination. The eminent archaeologist, Mr. C. H. Read, who accepts Mr. Reid's conclusions,

[1] iv, 16 (30), §§ 103–4. Prof. Ridgeway (*Proc. Soc. Ant.*, 2nd ser., xx, 1904, p. 343) affirms, Prof. Rhys (*Celtic Britain*, 1904, p. 304) apparently denies that *Ictis* and *Vectis* were phonetically connected. See Addenda, p. 740.

[2] See p. 499, n. 5, *supra*.

assures us that a voyage from St. Michael's Mount to the mouth of the Loire is not to be thought of, for it would have involved a 'long and dangerous sea passage'.[1] Is he serious? This long sea passage was far shorter than the passage from the Isle of Wight: why it was more dangerous than a passage which involved navigation in the neighbourhood of the Channel Islands as well as of Ushant no seaman will be able to understand. The passage which seems so terrible to Mr. Read was made by Pytheas.[2] The passage from Italy to Sardinia was longer: several times longer was the passage from Britain to Iceland, which was made long before the invention of the compass;[3] as long or longer the passage from Scandinavia to Britain, which was made, according to Mr. Read himself,[4] in the Bronze Age. That the Veneti should have been quite willing to sail from the Isle of Wight to the Loire, but so afraid of sailing in their stout ships from Cornwall that they deliberately added more than a hundred miles to the length of their voyage, is a mystery which Mr. Read must be left to explain.

But Mr. Reid, in the conversation which passed between us, urged reasons in favour of his theory which are omitted in his paper and to which I shall endeavour to do justice. Archaeological evidence, he remarked, shows that the people of Cornwall were far more uncivilized than those of Hampshire: even supposing that St. Michael's Mount was an island, it had no real harbour; and it would have been very dangerous for mariners to attempt to get there especially in a fog or a south-westerly gale. I reply that it would also have been dangerous in such weather to attempt to fetch the coast of the Isle of Wight, as the ship would have incurred the risk of running a-tilt against the limestone causeway; that in a fog the skipper would have anchored; and that, notwithstanding the lack of a proper harbour, the ship would have lain snugly in sheltered water under the lee of St. Michael's Mount. The comparative barbarism of the people of Cornwall is irrelevant: as they wanted to sell their tin, there was no danger that they would molest their customers. Besides, Mr. Reid seems to forget that the people who produced the tin delivered it to the traders at Ictis. The traders transacted business directly with them; and, assuming that Ictis was the Isle of Wight, they were as barbarous when they had crossed the limestone causeway as they had been when they left the tin mines. Mr. Reid's argument compels him once more to throw overboard the ancient authority, who, as he insists, 'can be literally depended on'; for Diodorus distinctly states that the tin-mining inhabitants of Belerium were friendly to strangers, and *from their intercourse with foreign merchants* had become comparatively civilized.[5] This passage proves that,

[1] *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 85.

[2] See p. 221, *supra*.

[3] *Dicuili liber de mensura orbis terrae*, ed. G. Parthey, 1870, pp. 42–4 (7, 11–4). Dicuil was an Irish monk, who wrote A.D. 825.

[4] *Guide to the Ant. of the Bronze Age* (Brit. Museum), p. 146.

[5] *Φιλόξενοί τε διαφερόντως εἰσὶ καὶ διὰ τὴν τῶν ξένων ἐμπόρων ἐπιμιξίαν ἐξημερωμένοι τὰς ἀγωγάς.*

according to Diodorus, Ictis was in the territory of Belerium, and by itself demolishes Mr. Reid's theory. For how could the inhabitants have become civilized by their commercial dealings if the merchants never came near Belerium, and the only inhabitants who came in contact with them were wagoners or boatmen ?

It is clear then that the case for the Isle of Wight rests upon the geological evidence, such as it is, that at the time when Ictis was a trading station, St. Michael's Mount was ' an isolated rock rising out of a swampy wood '. Common sense and the historical evidence are all on the other side. If St. Michael's Mount had not been available, there would have been nothing to prevent the traders from shipping the tin at Falmouth or in Plymouth Sound ; and acceptance of Mr. Reid's theory involves, besides other insuperable difficulties, the assumption that the tin-merchants were ignorant of the first principles of business.

III. We now come to the question, When did the overland trade in tin between Corbilo and Massilia begin, and how long did it last ? That it existed before the time of Pytheas—that is to say, at least as early as the fourth century before Christ—is certain ;[1] for, as we have seen, Pliny and Diodorus Siculus derived their information about Ictis ultimately from him.[2] Müllenhoff,[3] indeed, contends for a still earlier date. Only on this hypothesis, he argues, can we explain the remarkable fact that the great Celtic immigration at the beginning of the fourth century B.C. not only did no harm to Massilia but actually increased its prosperity, the profits of the trade being appreciated by the Celts themselves. Still, there is no evidence that it existed (except in the form of intertribal barter) before the foundation of Massilia, or even that it had begun long before Pytheas visited Britain.

Professor Ridgeway insists that it is ' obvious that when the Belgic tribes . . . made permanent settlements on the south-east coast of Britain, the course of trade would pass regularly from Kent into Northern France, and that the old route by Armorica, Corbilo, and the Loire would fall into disuse '.[4] If anything is ' obvious ', it is that the course of trade would continue to follow the most convenient route, and that merchants would not saddle themselves with the expense of conveying tin, destined for Mediterranean markets, all the way from Cornwall to Kent. Besides, how was it to be conveyed thither ? Certainly not by land ; for Professor Ridgeway tells us himself that the barrier interposed by the great forest of Anderida would have rendered this impossible.[5] Certainly not by sea ; for, unless the merchants had taken leave of their senses, why should they have paid for the voyage from Cornwall to Kent, then for the voyage from Kent to Boulogne, and then for the long overland journey to Marseilles, when, by taking the route which led

[1] This is admitted, or rather maintained, by Prof. Ridgeway.
[2] See p. 499, nn. 2 and 5, *supra*.
[3] *Deutsche Altertumskunde*, i, 1890, p. 223.
[4] *Folk-Lore*, i, 1890, p. 105.
[5] See p. 501, *supra*.

from St. Michael's Mount to the mouth of the Loire, both the voyage and the land journey would have been considerably shortened? If Caesar does not expressly mention Corbilo, neither does he expressly mention any other commercial port; and he does imply that the Veneti had the lion's share of the carrying trade with Britain.[1] Possibly Corbilo had lost its importance by the time of Caesar; but the estuary of the Loire still formed one of the two most important harbours in the west of Gaul, and Strabo mentions it as one of the four principal Gallic ports from which ships bound for Britain set sail.[2] The argument based upon the fact that the overland journey lasted thirty days implies that the merchants would have deliberately preferred a longer to a shorter route; and as the distance from the mouth of the Loire to Massilia was about four hundred and eighty miles *in a straight line*, it does not seem incredible that the journey should have lasted thirty days. But what puzzles me most in Professor Ridgeway's argument is that, while it is partly based upon the testimony of Diodorus, it sets that testimony at defiance. The professor holds that the authority whom Diodorus followed was Posidonius. If so, Posidonius stated that in his time British tin was shipped for the Continent at Ictis. Now Professor Ridgeway identifies Ictis with the Isle of Wight. I have shown that Ictis was St. Michael's Mount. But, according to Professor Ridgeway, British tin was shipped, in the time of Posidonius, neither at the Isle of Wight, nor at St. Michael's Mount, but in Kent.[3] The train of thought which led to this conclusion is one which my poor brain is powerless to follow.[4]

Professor Haverfield[5] affirms that the Roman annexation of Gallia Narbonensis 'secured that trade route by which Diodorus Siculus tells us that British tin reached the Mediterranean, that is the route from Narbo by the "pass of Carcassonne" and Toulouse to Bordeaux'; but I cannot find any evidence that this was the route to which Diodorus referred.

Professor Rhys[6] has constructed a theory about the course of the tin trade during the maritime supremacy of the Veneti which is even more remarkable than that of Professor Ridgeway. He tells us that 'at one time they probably landed British tin at the mouth of [the Loire] . . . and they fetched some of it at any rate from the south-

[1] *B. G.*, iii, 8, § 1. Cf. Strabo, iv, 4, § 1.

[2] *Geogr.*, iv, 5, § 1.

[3] I cannot see how Mr. Reginald Smith (*Guide to the Ant. of the Early Iron Age* [Brit. Museum], p. 85) reconciles his theory, that the route in 'the opening years of the first century B.C.' passed through Kent with his previous assertion (p. 84) that 'about 90 B.C.' it left the British coast at the Isle of Wight.

[4] As Professor Ridgeway assumes that Posidonius was the authority whom Diodorus followed both in v, 22 and in v, 38, he would be compelled to maintain that in the passage which served as the basis of the former chapter Posidonius was describing only the route which the tin trade followed in the time of Pytheas, in the other that which it followed in his own time. How can the professor prove this?

[5] *Proc. Soc. Ant.*, 2nd ser., xviii, 1900, p. 119.

[6] *Celtic Britain*, 1904, pp. 47–50.

east of Britain'. In other words, the tin was conveyed at heavy cost by the Britons three hundred miles from Cornwall to the south-east of Britain, in order that the Veneti might add at least two hundred miles to the voyage which they would have undertaken if they had fetched it direct from Cornwall; and this was done although, as Professor Rhys himself assures us, there was 'communication between the Dumnonii [of Cornwall] and the nearest part of Gaul during the Venetic period'. The professor adds that 'whatever direct trade in tin there may have been between the tin districts of Britain and the Loire, it must have been utterly unknown to Caesar'. I reply that if, as Professor Rhys holds, there was trade in tin by way of South-Eastern Britain between the tin districts of Britain and the Loire, this trade also must, on Professor Rhys's theory, have been unknown to Caesar, for he mentions neither the one nor the other; but that the voyage which Crassus made to the tin-producing districts of Cornwall, and about which Caesar is equally silent, shows that Caesar was not ignorant, but merely reticent.

But Professor Ridgeway would assign a different reason for Caesar's silence. Remarking that 'when Strabo, writing as a contemporary, is describing the exports from Britain, he omits the mention of tin, whilst from the extract from Posidonius, quoted alike by him and Diodorus, it is plain that when the Stoic explorer visited North-Western Europe, the British tin trade was still of importance', the professor suggests that in the time of Caesar Britain ceased to export tin.[1] But did not Strabo write long after Caesar died? Professor Haverfield, on the other hand, has given reasons for the view that 'the early Cornish tin trade, which Posidonius and Caesar knew, died out about the beginning of our era'; and he suggests that it may have done so because the Romans had just discovered 'the real site of the Cassiterides in N. W. Spain'.[2] 'Very little,' he remarks, 'has been found west of Exeter which can be connected with the first two centuries of the Roman Empire . . . Plainly the Romans of the conquest period did not care to advance beyond Exeter . . . Yet if the tin trade had then been flourishing they would hardly have stopped. We must put the halt at Exeter beside the silence of the writers after Caesar, and suppose that for some reason the tin trade had ceased in Cornwall. Perhaps as iron took the place of bronze in many lands tin was no longer in such demand; perhaps the Spanish ore was cheaper than the Cornish; perhaps the accessible Cornish tin streams seemed exhausted. Whatever the reason, the Cornish tin trade vanished before A.D. 50. It reappears two centuries later.'[3]

Now the evidence that Professor Haverfield offers of its having *reappeared* is simply the discovery of one inscribed ingot of Cornish tin, which belonged to the fourth century; and if no inscribed ingots of an earlier date have been found, their absence hardly proves that the Romans had not worked the mines before. This Professor

[1] *Folk-Lore*, i, 1890, pp. 83–4.
[2] *Archaeol. Journal*, xlix, 1892, p. 178.
[3] *Proc. Soc. Ant.*, 2nd ser., xviii, 1900, pp. 119–20.

Haverfield admits; but, he insists, 'it does prove that we have no right to say that mining was going on.'[1] Possibly: but if so, *the absence of inscribed ingots of tin in Spain*[2] *equally proves that we have no right to say that mining was going on there.* Yet, if it was suspended in Cornwall, it must have been contemporaneously active in Spain. It is true that no Roman antiquities of earlier date than the third century have been found in Cornwall, except some Samian ware and coins of Trajan and Vespasian;[3] and it may be true that, as the professor says, these discoveries 'prove no Roman influence or occupation':[4] but, on the other hand, Cornwall has very few Roman antiquities even of the third and fourth centuries,[5] and no Roman or Romanized towns or villas.[6] Is it not then possible that, as Professor Gowland suggests, the mines were worked throughout the whole period of the Roman occupation of Britain, but not under Roman control?[7] He points out that 'the stamps had been impressed [upon the solitary ingot] when the metal was cold, and hence not necessarily at the mine, but very probably by a Roman trader or officer at the coast'.[8] Professor Haverfield indeed states that the ingot was found not more than a mile and a half from 'an old working', which has yielded Roman coins:[9] but Professor Gowland supports his own view by the argument that 'at the Roman lead mines in Britain the inscriptions were always cast on the ingots of lead when they were made, and at the copper mines were stamped on the cakes of copper while they were red hot'. 'The real site of the Cassiterides' was not, as Professor Haverfield thinks, 'in N. W. Spain,' but in the British Isles. 'The silence of the writers after Caesar' in regard to the British trade in tin, on which he lays stress, really resolves itself into the silence of Strabo; for although the professor is quite right in saying that 'later authors [namely, Diodorus, Strabo, and Pliny] merely include it in quotations from earlier literature', those who are familiar with their writings will admit that there was no reason why any of them, except Strabo, should have expressly added to those quotations the information that the British tin trade continued in their own time. We should certainly have expected that

[1] *Proc. Soc. Ant.*, 2nd ser., xviii, 1900, p. 122.

[2] Numerous Roman inscribed objects of lead have been discovered in Spain (*Corpus Inscr. Lat.*, ii, 4964, and Suppl., 6243, 6247–8); but so far as I can ascertain, none of tin.

[3] *Archaeol. Journal*, xlvii, 1890, p. 232.

[4] *Proc. Soc. Ant.*, 2nd ser., xviii, 1900, pp. 119–20.

[5] *Archaeol. Journal*, xlvii, 1890, pp. 230–3; xlix, 1892, p. 178; *Corpus Inscr. Lat.*, vii, 13.

[6] *Proc. Soc. Ant.*, 2nd ser., xviii, 1900, p. 120.

[7] *Ib.*, p. 122.

[8] *Ib.*, p. 118.

[9] *Ib.* What puzzles me is how Professor Haverfield reconciles his view that in the third century 'Cornish tin began to take its place as an article of commerce in Roman Britain' (*Mélanges Boissier*, 1903, p. 251) with his own suggestion (*ib.*, p. 250) that 'either the tin ores had never been so rich as fancy painted, or the accessible deposits had been worked out [two centuries earlier], or . . . Spanish competition had ousted British tin'. Evidently the accessible deposits had not been worked out; and British tin must have had superabundant vitality if it reasserted itself two centuries after it had been ousted.

Strabo would have included tin in his list of British exports if it had been exported in his time ; and I will not attempt to explain away his silence : but can it outweigh the extreme improbability that for two centuries the civilized world should have been entirely cut off from one of the two sources from which its supply of tin had previously been derived ? And when Professor Haverfield suggests that ' as iron took the place of bronze in many lands, tin was no longer in such demand ', does he not momentarily forget that not only in the lands round the Mediterranean but also in those of Northern and Western Europe iron had taken the place of bronze for many purposes several centuries before the Christian era, and that, on the other hand, those implements and ornaments which were still made wholly or in part of bronze were probably in greater demand than before ?

IV. We have now to deal with the Phoenicians. Sir George Cornewall Lewis [1] and various other writers have endeavoured to prove that the Phoenicians (including the Carthaginians) never traded directly with Britain for tin ; and in 1896 Dr. Arthur Evans remarked that 'the days are gone past when it could be seriously maintained that the Phoenician merchant landed on the coast of Cornwall '.[2]

Now Dr. Evans's distinguished father, who holds that the Cassiterides ' are rightly identified with Britain ', observes that ' the traces of Phoenician influence in this country are . . . at present imperceptible. But,' he continues, ' it may well be that their system of commerce or barter was such as intentionally left the barbarian tribes with whom they traded in much the same stage of civilization as that in which they found them, always assuming that they dealt directly with Britain and not through the intervention of Gaulish merchants.' [3]

Some merchants certainly landed, if not on the coast of Cornwall at all events on that of Ictis : is there any reason in the nature of things why Phoenician merchants should not have done so ? To the old-fashioned view there are only two objections worth considering, namely, first, that ' the tin trade was carried on overland through Gaul ',[4] and, secondly, that the tin which was shipped to Gades may have come not from Britain but from the mines of North-Western Spain. But, as we have seen, there is no evidence that the overland trade had begun before 600 B.C.,—the approximate date of the foundation of Massilia; nor is there any evidence that the Phoenicians took part in it. From Gades to Cornwall the voyage, as George Smith observes, was shorter than the voyages ' from Tyre to Malta,

[1] *Hist. Survey of the Astronomy of the Ancients*, pp. 451–5.

[2] *Report of . . . the Brit. Association*, 1896, p. 910.

[3] *Anc. Bronze Implements*, p. 419. Cf. F. J. Haverfield in *Mélanges Boissier*, p. 249, n. 1. Mr. Reginald Smith (*Guide to the Ant. of the Early Iron Age* [Brit. Museum], p. 137) suggests, with the approval of Mr. C. H. Read, that a bronze statuette, found near Aust-on-Severn, may have been deposited 'by Phoenician traders to our shores '. Cf. *Proc. Soc. Ant.*, 2nd ser., xx, 1904–5, p. 192.

[4] See G. Smith, *The Cassiterides*, p. 54.

Carthage, or Sicily, which they were performing continuously '.[1] If Desjardins[2] is right in affirming that ' the name *Corbilo* unquestionably looks Phoenician ', and that a Phoenician inscription has been found near Guérande, it may be inferred that the carrying trade between Britain and Corbilo was at one time either wholly or partly in Phoenician or Carthaginian hands. That tin was obtained in ancient times from the mines of North-Western Spain must be admitted : not only is the fact attested by the statements of Strabo and Pliny,[3] but it has been proved by the researches of Mr. W. C. Borlase.[4] But there is some evidence that tin also came from Cornwall to Gades. Festus Avienus[5] tells us, ultimately, it may be assumed, on the authority of the Carthaginian traveller, Himilco, that both the Carthaginians and the people of Gades used to sail to the British seas.[6] Sir George Cornewall Lewis,[7] indeed, argues that ' if the date of the voyages of Hanno and Himilco is correctly fixed, it follows that at a period subsequent to the expedition of Xerxes, the Carthaginians . . . had not carried their navigation far along the coasts of the Atlantic ; and that they sent out two voyages of discovery—one to the south, the other to the north—at the public expense '. All that we know about the date of Himilco's voyage is that it was not later than the fifth, probably in the sixth century B.C.,[8] and, according to Pliny,[9] its object was ' to explore the outer parts of Europe'. Anyhow the evidence remains that after Himilco's time, if not before, the Carthaginians traded by sea with Britain.[10]

[1] G. Smith, *The Cassiterides*, pp. 47–9. See also pp. 56–7, and E. H. Bunbury, *Hist. of Anc. Geogr.*, i, 12.

[2] *Géogr. de la Gaule rom.*, i, 329.

[3] See pp. 484–6, *supra*.

[4] *Tin Mining in Spain*, p. 28.

[5] *Ora Maritima*, 113–6.—

Tartesiisque in terminos Oestrymnidum
Negotiandi mos erat : Carthaginis
Etiam coloni, et vulgus, inter Herculis
Agitans columnas, haec adibant aequora.

See also H. F. Tozer, *Hist. of Anc. Geogr.*, pp. 110–1.

[6] Prof. Haverfield (*Eng. Hist. Rev.*, xix, 1904, p. 746) thinks that ' the " Periplus " of Avienus cannot safely be attributed to Himilco ' ; but M. Camille Jullian (*Ann. de la Faculté des lettres de Bordeaux,—Bull. hisp.*, v, 1903, p. 109 ; *Journal des Savants*, nouv. sér., No. 2, 1905, pp. 95–8) supports my view. I am not sure, however, that Prof. Haverfield means to express a doubt whether the Periplus was *ultimately* based upon Himilco's report. Cf. *Rhein. Mus.*, l, 1895, p. 336.

[7] *Hist. Survey of the Astronomy of the Ancients*, p. 455.

[8] E. Hübner, *Monumenta linguae Ibericae*, 1893, p. xxvi ; H. d'Arbois de Jubainville, *Principaux auteurs de l'ant. à consulter sur l'hist. des Celtes*, &c., p. 42.

[9] *Nat. Hist.*, ii, 67, § 169.—Hanno Carthaginis potentia florente circumvectus a Gadibus ad finem Arabiae navigationem eam prodidit scripto, sicut ad extera Europae noscenda eodem tempore Himilco. I find that Müllenhoff (*Deutsche Altertumskunde*, i, 1890, pp. 93–5) has anticipated a remark which I was about to make, namely, that the object of Himilco's voyage was undoubtedly to open up new markets for trade, and not merely to explore. See also Lord Avebury's *Prehist. Times*, 1900, pp. 57–67, though I think that his argument might have been more valuable if he had taken note of Mr. Borlase's *Tin Mining in Spain*.

[10] Mr. W. C. Borlase (*Tin Mining in Spain*, pp. 24–6), remarking that ' there is an extremely rare form of [the palstave], namely with two loops, and that

Dr. Arthur Evans, I know, warns us that 'a truer view of primitive trade as passing on by inter-tribal barter has superseded the idea of a direct commerce between remote localities'.[1] But the testimony of Diodorus, that is to say of Pytheas, proves that traders purchased tin off the Cornish coast from the natives who had prepared it for market, carried it across the Channel, and unloaded it on the coast of Gaul, whence it was conveyed overland to the mouth of the Rhône. If this was not 'direct commerce', what was? That there was 'inter-tribal barter' in ancient times, no well-informed person would deny; but that there was also 'direct commerce between remote localities' is as well attested as any fact of ancient history can be.

Mr. C. T. Newton indeed argues that 'if the Phoenicians frequented any portion of the British coast, it is probable that they would have given names to the more important harbours and promontories, as they did in Africa and Spain'.[2] But is it not also probable that they found it sufficient to hold, or even to occupy temporarily, as occasion required, one or more of the Scilly Islands, or perhaps St. Michael's Mount, and that they may have given names to these places, although the names have not survived.[3] Their settlements in Africa and Spain were not temporary but permanent.

I freely admit that the testimony of Festus Avienus is not conclusive; but I see no reason for rejecting the statement of Strabo that the Phoenicians traded directly for tin with the Cassiterides—that is to say, the British Isles—and that they originally monopolized the trade.

M. Salomon Reinach,[4] who supports the view that the Phoenicians traded directly with Cornwall, insists, referring to a well-known passage in Thucydides,[5] that the overland route must have been earlier than the maritime. 'Corinth,' says Thucydides, 'being

has been found exclusively in Cornwall and Devon (in the mining districts especially), in Ireland, and in the western and north-western portion of the Iberian Peninsula', and that 'bronze celts of this class belong . . . to . . . 1250 to 1050 B.C.', concludes that 'at that period then—the very period to which has been assigned the foundation of Gades—Cornwall and the west coast of Spain were already in communication'. Perhaps; but not necessarily Cornwall and Gades. Similar celts have also been found in France (J. Evans, *Anc. Bronze Implements*, pp. 96–7).

Müllenhoff also argues (*Deutsche Altertumskunde*, i, 1890, pp. 5–8) that the passage in the *Odyssey* (x, 81–6) which describes the country of the Laestrygones, where the days in summer were very long and the nights very short, would seem to be based upon stories told by Phoenician mariners; but, as I have already remarked (p. 218), if Homer's lines were founded upon fact, it is more probable that the stories came to him from Scandinavia.

[1] *Report of . . . the Brit. Association*, 1896, p. 910.

[2] *The Builder*, Aug. 26, 1865, p. 604.

[3] C. F. Wiberg (*Der Einfluss der klassischen Völker*, &c., 1867, p. 13) thinks that 'the promontory of Herakles', or Hartland Point (Ptolemy, *Geogr.*, ii, 3, § 2), may owe its name to the Phoenician worship of Hercules; but I do not know that any one except Prof. Boyd Dawkins (*Early Man in Britain*, p. 461) attaches any importance to this suggestion.

[4] *L'Anthr.*, x, 1899, p. 401.

[5] i, 13, § 5.—οἰκιοῦντες γὰρ τὴν πόλιν οἱ Κορίνθιοι ἐπὶ τοῦ ἰσθμοῦ ἀεί δή ποτε ἐμπόριον εἶχον, τῶν Ἑλλήνων τὸ πάλαι κατὰ γῆν τὰ πλείω ἢ κατὰ θάλασσαν, τῶν τε ἐντὸς Πελοποννήσου καὶ τῶν ἔξω, διὰ τῆς ἐκείνων παρ' ἀλλήλους ἐπιμισγόντων, &c.

seated on an isthmus, was naturally from the first a centre of commerce; for the Hellenes within and without the Peloponnese, in the old days when they communicated chiefly by land, had to pass through her territory in order to reach one another.'[1] M. Reinach argues that 'nothing could have suggested to the Phoenicians the idea of going with their ships in search of tin if they had not already known the existence not only of the metal but also of the distant country which produced it . . . the Phoenicians of Spain no more discovered the Cassiterides and tin than the Portuguese discovered India and spices'. This may be freely admitted. But the Phoenicians may well have acquired the knowledge upon which they acted long before the direct overland trade which Diodorus describes began. Tin was probably conveyed in very early times from Cornwall to Gaul for the use of tribes who inhabited that country before the immigration of the Celtic-speaking invaders; and, since Gaul was in communication with Britain from the beginning of the Bronze Age,[2] the knowledge that tin was to be obtained in Britain might have reached Phoenician ears even before Gades was founded.

But the most striking contribution which M. Reinach has made to the literature of this subject is the suggestion that the traders who first sailed from the Mediterranean into the English Channel were not Phoenicians but Phrygians. Speaking of the well-known passage, which I have already quoted, in which Pliny says that Midacritus was the first who imported tin from 'the tin island',[3] he argues that the generally accepted identification of Midacritus with the Phoenician Melcarth is erroneous. He points out that in Pliny's list of discoverers all except the most famous names are accompanied by a complementary designation, for example (Toxius), *Caeli filius*[4]. Therefore, even if, as has been supposed, what Pliny wrote was not *Midacritus* but *Melicertus* (Melcarth), that unfamiliar name would have been followed by some explanatory addition. M. Reinach then quotes two passages from Hyginus[5] and Cassiodorus[6] respectively. In the former we read that 'King Midas, the Phrygian, son of Cybele, was the first to discover lead and tin' (*Midas rex Cybeles filius Phryx plumbum album et nigrum primus invenit*); in the latter, that 'Midas, the ruler of Phrygia, discovered tin' ([Aes enim Ionos Thessaliae rex], *plumbum Midas regnator Phrygiae reppererunt*). It is clear then, says M. Reinach, that, as the Jesuit scholar, Hardouin, perceived more than two centuries ago, for *Midacritus* in the MSS. of Pliny we ought to read *Midas Phryx*. He adds that from a fragment of the Seventh Book of Diodorus, preserved in the Chronicle of Eusebius, we learn that the maritime supremacy of the Phrygians began about 903 B.C., and that of the Phoenicians in 824.[7]

[1] B. Jowett, *Thucydides translated into English*, i, 1881, p. 10.
[2] See p. 126, *supra*.
[3] See p. 485, n. 5, *supra*.
[4] *Nat. Hist.*, vii, 56 (57), § 194.
[5] *Fabulae*, ed. M. Schmidt, 1872, CCLXXIV (p. 149).
[6] *Variarum* iii, 51 (J. P. Migne, *Patrologiae cursus completus*, lxix, 1848, col. 594).
[7] See C. Müller's edition of Diodorus, i, 1842, p. 316 (Reliquiae libri vii, 13).

DENE-HOLES

Of the various theories which have been published as to the object of dene-holes three only are worth considering, namely, that they were granaries; that they were refuges; and that they were sunk in order to obtain chalk.

Subterranean granaries have of course been used in many countries;[1] but it is said that no grain has ever been found in any dene-hole,[2] whereas grain has been found in shallow pits and on numerous other prehistoric sites in Britain.[3] On the other hand, a thorough exploration of the famous group of dene-holes in Hangman's Wood, Essex, revealed fragments of two millstones.[4] The Reverend E. H. Goddard remarks that 'very similar places' in Brittany were used by 'the peasant armies during the war in La Vendée' as refuges and lairs, and argues that dene-holes served a similar purpose.[5] Perhaps, though it would have gone hard with the fugitives if their lairs had been discovered; but, seeing that strongholds were available, it is difficult to admit that they were dug with that object. The theory that they were shafts sunk for the extraction of chalk rests mainly upon the evidence of Pliny, who states that chalk was obtained in Britain for manure 'by means of pits sunk like wells with narrow mouths to the depth commonly of one hundred feet, where they branch out like the veins of mines' ([creta] *petitur ex alto, in centenos actis plerumque puteis, ore angustis, intus ut in metallis spatiante vena*[6]). Messrs. T. V. Holmes and W. Cole, who superintended the exploration of the dene-holes in Hangman's Wood, argue that 'the above account could not have been given to Pliny by any man who had ever descended into one of our [Essex] . . . dene-holes, which are entered by . . . narrow shafts, but whose lofty symmetrical chambers cannot be described as " branching out like the veins of mines ".'[7] I think, on the contrary, that, allowing for the natural inaccuracy of a writer who gave his own version of information supplied by one who had perhaps himself not descended into a dene-hole, Pliny's description was remarkably correct: the chambers which open out at the bottom of the shafts in Hangman's Wood are arranged in the shape of a star-fish; the only material error with which Pliny can be charged is that he compared them to the veins of mines; and that he was alluding to them I have no doubt. Messrs. Holmes and Cole

The commencement of the maritime supremacy of the Phoenicians is here dated 58 years after the commencement of that of the Phrygians, and 279 years after the Trojan War.

[1] *Archaeol. Journal*, xxxix, 1882, p. 18; *Essex Naturalist*, i, 1887, pp. 266–76.
[2] *Trans. Essex Archaeol. Soc.*, N.S., vii, 1900, p. 252.
[3] See pp. 151, n. 4, 253, 256, *supra*.
[4] *Essex Naturalist*, i, 252.
[5] *Trans. Essex Archaeol. Soc.*, N.S., vii, 253–4.
[6] *Nat. Hist.*, xvii, 8 (4), § 45. Cf. *Essex Naturalist*, i, 249.
[7] *Ib.*, pp. 249–50.

are, however, on firm ground when they point out that his informant may have wrongly assumed that the shafts were sunk in order to obtain chalk because the chalk that was extracted from them was utilized. 'And,' they continue, 'a foreigner accidentally discovering secret pits —and our surface trenches showed our dene-holes to have been secret excavations—would almost necessarily be deceived as to their use by natives.' But is it not possible that Pliny's informant may have been a Briton? And, assuming that he was deceived as to the purpose of the dene-holes, why was he allowed to learn the existence and arrangement of the chambers, and, approximately, the depth of the shaft?

Nevertheless, Messrs. Holmes and Cole are undoubtedly right in the main. It has been argued that dene-holes are situated in places which must always have been uncultivated, whereas the tracts in which chalk lay near the surface may have been already occupied; that chalk has been obtained in Wiltshire in modern times by mining although it was to be had near the surface; and that the labour of sinking the shafts may have been compensated by saving the cost of transporting chalk from distant parts, where it was the surface rock.[1] But, as Messrs. Holmes and Cole observe, 'there is plenty of bare chalk within a mile' of Hangman's Wood; and, as they pertinently ask, if the dene-holes were sunk for chalk, why was their position so carefully kept secret?[2] Again, Mr. Spurrell, who admits that where chalk lay very deep shafts may have been sunk merely in order to obtain it, remarks that 'it is evident that where the land is white with chalk the pits of great depth so often found there could not have been dug for manure, and the natives of Kent in such situations scout the idea as absurd'.[3] Messrs. T. E. and R. H. Forster contend that the elaborate design of the chambers in Hangman's Wood is 'in reality a strong confirmation' of the truth of 'the chalk-quarry theory'; for 'the star-fish-shaped pit . . . enables the miner to win more chalk at one sinking; and if no examples of it were known, it would be necessary to postulate its existence in order to supply the missing link between the primitive bell-pit and the pillared and galleried mine of the kind seen at Chislehurst'.[4] But is the 'bell-pit' primitive, and is there a link, missing or otherwise? Anyhow it is incredible that the people of Essex, if they had undertaken the prodigious labour of sinking 70 shafts simply in order to obtain better chalk than what they could have found hard by at the surface, would have contented themselves, after boring through 60 feet of sand and gravel, with 'the very uppermost [and therefore worst] chalk'.[5] As Mr. Holmes remarks,[6] 'it must be obvious that the course which would commend itself to all seekers after superior chalk would be to begin operations where chalk is at the surface,

[1] *Journ. Brit. Archaeol. Association*, N.S., x, 1904, pp. 98–101.
[2] *Essex Naturalist*, i, 250.
[3] *Archaeol. Journal*, xxxix, 1882, p. 19.
[4] *Journ. Brit. Archaeol. Association*, N.S., x, 1904, p. 98. Cf. *Times*, Sept. 30, 1905, p. 3, cols. 3–4.
[5] *Essex Naturalist*, i, 250–1.
[6] *Geol. Mag.*, 1898, p. 453.

make a shaft 10 to 20 feet deep, and procure chalk lying at that depth'; and, while he freely admits that 'a farmer might naturally prefer to get chalk at a depth of 60 to 80 feet on his own land rather than . . . from some one else's pit a mile or two away', he emphasizes the absurdity of supposing that 'any people . . . concentrated their pits where they got the least return for their labour, and where there was no counterbalancing advantage . . . as they must have done at Hangman's Wood and Bexley on the Chalk-pit hypothesis'.[1]

Charred wood, bones of animals, and large quantities of coarse pottery have been found in a dene-hole near Dunstable,[2] which is sufficient evidence that some dene-holes were occasionally inhabited.

I conclude that dene-holes were intended to serve as granaries; that they may have been used occasionally as places of concealment; and that the chalk which was taken out of them was used, if it was wanted, for manure. It is significant that their name means '*Dane*-holes', that is, hiding-places from the Danes.[3]

The 'bell-pits' which have been already mentioned, and which are sometimes confounded with dene-holes, were undoubtedly made for the sake of the chalk; and, unlike dene-holes, they were made broad in order that a large amount of material might be taken out of them at each haul.[4]

Some of the Kentish dene-holes, if Mr. Goddard is rightly informed, contained bronze implements;[5] and those of Essex are almost certainly post-neolithic.[6] Some bell-pits are ancient, but I doubt whether it could be proved that any were pre-Roman: Pitt-Rivers[7] indeed believed that it was from the Romans that the Britons learned to use chalk as top-dressing.

THE COAST BETWEEN CALAIS AND THE SOMME IN THE TIME OF CAESAR

The question of the period during which the gulf of St. Omer existed has given rise to much discussion. According to Reclus,[8] Desjardins,[9] and many other writers,[10] even in the time of Caesar this so-called gulf, which was really a shallow salt-water 'mere', covered the lowlands north-east of the hills of Artois between Sangatte and Dunkirk, and extended inland to within a short distance of St. Omer.

[1] *Geol. Mag.*, 1898, p. 453.
[2] Worthington G. Smith, *Man, the Primeval Savage*, pp. 326–7.
[3] J. A. H. Murray, *New Eng. Dict.*, iii, 192–3.
[4] *Geol. Mag.*, 1898, p. 457.
[5] *Trans. Essex Archaeol. Association*, N.S., vii, 1900, p. 253.
[6] *Vict. Hist. of . . . Essex*, i, 310–1.
[7] *Excavations in Cranborne Chase*, i, 4.
[8] A. Joanne, *Dict. géogr. . . . de la France*, 1869, p. xli.
[9] *Géogr. de la Gaule rom.*, i, 349 and n. 3, 391.
[10] In particular C. de Laroière in *Annales du comité flamand de France*, x, 1868–9 (1870), pp. 249–322.

No evidence, however, has been adduced to show that it existed at that time;[1] and it has been proved by M. J. Gosselet that it did not exist before the latter part of the third century of our era, for Gallo-Roman remains, including 2,354 coins, some of which belong to the time of Postumus, have been found in the area. As M. Gosselet says,[2] the *Sinus Itius* is a mere invention of writers of the seventeenth century.

The ancient topography of Wissant, of the estuary of the Liane, and of the headlands of Blancnez, Grisnez, and Alprech, is discussed in the article on the Portus Itius.[3]

The inland extension of the bay formed by the estuary of the Canche has steadily diminished since the time of Caesar; and whereas, during the last century at all events, the headland on its southern side has gained considerably on the sea, the 'Pointe de Lornel' on the north and the neighbouring sand-dunes have suffered continual erosion.[4]

The country which lies between the hills of Artois and the sea, from the mouth of the Canche to the mouth of the Somme, is, as Reclus[5] remarks, of recent formation; and, as late as the ninth century, the environs of the town of Rue, which is now about six miles from the sea, were covered by a vast shallow lake, 20,000 hectares, or about 78 square miles, in extent.

THE CONFIGURATION OF THE COAST OF KENT IN THE TIME OF CAESAR

This volume is not a treatise upon the physical geography of Ancient Britain; and I am only concerned with geographical questions in so far as they are essential to a right understanding of the history. It is impossible to understand the narrative of Caesar's invasions of Britain without considering how far the physical geography of that part of the island which was the theatre of his operations differed from what it is now.

[1] See *Bull. de l'Acad. Roy. . . . de Belgique*, 3e sér., viii, 1884, pp. 681–9. Desjardins (*Géogr. de la Gaule rom.*, i, 391) admits that it is only 'probable' that it existed at that time. Cf. R. Blanchard, *La Flamande*, 1906, pp. 134–46.

[2] *Rev. sc.*, 2e sér., xv, 1878–9 (1879), pp. 90–3. According to M. V. J. Vaillant (*Classis Britannica*, 1888, pp. 66–7), an inscription (*SALINATORES CIVITATIS MORINORVM*), 'cité par I. Gruter, nous rapporte à une époque où la mer pénétrait librement jusqu'au delà de Saint-Omer et où les marais salants étaient exploités sur les rives de ce large golfe par les Morins et les Ménapiens.' M. Vaillant gives neither the reference nor the date of the inscription: it was found at Ariminum in Cisalpine Gaul, and makes mention of the emperor Vespasian; and it is reproduced in Gruter's *Inscr. ant. totius orbis Romani*, ii, 1707, p. MXCVI, 4. Needless to say, it does not prove that the 'gulf' existed in Vespasian's time, but only that there were salt-works in the territory of the Morini.

[3] See pp. 565–7, 572, 586–7, *infra*.

[4] *Boulogne-sur-mer et la région boulonnaise*, i, 359–61.

[5] A. Joanne, *Dict. géog. . . . de la France*, p. xlii.

I. BETWEEN RAMSGATE AND SANDOWN CASTLE

Thanet, as everybody knows, was an island in Caesar's time ; and Bede [1] says that it was separated from the mainland by an estuary three furlongs broad : but the late George Dowker [2] concluded from ' an attentive examination of the estuary ' that it was ' much shallower and narrower than is generally supposed '.

John Lewis,[3] a well-known antiquary of the eighteenth century, and William Boys,[4] the historian of Sandwich, maintained that an estuary, in which was included the harbour of Richborough, known to the Romans as Portus Ritupis, had extended from the cliffs of Ramsgate southward to Walmer, covering the sites of Stonar and Sandwich and indeed the whole of the low ground between Sandwich and Deal, and washing the shore of an island on which stood Richborough Castle. A recent writer, Mr. H. Sharpe,[5] who endorses this opinion, argues that the Roman road from Canterbury to Richborough harbour (*ad portum Ritupis* [6]) terminated at Each End. The road ' cannot ', he insists, ' have run to Sandwich in Roman times. Montagu Burrows . . . *Cinque Ports*, 1888, p. 30,[7] says—" Sandwich and Stonar are wholly English. No Roman remains have been found at either " . . . there is good reason to suppose that the land upon which it [Sandwich] stands and the land over which the Sandwich end of the road runs were not formed when the Romans were here.' [8] And again, ' There is another reason for supposing that Each End was . . . the place where the boats left the mainland for the island [of Richborough]. [The road running northward from Dover] is marked on the Ordnance map [9] as a Roman road, and if complete would run to Each End, not to Richborough Castle or to Sandwich . . . the last mile from [Woodnesborough] to Each End, is missing.' [10]

Now, in regard to Stonar, Professor Burrows, as we shall presently see, is mistaken; and, granting that the Roman road from Dover would, if complete, run to Each End, how can Mr. Sharpe prove that it did not run further ? The late George Dowker stated, in a paper which was published after his death, that he had himself ' traced the Roman road to Woodnesborough, and thence by Each End to near the Richborough Island ' ; [11] and the views of Lewis and Boys, which Mr. Sharpe endorses, as to the wide extent of the estuary at the time of the Roman conquest of Britain have been stultified by discoveries to which Mr. Sharpe does not allude. Roach

[1] *Hist. eccl.*, i, 25. Cf. Solinus, ed. Th. Mommsen, p. 114.
[2] *Archaeol. Cant.*, xii, 1878, p. 3. See also *Twenty-third Report East Kent Nat. Hist. Soc.*, 1881, p. 48.
[3] *Ant. of Richborough*, &c., 1774, pp. 137–9 ; *Archaeologia*, i, 1770, pp. 79–83.
[4] *Hist. of Sandwich*, 1792, p. 865.
[5] *Archaeol. Journal*, liii, 1896, p. 207.
[6] *Itin. Ant.*, ed. P. Wesseling, 1735, p. 472.
[7] The reference is incorrect. For ' 30 ' read ' 230 '.
[8] *Archaeol. Journal*, liii, 1896, p. 207.
[9] Sheet 290.
[10] *Archaeol. Journal*, liii, 1896, pp. 212–3.
[11] *Archaeol. Cant.*, xxiv, 1900, p. 110.

Smith affirms that 'Roman remains, indicative of habitations, have been discovered in the sand-hills considerably to the north of Sandown Castle', and that 'coins have been found at Stonar, opposite to Richborough'; and from these facts he infers that 'the recession of the sea from the low land between Thanet and Walmer probably commenced at a period much earlier than has been commonly supposed'.[1]

That the hill on which Richborough Castle stood was nearly if not quite insulated is generally admitted;[2] but Mr. George E. Fox remarks that it 'was probably not washed by the open sea, though a broad channel may have flowed close beside it, forming one of the southern mouths of the strait, while a narrow strip of salt-marsh and sand-bank lay between it and the open sea'. It would be more correct to say that the island, on its eastern side, was separated by a channel from Stonar Beach, the southern extremity of which lay east by north of the site of Sandwich: the sand-hills were on the south-eastern side of this beach, from which they were divided by a narrow channel. Mr. Fox goes on to say that 'a large extent of what is now marshland, lying to the west of the hill, may then have . . . formed the haven,[3] making of the camp hill an island'. He argues, however, that, on the eastern side, the channel 'could not have hugged the hill very closely, as at no great distance to the south of the station on this same side, and in the low ground presumably near the shore, fragments of a Roman house were discovered in 1846'.[4]

In the year 1876 Dowker affirmed that 'the low shore and sand hills' which now extend from the Deal beach to the latitude of Sandwich 'extended [in the time of Caesar] much less than at present';[5] and in a map which accompanied his paper[6] he contrasted the low-water line between Walmer and Sandwich, as he believed it to have existed in 55 B.C., with the low-water line as it existed at the time when he wrote. In the latitude of Sandwich the modern low-water line is traced on this map a mile and a half east of the hypothetical ancient line, which distance gradually diminishes to three-quarters of a mile in the latitude of Worth and about one furlong in the latitude

[1] *Ant. of Richborough, Reculver and Lymne*, 1850, pp. 53–4. See also *Archaeol. Cant.*, xiv, 1882, pp. 368–9; xxiv, 1900, p. 108; and *Archaeologia*, li, 1888, p. 465. Beale Poste (*Britannia antiqua*, 1857, p. 282) states that in one of the sand-hills, half a mile north of Sandown Castle, a large number of coins of Victorinus, Probus, Tetricus, 'and others of the lower empire' were found in 1839.

[2] See *Archaeol. Cant.*, viii, 1872, pp. 13–4. Boys, quoted by Roach Smith (*Ant. of Richborough*, &c., p. 53) remarks that, 'in digging to lay the foundation of Richborough sluice, the workmen, after penetrating through what was once the bed of the river that runs close by . . . came to a seashore that had been suddenly covered with silt.'

[3] 'Just north of the Isle of Richborough', says Dowker (*Journ. Brit. Archaeol. Association*, xl, 1884, p. 272), 'is a large artificial excavation in the hill. I gave a description of this when I wrote the account of the . . . excavation at the Castrum; and I drew attention to its being a Roman harbour. It is just opposite a farm that goes by the name of "Fleet".'

[4] *Archaeol. Journal*, liii, 1896, p. 356.

[5] *Ib.*, xxxiii, 1876, p. 71. [6] *Ib.*, facing page 64.

of Deal. I find a difficulty in reconciling this map with Dowker's own statement that 'Roman pottery, coins and traces of the Roman occupation have been found in the sand-hills—and indeed below the sand-hills considerably northward of Deal, beyond Sandown Castle';[1] and from the fact which this statement records it follows that, in the time of the Roman occupation of Britain, the shore-line at the place where the discoveries in question were made cannot have been widely different from what it is now.

II. BETWEEN SANDOWN CASTLE AND WALMER CASTLE

When we endeavour to trace the shore-line, as it existed in Caesar's time, opposite Deal and Walmer, we find that the writers who have dealt with the question differ widely among themselves; while Dowker again shows himself a most troublesome witness. Unfortunately this meritorious geologist, who laboured hard to elucidate the geographical questions connected with the ancient history of East Kent, was a bad writer, and sometimes failed to make his meaning clear.

Major Rennell, who was in his day 'the acknowledged head of British geographers',[2] believed that Caesar landed at Deal. 'Of course,' he says, 'the margin of the ancient beach, on which Caesar landed, must now be very far within land, as well as very considerably raised.'[3] The words 'of course' prepare us for the discovery that Rennell quotes no authority and gives no reasons.

Professor Montagu Burrows,[4] also without giving either authority or reason, tells us that Deal 'probably had once a haven, which was choked up in very early times'. But choked up it was not unless it existed; and observe that its existence is only 'probable'. As a matter of fact, the so-called probability is unsupported by any evidence.[5] The professor goes on to say that 'the old town [of Deal] was already separated from the sea by a considerable interval when Henry [the Eighth] built the three castles of Deal, Sandown, and Walmer for the protection of the coast, which had now become a continuous stretch of steep shingly beach'. Now if, in the time of Henry the Eighth, 'the old town was *already* separated from the sea by a considerable interval,' the inference is that it had once been quite close to the sea; and of this there is no evidence. Was the professor thinking of Leland,[6] who describes 'Deale' as 'half a Myle fro the

[1] *Archaeol. Cant.*, xxiv, 1900, p. 108.
[2] *Dict. Nat. Biogr.*, xlviii, 15.
[3] *Archaeologia*, xxi, 1827, p. 505.
[4] *The Cinque Ports*, 1888, p. 229.
[5] I am glad to find that this remark has been anticipated by Mr. C. R. S. Elvin (*Records of Walmer*, 1890, p. 30).
[6] *Itinerary*, 2nd ed., vii, 1744, fol. 127 (p. 116). Professor Burrows may perhaps have followed Hasted, who says (*Hist. of Kent*, iv, 1799, p. 163) that '*Upper Deal* was composed of the habitations of a few poor fishermen only, though at a less distance from the sea than at present, owing to the great increase

Shore of the Se, a Fisshcher Village iii. Myles or more above Sandwic' ? If so, why should he assume that because Deal in the time of Leland, that is to say, of Henry the Eighth, was half a mile from the sea, it had once been on the sea ? The only conceivable reply to this question would be that as Upper Deal is now more than half a mile from the sea,[1] and as, according to Leland, it was only half a mile from the sea in the time of Henry, it may once have been actually on the sea-shore. But Deal Castle was built by Henry ; and the sea was therefore at least as far from Upper Deal in his time as it is now. The truth is that Leland's ' Myles ' were sometimes very long : he tells us that Sandwich was ' iii. Myles ' from Deal, and it is really six.

Dowker, in the paper which he published in 1876,[2] maintained that ' Deal probably did not exist in Roman times ', and that, when Caesar landed in Britain, ' the coast was cut back behind Deal ' : [3] that is to say, he virtually committed himself to agreement with the view, already stated, of Major Rennell. In the same paper he affirmed that ' the present town of Deal is situated on a comparatively recent beach ', and went on to say, in proof of his assertion, ' I have evidence of the beach at the back of Deal containing mediaeval remains.' [4] What the evidence was, he did not say ; and what he meant by ' the beach at the back of Deal ', I do not know. In 1887 another paper [5] was published, containing a report of his views. Herein I find that there is ' no evidence ' of ' a shore-line cutting far back beyond the Deal beach '. No evidence in 1887, though in 1876 the evidence was irrefragable.[6]

The opinion of Stukeley,[7] who believed that Caesar had landed between Walmer Castle and Deal, was diametrically opposed to

of beach thrown on this shore afterwards ' ; and in note *e* he observes that ' Leland . . . seems to confirm this '. Leland, as I show in the text, does no such thing. Hasted goes on to say that ' Even so late as the year 1624, a house . . . on the *west* side of the *Lower Street* (the farthest at this time from the sea shore) is described in a deed of that date to abut *ad le sea bank versus orientem* '. Very likely : but the fact does not prove that the west side of Lower Street was an inch nearer the sea in 1624 than it is now ; for the breadth of '*le sea bank*' is not stated. Anyhow Deal Castle has not moved since 1624 : therefore, if Hasted is right, the sea must then have made a sudden bend landward immediately north of Deal Castle, and formed a bay ; which is absurd. The west side of Lower Street is now about 550 feet from the high-water mark of ordinary tides (*Six-Inch Ordnance Survey*, Sheets 58 and 58A).

[1] The distance from the ' high-water mark of ordinary tides ' to the nearest point of Upper Deal appears to be about 3,900 feet (*Six-Inch Ordnance Survey*, Sheet 58).

[2] *Archaeol. Journal*, xxxiii, 1876, p. 71.

[3] *Ib.*, p. 58.

[4] *Ib.*, p. 59.

[5] *Proc. Geologists' Association*, ix, 1885–6 (1887), pp. 174–5.

[6] ' It is certain,' wrote Dowker in 1876 (*Archaeol. Journal*, xxxiii, 59), ' that when the sea swept the Stonar beach, Deal had no existence.' Even men of science sometimes use the word ' certain ' a little rashly. At that time Dowker asserted that the Stonar beach ' must have travelled from the cliff between Dover and Deal '. In 1887 (*Proc. Geologists' Association*, ix, 174–5) he ' pointed to the stones of which it is composed as evincing their origin from the cliff at Pegwell . . . To imagine it to have travelled from the south, we must,' he said, ' have a shore-line cutting far back beyond the Deal beach, of which at present there was no evidence.'

[7] *Itin. curiosum*, 2nd ed., 1776, pp. 126–7.

that of Rennell. He maintained that Caesar's camps must have been 'absorpt by the ocean, which has so long been . . . wasting the land away'. 'Even since Henry the VIII^ths time,' he continued, 'it has carried off the seaward esplanade of the three castles' [of Walmer, Deal, and Sandown].[1] But it does not follow that in the interval which separated the time of Caesar from the time of Henry the Eighth the sea in the neighbourhood of Deal had been continuously gaining upon the land. It would appear that in the last four centuries it has alternately advanced a little and receded.[2] In 1615, 1626, and 1627 the waves were wearing away the walls which had been erected for the protection of the castles of Walmer and Deal.[3] During the latter half of the eighteenth century, however, shingle was being rapidly thrown up along the coast between St. Margaret's Bay and a point which, as Mr. Elvin[4] says, was 'considerably to the north of Sandown Castle'; and, although during the first thirty years of the nineteenth century the sea was again encroaching, at all events at Walmer, the bank of shingle between the Rifle Range at Kingsdown and Walmer then began again to increase, while northward of Deal as far as Sandown Castle the sea was simultaneously gaining ground. In 1885 shingle was still accumulating at Walmer Castle and also at Deal, although it was recognized that at the latter place its movements were variable. For some years previously, however, the shingle which formerly protected the cliffs between St. Margaret's and Kingsdown had been travelling northwards past Walmer to Deal; and during the fourteen years that followed 1885 the same process was going on: I daresay it is going on still. At Deal, wrote Dowker in 1899,[5] 'the shore line has been nearly stationary until we approach the north end of Deal, where the . . . sea had washed most of the beach away and carried it past the Castle.' Finally, it must be borne in mind that from various places between Walmer and the North Foreland a great deal of shingle has been abstracted.[6] Still, if *The North West View of Walmer Castle*, by S. and N. Buck, which was published in 1735, was approximately accurate, the sea was a good deal nearer the castle then than it is now; and the observations that were made between 1741 and 1884 show that while in that period the sea at Sandown Castle gained 200 feet upon the land, off Deal Castle the increase of shingle amounted to 120 feet, and off Walmer Castle to no less than 385.

[1] It must be borne in mind that Stukeley wrote before the great increase of shingle in the neighbourhood of Walmer.

[2] *Ib.*; C. R. S. Elvin, *Records of Walmer*, pp. 2–3, 5.

[3] *Calendar of State Papers, Domestic Series*, of James I, 1611–8, p. 324 (vol. lxxxii, 129), under date 1615; *ib.*, Charles I, 1625–6, p. 321 (vol. xxv, 82), under date 1626; *ib.*, 1627–8, p. 200 (vol. lxv. 62), under date 1627. In the British Museum is a print, called 'N.W. View of Deal Castle', published in 1735, from which it would appear that at that time the castle was as close to the sea as it is now,—neither more nor less.

[4] *Records of Walmer*, p. 5.

[5] *Coast Erosion*, p. 3.

[6] *Report of . . . the Brit. Association*, 1888 (1889), p. 910. The following table,

The Reverend Beale Poste, a well-known antiquary of the nineteenth century, maintained[1] that the bank of beach upon which Deal stands must have existed in the time of Caesar, 'since numerous Roman coins are found at neap tides at low water on the chalk at the edge of the beach.' He added that 'when the piles for the pier were driven into the beach in 1842, it was found in a highly concrete state, almost like rock, denoting great antiquity'. The former statement, if it is correct,[2] would seem to prove that the shore-line has receded, in other words, that the sea has on the whole gained upon the land since the days of Caesar; the argument based upon the condition of the beach into which the piles were driven only tends to show that the lower stratum of the beach was old.

Quite recently a discovery has been made which ought to set the question at rest. Romano-British interments have been unearthed about seven hundred yards north of Walmer Castle, 'on the low ground . . . adjoining, and only on a slightly higher level than the Castle meadows.'[3] The spot where they lay is about two hundred and fifty feet west of the high-water mark of ordinary tides. The discovery, as Mr. Cumberland Woodruff remarks,[4] proves that 'the shore lands [between Walmer and Deal] were protected then as now, though probably [or rather certainly] by a much thinner line of shingle'.[5]

compiled by Major A. C. Hepper, R.E. (*ib.*, 1885, p. 440), illustrates the movements of the shingle during the period between 1741 and 1884:—

Place	*From*	*To*	*Increase* *Feet*	*Decrease* *Feet*
Walmer Castle	1741	1841	308	—
„ „	1841	1859	34	—
„ „	1859	1872	33	—
„ „	1872	1884	10	—
Deal Castle	1741	1859	85	—
„ „	1859	1872	—	40
„ „	1872	1884	35	—
Sandown Castle	1741	1859	—	145
„ „	1859	1872	—	50
„ „	1872	1884	—	5
No. 2 Battery	1859	1884	140	—

The encroachment of the sea north of Deal between 1848 and 1856 was due to the extraordinary prevalence of north-easterly winds.

[1] *Britannia antiqua*, 1857, p. 282.

[2] In regard to this statement, and also that of Roach Smith, recording the discovery of coins at Stonar (see p. 520, *supra*), Sir John Evans has written to me, 'I have no personal knowledge of either of the finds of Roman coins that you mention. Roach Smith, however, and Beale Poste are competent authorities in such a case, and I see no reason why you should not accept their statements.'

[3] *Archaeol. Cant.*, xxv, 1902, p. 1.

[4] *Ib.*, pp. 4–5. This discovery stultifies Hasted's remark (*Hist. of Kent*, iv, 1779, p. 173), that 'towards the village of Walmer [as one comes from Deal] is a flat, many feet lower than the high-water mark, which the beach thrown up along the shore has fenced from the sea, and which probably when *Caesar* landed on this coast might be all covered with water'. Cf. C. R. S. Elvin, *Records of Walmer*, p. 3.

[5] See also *Archaeol. Cant.*, xxvi, 1904, pp. 11–2.

The conclusion appears to be this. There is no reason to suppose that the coast-line between Sandown Castle and Walmer Castle was very different in Caesar's time from that which is depicted on the Ordnance Map; and there is positive proof that between Walmer Castle and Deal Castle, at some period of the Roman occupation, it was nearly the same. On the other hand, it is certain that since Caesar landed a great deal of shingle has accumulated along this part of the coast, especially at Walmer; and it may be inferred that the beach was less steep then than it is now.

III. THE GOODWIN SANDS

Before we attempt to inquire what was the condition of the Goodwin Sands in the time of Caesar, it will be well to state the relevant facts which have been ascertained since exact observations began to be recorded.

'The north-eastern part of the North Goodwin,' says the author of the *Channel Pilot*,[1] 'dries in places 7 feet at low water; the South Goodwin not more than 4 or 5 feet at any part.'

The form of the sands is altered periodically by the tides. Beale Poste argued in 1857 that the Goodwin Sands were still growing, as 'Kingsdown Mark, a pile . . . built in the reign of Elizabeth to show the South Sand head, is . . . of no use, the sand having now extended itself a mile further to the southward'. Moreover, he says, it was stated in the Report of the Commission of the Harbours of Refuge for 1845 that 'the Brake Sand, a branch of the Goodwin Sands in the Small Downs, had moved *bodily inwards* towards the shore seven hundred yards within the last fifty years'. This, he maintains, can only mean that 'a deposit has taken place on the inward side of the sand . . . while the outward side has been eroded by the winds and tides'.[2] In 1885 it was found that 'the former Bunthead shoal' had 'entirely disappeared',[3] and that 'the whole body of the South Calliper' had 'moved about a mile north-eastward'. Again, it was ascertained by 'a re-survey of the Downs, Goodwin Sands, and adjacent coast', executed in 1896, that since 1887 considerable changes had taken place. 'The Goodwin Sand,' we learn from this source, 'has continued its general movement towards the coast, and the area of drying sand has largely increased.'[4]

The results of borings carried out at various times in the Goodwin Sands have shown that blue clay, resting on chalk, was found at the depths of 7, 15, 57, and 78 feet.[5] From these data Sir Charles Lyell[6] concludes that the Goodwins 'are a remnant of land, and not "a

[1] Part i, 9th ed., 1900, p. 339.
[2] *Britannia antiqua*, pp. 288–9.
[3] *The Channel Pilot*, part i, 1900, p. 338.
[4] *Geogr. Journal*, ix, 1897, p. 655.
[5] G. B. Gattie, *Memorials of the Goodwin Sands*, 1890, pp. 3, 5–6.
[6] *Principles of Geology*, 1875, i, 530–1.

mere accumulation of sea sand " ; ' and, referring to the destructive storm mentioned in the *Anglo-Saxon Chronicle*[1] as having occurred in 1099, he conjectures that ' the last remains of an island, consisting, like Sheppey, of clay, may perhaps have been carried away about that time '.

Dr. Guest[2] holds that in Caesar's time the Goodwin Sands did not exist. He reminds us that, according to Somner,[3] it was the opinion of ' several men of judgement ' that they had not appeared until after the time of Earl Godwin, and, remarking that this was also the view of Sir Thomas More, he argues that ' we may infer that such at that period was the opinion of educated men who had local knowledge '. Leland,[4] he goes on to say, ' attributed the decay of Sandwich to the Goodwin Sands, and as Sandwich was a flourishing port in the fourteenth century, we may infer that it was not till the fifteenth that the sands attained those formidable dimensions which produced so much mischief.' Immediately north of Sandown Castle there is, he observes, a tract of land covered with low sand-hills, which, in Philipot's map of Kent, are called the ' smale downs ',[5] and upon which the sea has long been encroaching. He accounts for the name given to the roadstead by assuming that it once formed part of the ' smale downs ', and affirms his belief that ' the flats round Sandwich once projected into the sea as a low ness or foreland,—probably divided into islands, of which Lomea [an island which John Twine asserted to have formerly existed about four miles from Thanet] was the easternmost '. He assumes that as Lomea is not mentioned in Domesday Book, it perished by some natural convulsion before the end of the eleventh century, and goes on to say that ' After the destruction of this island, the Goodwin Sands may have been gradually accumulated, not necessarily on the site of the island, but near it, and the Downs just as gradually excavated '.[6] Beale Poste[7] also affirms that in 1098 ' an island named Lomea was overflowed, on which occasion the sands are said to have been formed. This is mentioned by Giraldus Cambrensis, and from him by Twine . . . But Earl Goodwin (*sic*) died in . . . 1053, and Domesday-book negatives that any extensive tract of land was overflowed and lost, in this direction.'

Now John Twine[8] (or Twyne) merely says that he has read about Lomea in the works of ' certain writers '. It was once, he says, a low fertile island, which was submerged in consequence of a great storm, and covered with sand, and it is now the Goodwin Sands. As for Giraldus Cambrensis, I have searched his writings diligently,

[1] *Anglo-Saxon Chronicle*, ed. B. Thorpe, ii, 1861, p. 203.—An. M.XCIX.
[2] *Archaeol. Journal*, xxi, 1864, pp. 235–6.
[3] *Treatise of the Roman Ports and Forts in Kent*, 1693, p. 24.
[4] *Itin.*, 1744, vii, 113.
[5] *Villare Cantianum*, 1669, map facing p. 1.
[6] See the fantastic map inserted between pages 330 and 331 of Guest's *Origines Celticae*, vol. ii, in which ' Lomea ' is placed N.W. of the Goodwins.
[7] *Report of . . . the Brit. Archaeol. Association . . .* Sept., 1844, p. 371.
[8] *De rebus Albionicis*, 1590, pp. 24, 27–8.

and I can find no mention whatever therein either of Lomea[1] or of the Goodwin Sands. The name 'Downs' is easily accounted for. 'The DOWNS,' says the author of the *Channel Pilot*,[2] 'in a general sense, implies the numerous banks lying immediately off the coast between the South and North Forelands . . . that [anchorage] which is commonly . . . known as the Downs is off the town of Deal between Walmer Castle and the northern part of the town,' &c. I see no reason to doubt that the name of the roadstead is derived from the aforesaid banks and from the sand-*dunes* on the shore.

Somner,[3] remarking that, according to the common opinion, Lomea was submerged in 1097, observes that there is no notice of such an island either in Domesday Book or in 'any Author whether foreign or domestick, of any antiquity, that ever I could meet with'.

The late C. H. Pearson[4] inferred from 'the legend of their formation'[5] that the sands were 'first remarked about the end of the eleventh century', and that they were 'probably formed by bank-currents gradually depositing sand about a shoal'.

On the other hand, S. Pritchard,[6] the historian, so called, of Deal, argues that the sands must have existed 'from all time' as otherwise Deal and the adjoining country would inevitably have been inundated. Why? The island, the former existence of which is assumed by Sir Charles Lyell, would have been as good a protection as the sands; and in the time, which was certainly anterior to the Roman invasion of Britain, when the shingle bank had not accumulated to a sufficient height,[7] the very small area in the neighbourhood of Deal which is below high-water mark may have been inundated, unless, as Dowker[8] and Mr. Spurrell[9] believe, the level of the land has been depressed since the Roman occupation.

[1] According to Chambers's *Ency.*, v, 1901, p. 296, Lomea has been identified with '*Infera insula* of the Romans'. The writer does not inform us by whom *infera insula* was mentioned.

[2] Part i, 1900, p. 337.

[3] *Treatise of the Roman Ports and Forts of Kent*, pp. 20–1.

[4] *Hist. Maps of England*, p. 2.

[5] Richard Lilburne (*Topographie . . . of . . . Kent*, 1659, pp. 262–3), alluding to the well-known legend as to the origin of the Goodwin Sands, says, 'the most probable relation of the rise of the same is thus. *Goodwin* . . . was . . . owner of a great quantity of flat Lands in the County (neer the *Isle* of *Thanet*) defended from the sea by a great wall, which lands afterwards (in the year 1099) was parcell of the possessions of the Abbot of St. *Augustine* (but reteyned the name of *Goodwin* . . .), and that Abbot, being then also owner of the Rectory of *Tenterden*, and having begun the building of this steeple . . . the thoughts, and actions, of him, and his agents were so set upon the finishing of that work, that they neglected the care of watching, and preserving the aforesaid wall, and (3. of November in that year) the sea broke over, and . . . drowned the aforesaid lands (overwhelming the same) with a light sand . . . and the place thereby obteyned the name of *Goodwin Sands* . . . and thus (accidentially) this *Tenterden* steeple is said to be the cause of *Goodwin Sands*.'

[6] *Hist. of Deal*, 1864, p. 106.

[7] See p. 524, *supra*.

[8] *Coast Erosion*, 1899, p. 12.

[9] *Archaeol. Journal*, xlii, 1885, pp. 284–5. According to Mr. Clement Reid (*Archaeologia*, part ii, 1906, p. 285) 'the relative level of sea and land in the south of England appears to have remained unchanged' since 'late Neolithic times'. See, however, Addenda, p. 740.

The reader has doubtless already concluded that it is impossible to affirm either that the Goodwin Sands existed in the time of Caesar, or that they had not then accumulated to such a degree as to attract attention, or that their place was occupied by an island. If the silence of Domesday Book and, as it should seem, the absence of any other positive testimony constitutes an argument against the hypothesis of Sir Charles Lyell,[1] the same argument may be advanced to show that before the Norman Conquest the sands had not begun to appear. Yet, as we shall see in a subsequent article, there is some reason to believe that either sands or an island were there when Caesar invaded Britain.[2] Tradition, vague as it is, combined with Lyell's authority, disposes me to accept tentatively the latter alternative.

IV. THE SOUTH FORELAND AND THE DOVER CLIFFS

Professor Montagu Burrows [3] affirms that 'the space over which the tides travel [in the Straits of Dover] must be at least two miles wider than it was some 2,000 years ago'. This is one of the *ex cathedra* statements in which the professor's work abounds, and for proof of which his amazed readers search his pages in vain. Dowker's estimate is more moderate: he only bids us 'assume the Straits are now one mile wider than when Caesar visited our shores'; [4] but, like Professor Burrows, he requires us to make this assumption in the dark.

In M. Vivien de St.-Martin's great work it is stated that Cape Grisnez 'perd en moyenne 25 centim. par an; autrement dit, il recule 25 m. par siècle'.[5] Assuming the accuracy of this statement, and assuming, further, that the rate of erosion has been constant since the invasion of Caesar, Cape Grisnez then projected seaward 489 metres, or about 534 yards further than it does now. I take for granted that the statement is based upon exact and prolonged observation; but when did that observation begin? [6]

As for the South Foreland, it is certain that, as Dowker says,[7] it is (or at all events was in 1885 and for some years previously) 'being gradually undermined by the sea'; but it would be a great mistake to leap to the conclusion that this erosion has been going on con-

[1] There is not much force in Professor Boyd Dawkins's argument (*Early Man in Britain*, p. 483), that an island on the site of the Goodwin Sands would not have escaped the notice of Ptolemy. Ptolemy does not mention Sheppey (or else Thanet) and other islands.

[2] See pp. 657–9, *infra*, and cf. R. Blanchard, *La Flamande*, pp. 128, 133.

[3] *The Cinque Ports*, p. 8.

[4] *Twenty-third Report East Kent Nat. Hist. Soc.*, 1881, p. 57.

[5] *Nouveau Dict. de Géogr. univ.*, ii, 1884, p. 542.

[6] According to M. Léon Lejeal, the author of an interesting article on 'Le littoral' in *Boulogne-sur-mer et la région boulonnaise* (i, 365), 'certains hydrographes affirment qu'au Grisnez, la falaise s'entame de 0,25 centimètres par an.' I presume that this was the authority upon which M. de St.-Martin relied.

[7] *Archaeol. Journal*, xxxiii, 1876, p. 60.

tinuously since the time of Caesar. In 1850 Captain K. B. Martin, who was harbour-master of Ramsgate, affirmed that the cliff between Dover and the South Foreland, being protected by 'an inclined plane of shingle' from the sea, had 'preserved its contour from time immemorial'.[1] The phrase is somewhat vague: but the captain was a careful observer; and we may believe him when he tells us that since his boyhood, fifty years before the time when he wrote, there had been no change.[2] Why, then, were the Dover cliffs and the South Foreland being gradually eaten away in 1876, when Dowker wrote, and in 1884? Simply because the supply of shingle had, from various causes, been cut off.[3] The erosion, said Mr. E. R. N. Druce, Engineer to the Government pier at Dover, takes place 'at no particular rate, but falls of cliff at the points above named have taken place at intervals for some years past . . . since they have lost the protection of the shingle at their base'. He added that the loss was 'confined to areas bare of shingle', and that, so far as he could ascertain, there existed no 'data for determining the rate of erosion from early maps or other documents'.[4] It would appear, then, that Professor Burrows's assertion is based upon pure imagination.

[1] *Naut. Mag.*, 1850, p. 216.

[2] In Capt. McDakin's *Coast Erosion,—Dover Cliffs*, 1899, pp. 7–9, a list is given of the notable falls which have been recorded. In 1853 there was a heavy fall near Holy Trinity Church, Dover; in 1872 at the East Cliff; in 1896 at the South Foreland; and (*Times*, Jan. 11, 1905, p. 7, col. 1, Jan. 13, p. 7, col. 2) in 1905 there were landslips at St. Margaret's Bay, near Hope Point, and at Fan Bay.

[3] Dowker (*Twenty-third Report East Kent Nat. Hist. Soc.*, 1881, p. 63) attributed this loss of shingle to the Admiralty Pier at Dover. 'The formation of the Dover Harbour,' he says, 'has favoured the accumulation of beach west of that point; the current, moreover, after passing the obstacle, is deflected inland, and thus, at St. Margaret's Bay, a former collection of beach is being removed towards Deal.' On the other hand, Sir John Coode, who is described in the *Dictionary of National Biography* (Suppl., ii, 52) as 'probably the most distinguished harbour engineer of the nineteenth century', states (*Parl. Papers*, lviii, 1873, p. 455[3]) that 'so far from the pier having acted as a check to the passage of the shingle, there has been a considerable loss to the westward of it within the last 20 years'. 'I have no hesitation,' he adds (*ib.*, p. 456(4]), 'in stating, in the most distinct and positive terms, that this decrease [of shingle on various parts of the coast south-west of St. Margaret's Bay] has not been caused by "the extension of the Admiralty Pier at Dover", inasmuch as the various facts that have been brought out in the course of my recent investigation lead distinctly and unmistakably to the opposite conclusion . . . having regard to the facts previously stated, as to the diminution of shingle to the westward of Folkestone, near Sandgate and Hythe, &c. . . . I have arrived at the conclusion that this [decrease of shingle between Dover and St. Margaret's Bay] is due to the remarkable accumulation of shingle, and consequent projection towards the south-east of Dungeness' (*ib.*, p. 457[5]).

About the year 1721 the supply of shingle was temporarily cut off by the fall of part of the Castle Cliff. See Capt. John Perry, *Account of the Stopping of Daggenham Breach*, &c., 1721, p. 119.

[4] *Report of . . . the Brit. Association*, 1885 (1886), p. 439. See also pp. 406–7. According to J. B. Redman (*Proc. Inst. Civ. Engineers*, xi, 1851–2 [1852], p. 164) 'it appears that at an early period there was no shingle at all at Dover . . . which there is historical evidence to prove was the case; its gradual advance from the westward eventually blocked up the entrance', &c. Where the

When the subsidence which had taken place in the Neolithic Age was virtually complete the sea was bordered by a narrow plain, to which the high ground descended gradually. Erosion was at first rapid while the waters were devouring loose talus; but when beaches had had time to form it was of course retarded.[1] How slow it is where the rocks are hard is proved by the fact that the contour of a prehistoric camp near Hastings shows that the seaward defence was formed not by an artificial rampart but by the East Cliff.[2] Yet Professor Burrows asks us to believe that erosion has been as rapid in the chalk of the South Foreland as in the soft cliffs between Flamborough Head and the Thames.[3] Generally speaking, as erosion proceeds, cliffs become higher;[4] and it is obvious that if the Channel had been two miles wider in Caesar's time, the Dover cliffs, if they had existed, would have been insignificant. But since Caesar described them as 'precipitous heights',[5] and Cicero as 'astonishing masses of cliff',[6] they were evidently little lower then than now. Let the reader ponder these things, and he will realize how monstrously exaggerated is the estimate which assigns to the Straits of Caesar's time a breadth two miles less than our modern maps show.[7]

V. DOVER HARBOUR

That a natural harbour existed at Dover in the time of Caesar is beyond dispute. It is mentioned under the name of *Portus Dubris* in the *Itinerary* of Antonine;[8] and it was connected by a Roman road with Canterbury and London, and also with Richborough. Napoleon

'historical evidence' is to be found Redman omits to say; and I cannot find it: but it is certain that the movement of shingle along the coast began long before the historic period (*Geogr. Journal*, xxviii, 1906, p. 489).

Capt. McDakin (*Coast Erosion,—Dover Cliffs*, p. 5) remarks that 'the Roman Pharos on the Castle Cliffs and the foundations of a similar building in the Redoubt on the Western Heights, give us no indication that the edge of the cliff has receded since those earliest of Roman buildings occupied their present site'.

[1] Clement Reid in *Vict. Hist. of . . . Sussex*, i, 25, and *Geogr. Journal*, xxviii, 1906, pp. 488–9.

[2] *Vict. Hist. of . . . Sussex*, i, 469.

[3] See *Geogr. Journal*, xxviii, 1906, p. 490.

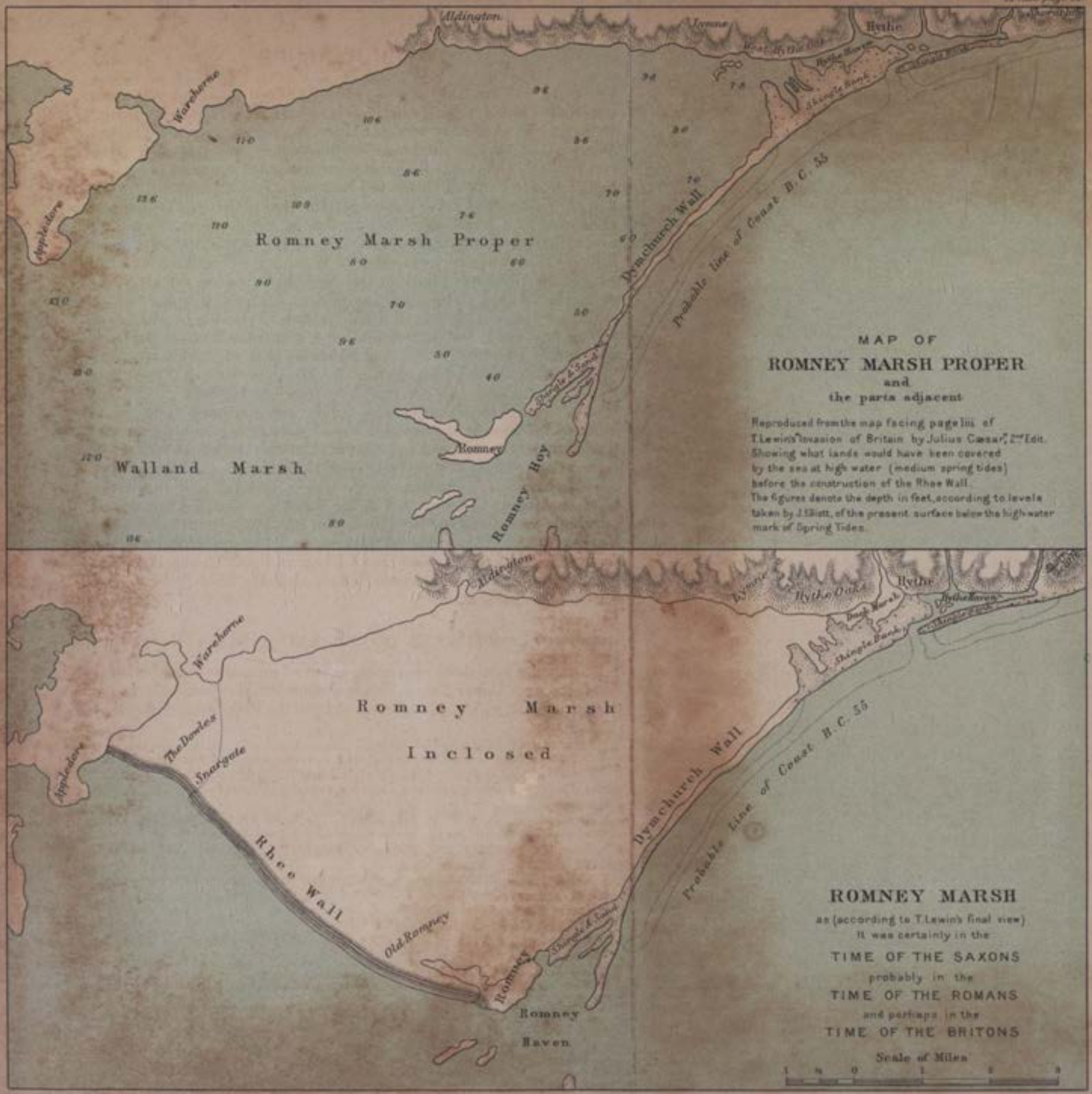
[4] *Ib.*, p. 489. Cf. A. J. Jukes-Browne, *Handbook of Phys. Geol.*, 1892, p. 171.

[5] Angusti montes (*B. G.*, iv, 23, § 3).

[6] See p. 329, *supra*.

[7] Experiments recently conducted by Captain McDakin (*Coast Erosion,—Dover Cliffs*, pp. 3–4, 12) showed that 'the average erosion of four years was unexpectedly small, only amounting to half an inch in a year'. He admits, indeed, that the average rate, since erosion began, 'has probably been much more rapid.' His general conclusions are, 'that the heaviest falls . . . take place after long continued rain . . . That the springs issuing from the base of the cliffs play an important part in undermining and bringing down the cliffs; and that the sea charged with a small amount of shingle [which it discharges like a gun] attacks the undercliff and removes it, but where the shingle accumulates in large quantities, it defends and supports the base of the cliffs,' &c.

[8] Ed. Wesseling, p. 473. See also *Corpus inscr. Lat.*, vii, 1228.



Stanford's Geogl Estabt London

the Third [1] affirms that it was entirely choked up about 950 A.D.; but this is a blunder, for the harbour is mentioned in Domesday Book.[2] Even as late as 1582 it was stated by an engineer, named Thomas Digges, that 'Before the peere was builte out, there are men alyue can remember that was no banckes or shelues of beache to be seene before Douer,[3] but all cleane sea betwene Arteclif [Archcliff] tower and the castle clyffe'.[4] Captain Martin [5] holds that the remains of anchors which have been dug up out of meadows in the valley prove that the estuary was navigable as far as Crabble; [6] and he believes that it actually extended to Water's End,[7] and covered the sites of the villages of Charlton and Buckland. Canon Puckle, however, argues that 'the primitive haven' covered a space which extended barely a quarter of a mile inland, 'bounded by the lower half of St. James' Street, Dolphin Lane, and Russell Street, and the east end of Dolphin Lane,' [8] and he states that when this area was 'partly uncovered in excavating for the new Russell Street gas works, quays and hawser-rings were brought to light'. Captain Martin's estimate, which is based upon very uncertain data, must be regarded as an exaggeration: the estuary may possibly have extended up to Crabble, but was certainly not navigable so far except perhaps by coracles. Many years ago the remains of a Roman bath were discovered on the site of St. Mary's church,[9] and in 1887 a statue belonging to the period of the Roman occupation was found 'during excavations for the foundation of the Carlton Club, in the Market Place'. These discoveries help to define approximately the western limit of the harbour; and I believe that Planche 17 of the Atlas accompanying Napoleon's *Histoire de Jules César* [11] represents it with tolerable accuracy.

VI. BETWEEN DOVER AND SANDGATE

During the last three centuries, at all events, the coast between Sandgate and Dover has undergone considerable changes. Large quantities of stone have been removed from the Folkestone cliffs; and

[1] *Hist. de Jules César*, ii, 157, note.

[2] *Domesday Book of Kent*, ed. L. B. Larking, 1869, p. 93, and Extension, p. 2.

[3] I need hardly say that Digges's statement, which refers only to the inlet where the port of Dover had been, does not support Redman's assertion (p. 529, n. 4, *supra*).

[4] *Archaeologia*, xi, 1792, p. 212, note *a*. Archcliff Fort is about 400 yards west of the first groyne on the western side of the Lord Warden Hotel.

[5] *Naut. Mag.*, 1850, p. 269. See also John Leland, *Itin.*, vii, 1744, fol. 128, p. 117.

[6] About 1 mile 4,100 feet in a straight line from the present high-water mark of ordinary tides (*Six-inch Ordnance Survey*, Sheet 68).

[7] Nearly 2 miles beyond Crabble (*ib.*, Sheets 67–8).

[8] *Archaeol. Cant.*, xx, 1893, p. 129.

[9] *Archaeologia*, v, 1779, p. 325; John Lyon, *Hist. of . . . Dover*, i, 1813, p. 9; *Archaeol. Cant.*, xx, 1893, p. 131.

[10] *Ib.*, xviii, 1889, p. 202.

[11] See also T. Hyde Page, *Considerations upon the State of Dover Harbour*, &c., 1784, p. 6.

landslips have occurred at Shakespeare's Cliff, between Folkestone and Sandgate, and behind East Wear Bay.[1] It would be useless, however, for our purpose, to describe these changes in detail; for they do not affect the topographical questions that belong to the history of Caesar's invasions of Britain. Excepting the disappearance of the little haven that once existed at Folkestone, the general character of this section of the coast was much the same in 55 B.C. as to-day. It may be, however, that the aspect of the high ground above East Wear Bay was different. Between the cliffs and the heights which rise about a quarter of a mile to the north of them there is a wild and broken plateau, called the Warren, through which the railway runs. Referring to this, William Phillips, a geologist of some repute, wrote in 1821, 'The cliff, bounding this ruin towards the sea, is, from its position, not *in situ*; and it is equally clear that the enormous masses of which it is composed, have fallen forward [probably by 'repeated falls'] from near the summit of the cliff *in situ*.'[2] When these convulsions began to transform the landscape cannot, as far as I know, be ascertained.

VII. ROMNEY MARSH

Between Hythe and Dungeness, on the other hand, there has been complete transformation. There, within the brief span of historical time, wind, tide, and river, and finally the labour of man, have wrought changes as remarkable as those that in other regions required the lapse of ages which the imagination fails to conceive. The antiquary who walks from Westenhanger Station to the brow of Lympne Hill, and looks out over the vast field of shingle that extends seaward, and, on his left, towards Hythe, and then over the broad level of the marsh that stretches away on his right between the Wealden upland and Dymchurch Wall, will easily picture to himself the scene that once was there.

1. Before we attempt to construct a map which may represent the coast-line between Sandgate and Dungeness, as it was in the time of Caesar, it will be well to state those relevant facts which are accepted by all geographers. There was a time when the area of Romney Marsh was covered by a bay. At a later epoch the marsh was fringed by a bar of shingle, which extended from Winchelsea to a point nearly opposite Shorncliffe. Between West Hythe and Shorncliffe streams flowed down from the hills, gradually forced an opening in the shingle opposite Hythe, through which the sea entered, and thus formed Hythe harbour, which, after remaining open for many centuries, was finally choked up about 300 years ago. For

[1] See *Phil. Trans.*, xxix, 1716, p. 469; lxxvi, 1786, p. 220; W. Lambarde, *Perambulation of Kent*, ed. 1826, p. 154; *Mem. Geol. Survey,—The Geology of the Weald*, 1875, pp. 302, 315–6; *Proc. Geologists' Association*, xiii, 1895, pp. 40–7; Capt. McDakin, *Coast Erosion,—Dover Cliffs*, pp. 7–9; *Pall Mall Gazette*, Jan. 18, 1906, p. 12, col. 2.

[2] *Trans. Geol. Soc.*, v, 1821, p. 17.

some time after the marsh became habitable the shingle protected it from the sea on the south, but gradually was so diminished that it became necessary to construct a sea wall. The river Rother debouched at some point within the area of Romney Marsh. During the Roman occupation of Britain there was a harbour called the Portus Lemanis, which has been located by one writer at Romney and by others at Lympne, while some have identified it with Hythe Haven. West of West Hythe Oaks, the marsh 'is a rich mould . . . while all to the east, as far as Sandgate, is (with the exception of a narrow strip to the south and east of Hythe, between the sea-beach and the hills) one vast bed of shingle'.[1]

2. The whole of Romney Marsh, properly so called,[2] is even now below the level of high water at spring tides. The hills which form its northern boundary have themselves changed since the time when the waves broke against their base. In the course of ages they have lost their original sharpness of outline, and, as we learn from the geologist who has described the formation of the Weald, have been 'worn down into undulating ground';[3] and nearly 200 years ago a local observer described how, after an unusually wet season, Lympne Hill had been completely transformed, in a single night, by a landslip.[4] But these changes are insignificant in comparison with that by which the old Bay of Appledore has become a fertile pasture. Of what material is this land composed? According to the late Thomas Lewin, it is 'absolutely and exclusively a sea deposit'; and, in proof of this assertion, he pointed to 'the marine shells which pervade the whole mass'.[5] But it needs little acumen to see that the presence of marine shells in the marsh does not justify Lewin in using the words 'absolutely and exclusively'; and the late Colonel George Greenwood maintained that the marsh had been formed by material brought down from the Weald by 'the aqueous erosion of the Rother'.[6] As a matter of fact, it was formed by the combined action of river and sea.[7] But unless and until a series of borings are systematically made, it will be impossible to describe the recent strata with precision.[8]

According to Topley, 'The cause of the original formation of

[1] The quotation is from T. Lewin, *The Invasion of Britain by Julius Caesar*, 2nd ed., 1862, p. lvii.

[2] Romney Marsh Proper extends eastward of the Rhee Wall, which runs from Appledore to New Romney.

[3] *Mem. Geol. Survey,—The Geology of the Weald*, p. 251.

[4] *Phil. Trans.*, xxxv, 1727, pp. 551–2.

[5] *The Invasion of Britain by Julius Caesar*, 1862, p. lii. We learn from the late F. Drew (*Mem. Geol. Survey,—The Geology of the Weald*, p. 206) that 'whatever the soil may be near the surface, it is almost invariably the case that, at a depth of 10 or 20 feet, there is loose sand, often containing recent marine shells', &c. See note 8, *infra*.

[6] *Athenæum*, Aug. 5, 1865, pp. 184–5.

[7] See *Geol. Mag.*, 1869, p. 128. The writer, 'W. T.,' was evidently the late geologist, William Topley.

[8] See *Proc. Geologists' Association*, xv, 1898, pp. 212–3, 222. As far as I can discover, the only absolutely trustworthy boring which has been made (*Quart. Journ. Geol. Soc.*, xliii, 1887, p. 204) shows that at Holmestone, near Lydd,

Romney Marsh is altogether unknown. It is usually attributed to " the meeting of the tides " ; but as this takes place over a rather wide area, and as shingle beaches and alluvial flats occur where no tides meet, the explanation is not altogether satisfactory.'[1] The well-known geologist, F. Drew, explains that as soon as the bay had become so shallow from the accumulation of silt that its bed was exposed at low water, the sediment carried down by the Rother began to be deposited on the surface. Like Topley, he confesses that how the silt had accumulated is ' not quite clear ' ; and he thinks that ' the newly formed surface ' may have been ' actually upheaved by oscillation of level, forming a plain well raised above the level of the sea ',[2] which, however, before the historic period, must have suffered a subsidence.[3] This supposition was based upon the fact that trees are found near Appledore a few feet below the surface, which, if they are *in situ*, must have grown at a time when the marsh was above the level of the sea, and were perhaps contemporaneous with the submerged forests of Devonshire and Cornwall.[4] Some authorities, however, as we shall presently see, hold[5] that they were drifted into their present position.

The late James Elliott, who in the middle of the nineteenth century was engineer of Dymchurch Wall, diligently investigated the history of the marsh, and added much to our knowledge. While the marsh was being formed it was gradually closed by a bar of shingle, composed of pebbles which had been partly broken off from the cliffs on the south-west, partly carried down by rivers,[6] and had been driven up the Channel by the prevailing winds.[7] Elliott remarks that ' the result of such a protection from the open sea would be, that all matter brought down by the hills would rest

which is outside the limits of Romney Marsh Proper, the recent strata, overlying Hastings beds, were as follows:—Shingle, 15 feet; Boulders, 4 feet; Brown Sand, 13 feet; Clay, 4 feet; Black and Grey Sand, 20 feet; Pebbles, 1 foot.

' Mr. Elliott,' says Drew (*Mem. Geol. Survey,—The Geology of the Country between Folkestone and Rye*, 1864, p. 16), ' tells me that he bored 70 feet in the Marsh, of which the last 50 were in sand.' ' I contend, however,' replies Dowker (*Proc. Geologists' Association*, xv, 212), ' that this does not prove anything, since the sand probably belonged to the Hastings Beds.'

[1] *Mem. Geol. Survey,—The Geology of the Weald*, p. 304. Topley goes on to point out that F. H. Appach, in *C. J. Caesar's Brit. Expeditions*, &c., p. 16, adopted a theory which had been originally put forward by James Elliott, but had been discovered by Elliott himself to be erroneous, attributing ' the silting up of the area ' to ' the presence of some supposed islands of Hastings Sand near Romney '.

[2] *Mem. Geol. Survey,—The Geology of the Country between Folkestone and Rye*, pp. 19–20.

[3] See p. 543, n. 1, *infra*.

[4] See p. 62, *supra*.

[5] See p. 543, *infra*.

[6] See *Proc. Inst. Civil Engineers*, xl, 1875, pp. 69–70.

[7] *Ib.*, pp. 109, 111. Lord Avebury, who refers to this paper in *The Scenery of England*, 3rd ed., 1904, p. 152, reports Sir Joseph Prestwich's views as to the movement of shingle incorrectly. Prestwich considered it ' well established ' that the general movement of the shingle along our south coast was eastward, although in the west bay of Portland it travels in the opposite direction.

nearly where it was first deposited, and, in process of time, dry land, at certain states of the tide, would appear' ; and that, on the ebb of every tide, ' all the water in the bay gradually receded towards the hills, and . . . made its exit at the eastern end of the shingle bank.'[1] He concludes that the shingle extended rapidly until it reached the eastern end of what is now Dymchurch Wall, but that its progress thenceforward was extremely slow. Meanwhile the sediment deposited by the sea was gradually raising the surface of the marsh.[2] Elliott, whose statements and opinions were incorporated by Lewin in his book on the invasions of Caesar, affirms that the advancing shingle spit was ' intersected only by a channel between Lydd and Romney ', which was ' the mouth of the estuary which lay behind the shingle ' ;[3] but Lewin, in a later article on the *Portus Lemanis*,[4] appears to have abandoned this view, for he there implies that the spit was continuous. At some period which preceded the erection of the Rhee Wall, that is to say, the first enclosure or ' inning ' of the marsh, it would appear to have reached the foot of the hills at West Hythe Oaks.[5] The result, according to Lewin, was that the marsh was temporarily enclosed. But, he says, ' this bar to the exit of waters from the marsh could not long continue, for, though the sea was excluded, the Limen [that is to say, the Rother] . . . and twenty smaller streams were continually increasing the volume of water within the marsh, and . . . the shingle spit was burst asunder between Romney and Lydd.' Thus, if Lewin's final view is correct, the sea again found an entrance on the west of Romney, and continued to overflow the marsh at high tide until it was finally shut out by the erection of the Rhee Wall. West Hythe Oaks was not the final

[1] C. Roach Smith, *Report on Excavations . . . at Lymne*, 1852, p. 41.

[2] *Proc. Inst. Civil Engineers*, vi, 1847, p. 467.

[3] T. Lewin, *The Invasion of Britain*, &c., 1862, pp. lxviii–lxix. See also p. lvi.

[4] *Archaeologia*, xl, 1866, pp. 361–74.

[5] Lewin observes (*The Invasion of Britain*, &c., 1862, pp. lvii–lviii) that as far eastward as West Hythe Oaks the shingle ' fulls ' all curve westward, having been bent in that direction by the inrush of the tides ; while from West Hythe Oaks to Sandgate they all curve towards the east. This, he says, proves that when they were formed, the mouth of the estuary near Hythe had already been closed. Appach, on the other hand (*C. J. Caesar's Brit. Expeditions*, p. 21, § 9), does not believe that the shingle spit reached West Hythe Oaks. Referring to the change of curvature in the shingle fulls, he says that it was ' evidently due to the cessation of the indraught ', which was ' obviously caused by the erection of the ancient wall at West Hythe '. Hence, he concludes, ' the fulls to the north of the point [where the change of curvature takes place] . . . were not formed until after the wall at West Hythe was built ; and as this is part of the north-eastern boundary of Romney Marsh, it follows that the fulls in question were formed after the formation of Romney Marsh.' Lewin also mentions ' the ancient wall at West Hythe ' ; but his final theory is that the erection of this dam became necessary because the shingle spit, after it had reached West Hythe Oaks, was burst by the waters, fed by the streams mentioned above (p. 532), which accumulated in the space between West Hythe Oaks and Hythe (see p. 547, *infra*). Appach holds that Romney Marsh was not formed until after the Romans had abandoned Britain ; and he is therefore constrained to argue that Hythe Haven did not exist during the Roman occupation, and that the Portus Lemanis was at Lympne. Both of these theories will be refuted in this article (pp. 543–8, *infra*).

goal of the shingle spit. For a long period, as Lewin remarks, 'the shingle from the west continued to advance . . . and for a time without again touching the hills;' but at length the advancing spit 'was again wrested aside and dashed against the hills at Hythe, between the present barracks and the more eastern of the two Hythe bridges over the canal'. According to Elliott, however, whose view was adopted by Lewin in the Appendix to his book on the invasion of Britain by Caesar, the shingle was not 'dashed against the hills at Hythe', but opposite Shorncliffe. Anyhow the final result was that from the eastern end of what is now Dymchurch Wall to a point nearly opposite Shorncliffe there extended an irregular tract of shingle, broken only opposite Hythe by an opening, which led to a narrow harbour extending along the foot of the hills. This opening was due to the streams which flowed down from the hills and found a vent by bursting the barrier of shingle, and the scour of which kept the harbour open until, about three hundred years ago, it was finally choked up. According to Elliott, the western extremity of this harbour was at West Hythe Oaks; according to Lewin's final view at Hythe itself. Between Dymchurch and Hythe the shingle formed a broad field; but the section between Hythe and Shorncliffe, which formed the southern boundary of Hythe harbour, was long and narrow. The whole tract was 'perfectly flat and *above high-water mark*'; and Elliott argues that it extended much further seaward in Caesar's time than it does now, because, while the supply of shingle drifted from the south-west was cut off by the gradual elongation of Dungeness, the eastward movement of the shingle along the fringe of the marsh still went on.[1] This argument he supports by a comparison of

[1] It is hardly necessary to point out that Dungeness *is* of recent formation. Various theories have been advanced as to its origin (see Mr. F. P. Gulliver's paper in the *Geogr. Journal*, ix, 1897, pp. 536–46, and *Proc. Inst. Civil Engineers*, xi, 1852, pp. 212–21); and attempts have been made to determine the time at which the oldest of the shingle 'fulls' which constitute the 'ness' was formed, by calculating the rate at which the point has advanced seaward since observations began to be recorded. Elliott remarks (*ib.*, vi, 1847, p. 476) that 'from the best existing data' Dungeness would appear to extend annually about two yards further out to sea; and that, as the rate of increase was probably more rapid at first, we may conclude that about nineteen hundred years have elapsed 'since the sea first left the original "full" at Lydd'. According to Redman (*ib.*, xi, 1852, p. 174), the increase has not been regular, and 'during certain periods the Ness has even been stationary': from the middle of the seventeenth to the middle of the nineteenth century, he adds, the average annual increase was nearly six yards. This is probably an exaggeration. Sir John Coode (*Parl. Papers*, lviii, 1873, p. 457) ascertained, from particulars recorded at the Trinity House, that 'from the year 1792 to 1850 the point advanced seaward 530 feet, or say, at the rate of 9 feet per annum; whilst from 1850 to 1871, the advance was 280 feet, or at the rate of from 13 to 14 feet per annum'. Topley (*Mem. Geol. Survey,—The Geology of the Weald*, p. 314) thinks that 'the oldest fulls are 1,000 years or more old'. Similarly Drew (*ib.*, p. 308) says that the shingle which forms Dungeness 'must have been . . . collected since the Rother first came to Romney'. See also H. J. Mackinder, *Britain and the British Seas*, 1902, pp. 42–3. 'In early Roman times,' he remarks, 'Dungeness appears not to have existed'; and he suggests that its formation was due to 'the diversion of the Rother mouth for the purpose of reclaiming Romney Marsh'.

the Ordnance Survey map executed in 1817 with an old map of the marsh, probably made about the year 1550, which is in the Cottonian MSS.[1] at the British Museum. Assuming the accuracy of the old map, it would appear that in the 267 years the shingle had receded about two furlongs; and Elliott concluded that in Caesar's time the coast line at Hythe must have been nearly a mile from the hills. Having had considerable experience in the handling of old maps, I so far differ from Elliott that I am rather disposed to assume the inaccuracy of the one on which he relies; but he is quite justified in concluding that the coast line was much further from the Hythe hills in 55 B.C. than now.[2]

Elliott's account of the formation of the Marsh has, however, been recently disputed in a paper by George Dowker,[3] which, although it swarms with bibliographical and historical mistakes,[4] cannot safely be ignored. The author begins by endeavouring to show that the Rother originally entered the sea at Romney; that it gradually raised both its bed and its banks by depositing sediment; and that 'the Rhee Wall was, in the first place, a natural river-bank'—the bank of the Rother—'subsequently raised and altered by the Barons of the Cinque Port of Romney',[5] but (if I have grasped his meaning, which is often obscure) only between Snargate and Warehorn.[6] He tells us that 'The sequence of changes in the Marsh may be summarized as follows:—Firstly, a shallow bay existed in a depression in the underlying rocks. Into this bay the waters of the Rother, Tillingham, and Brede, on their way to their outlet near Romney, deposited their silt, so that the northern half of the Marsh had become dry land previous to the time of the Romans. Around this bay were formed sand-hills. In time of flood the waters of the river that ran out at

[1] *Cot.*, Aug. I, i, 24–5.

[2] *The Invasion of Britain*, &c., 1862, pp. lvii–lx, cxx; *Proc. Inst. Civil Engineers*, xi, 1852, p. 169. Cf. *Mem. Geol. Survey,—The Geology of the Weald*, p. 312. Dowker (*Twenty-third Report East Kent Nat. Hist. Soc.*, 1881, p. 66) suggests that the Hythe beach may have come from the east!

[3] *Proc. Geologists' Association*, xv, 1898, pp. 211–23.

[4] It would be waste of time to catalogue these blunders, which will be obvious to any one who knows the literature of the subject: but I may remark that Dowker devotes several pages to a refutation of Elliott's earlier theory, which Elliott himself corrected in the notes with which he furnished Lewin; and that he ignored or was ignorant of Elliott's matured conclusions. He says (p. 214) that Elliott's 'first paper was written to assist Mr. Lewin . . . and his theory was printed with Mr. Roach Smith's "History of Further Excavations and History of the Roman Castrum at Lympne"'. Elliott's first paper (*Proc. Inst. Civil Engineers*, vi, 1847) was not written to assist Lewin; nor was his second, which was printed, not in a book which neither Roach Smith nor any one else ever published, but as an appendix to Roach Smith's *Report on Excavations made on the site of the Roman Castrum at Lymne*, 1852. The notes which Elliott wrote to assist Lewin were printed in the second edition of Lewin's *Invasion of Britain by Julius Caesar*, which appeared in 1862, and which Dowker never mentions. The unhappy man cannot even refer correctly to his own works. In his bibliographical note (p. 223) he quotes under his own name a paper 'On the River Limen', in *Archaeol. Cant.*, vol. xviii, in which no such paper is to be found.

[5] *Proc. Geologists' Association*, xv, 1898, p. 219.

[6] *Ib.*

Romney overflowed, and, depositing silt, raised the banks on either side. A slight depression of the land commenced, and has continued. Beaches accumulated, especially between Romney and Hythe, and between Romney and Winchelsea. Romney probably formed a promontory near Dymchurch, near where the ancient river, then called the Limen, discharged its waters.'[1] He explains that originally the sea was excluded from the marsh by sand-hills, and that 'the sand-hills appear to have been formed at a period before the accumulation of the beaches had commenced, since the beach effectually stops the formation of sand-hills '.[2] No sand-hills now exist in the marsh, except between Rye and Lydd, near New Romney, and near West Hythe; but, says Dowker, 'We may connect these sand-hills by a hypothetical line extending from Rye to Hythe.'[3] The reason which he gives for believing that there has been a depression of the land since the time of the Romans is that he has found evidences of post-Roman subsidence in 'the neighbourhood of Richborough, Reculvers, and the Swale marshes of Sittingbourne '.[4]

Now Dowker gives no sufficient reason for refusing to accept Elliott's view (which he travesties) that the sea once found its way over the marsh through a gap between the advancing shingle and the hills, and also through a break in the shingle spit,—in other words, for maintaining that the marsh had become dry land before the shingle beach was formed. The notion that the Rhee Wall was, 'in the first place, a natural river-bank' is simply fantastic. To begin with, its direction is almost a straight line, whereas it is well known that in open plains, where the slope is slight, rivers invariably pursue tortuous courses.[5] Along what is now called the Rhee Wall runs the high road from Appledore to New Romney. It occupies what was formerly a channel embanked on either side; and this channel provided an outlet for the waters of the Rother, whose actual mouth was at Appledore.[6] As Elliott says, 'In erecting this wall it became necessary to provide some exit for the waters from the hills as well as for the drainage of the land enclosed. This was done by cutting a channel parallel with the wall from the pool or lake at the *embouchure* of the river Limene at Appledore to the sea at Romney... the wall was necessary to be continued across this lake until it met the high land at Appledore.'[7] Again, I cannot understand why, if Romney Marsh Proper became dry land before the time of the Romans without being artificially enclosed, Walland Marsh and Guildford Marsh, which lie west and south of the Rhee Wall, should still have been periodically overflowed by the sea; nor is it clear how in that case the Rother could have excavated its hypothetical channel along the line of the Rhee Wall. Lastly, it is impossible, on Dowker's theory, to locate the Portus Lemanis. He denies that it was at Lympne: it could

[1] *Proc. Geologists' Association*, xv, 1898, p. 222. [2] *Ib.*, p. 214.
[3] *Ib.* [4] *Ib.*, p. 221. See p. 527, *supra*.
[5] See A. H. Jukes-Browne, *Handbook of Phys. Geology*, 1892, pp. 138–9, 219. The lower course of the Great Stour is a good example.
[6] See p. 543, *infra*. [7] *Archaeol. Cant.*, xiii, 1880, pp. 271–2.

not, on his theory, have been at Hythe or at West Hythe, for he implies that the shingle beach, behind which lay the historic Hythe Haven, did not yet exist;[1] and Romney—the only other possible site—is, as I shall afterwards show, out of the question.

I am not concerned to dispute Dowker's theory that the sea was excluded from the marsh on the south by sand-hills before the shingle beach was formed, though the mere presence of patches of blown sand near West Hythe and near Romney does not justify him in connecting them by 'a hypothetical line extending from Rye to Hythe'; nor does he offer any theory to account for the disappearance of this hypothetical line after it began to be protected by a barrier of shingle. The important point is that the fact of the erection of the Rhee Wall proves that before it existed Romney Marsh Proper was liable to be flooded by high tides.

3. It has long been a vexed question where, in the time of Caesar, and during the Roman occupation of Britain, the Rother discharged itself. Hasted[2] affirms that the bed of the river 'may yet very easily be traced . . . under the hills from *West Hythe* to Appledore'. Beale Poste,[3] who agrees with him, says that, according to the *Itinerary* of Antonine, the port of the river Lemanis, which he identifies with the Rother, was the Portus Lemanis; that, according to Somner, ancient records mention 'the Lymne branch of the Rother as still in existence in . . . 820 at . . . Warehorne, at about . . . three miles from the bend of our river towards Lymne'; and that 'we find the name Portus Limneus in Ethelwerd's *Chronicle*, iv, 3, in his annals of . . . 893, which seems to imply the "Port of the river Lemanis".' Holloway,[4] the historian of Romney Marsh, after saying, like Hasted, that 'traces of the ancient bed of a river are still visible under the foot of the Kentish cliffs', adds that 'our ancient chroniclers, according to Lambarde, called this same place "Limene Mouthe", and which is interpreted by Leland to betoken the mouth of the river Rother'. Drew[5] holds that the river Limen, or, as it is called by the anonymous geographer of Ravenna, Lemana,[6] must in the ninth century have flowed past Sandtun, 'the patch of Blown Sand between West Hythe and Butter's (or Botolph's) Bridge,' because in a charter of the year 833 allusion is made to 'a piece of land at Sandtun that was bounded on the south by the river Limen'. Finally, Mr. F. P. Gulliver thinks it probable that the Rother had, a thousand years ago, two 'main distributaries', one of which flowed out 'through an inlet in the bar south-west of Hythe'.[7]

[1] *Proc. Geologists' Association*, xv, 1898, pp. 221–2.
[2] *Hist. of Kent*, iii, 1790, p. 532.
[3] *Britannia antiqua*, pp. 262–3.
[4] *Hist. of Romney Marsh*, 1849, pp. 16, 20.
[5] *Mem. Geol. Survey,—The Geology of the Country between Folkestone and Rye*, pp. 19–20.
[6] *Ravennatis anonymi cosmographia*, ed. M. Pinder and G. Parthey, 1860, v, 31 (p. 438, 19).
[7] *Geogr. Journal*, ix, 1897, p. 545.
Mr. H. E. Malden, who believes that Caesar landed somewhere near Hurst, which is in Romney Marsh, about two miles and a half west of Lympne, affirms

Hasted's statement is quite incorrect. Elliott, who knew every inch of Romney Marsh, positively affirms that 'between Lymne and Appledore . . . not the slightest trace of any river remains'; [1] and his statement is confirmed by Topley.[2] Dowker [3] also observes that if the Rother had ever flowed out near Hythe, 'it must have occupied the space where the Military Canal exists, in which case it has left no historical or other trace behind, and against such a river the Ree Wall could have been no protection.' Moreover, if there is any force in the argument of Drew, the river flowed south of the blown sand near Butter's Bridge, that is to say, a good mile from the hills.[4] Elliott accounts for the belief that the river entered the sea near Lympne by the fact that a depression exists along the foot of the hills, 'many taking that to be the river which in truth was only an estuary . . . and which would only assume something of the character of a river at low water.' [5] In reply to Beale Poste, it is sufficient to

(*Journal of Philology*, xvii, 1888, pp. 176–7, n. 1) that, in A.D. 893, 'Hastings the pirate came here with his fleet . . . and sailed four miles up the Rother to the Weald.' There is not the slightest evidence that 'Hastings' came 'here' with his fleet. The record of his expedition is in the *Anglo-Saxon Chronicle* (ed. B. Thorpe, ii, 1861, p. 69). 'In this year' [893], says the chronicler, 'the great army, of which we long before spoke . . . came up to the mouth of the Limen with two hundred and fifty ships. The mouth is in the east of Kent, at the east end of the great wood which we call Andred . . . The river, of which we before spoke, flows out from the weald. On the river they towed up their ships as far as the weald, four miles from the outward mouth, and there stormed a work.' Mr. Malden (*op. cit.*, p. 176, note) avows his belief that 'the Romans embanked the marsh', and immediately afterwards says that 'the *Portus Lemanis* after that became accessible only from the east, inside the shingle spit opposite Hythe'. It would appear, then, that, according to Mr. Malden, the mouth of the Limen, up which the Danes sailed, was 'opposite Hythe'. But, according to the *Anglo-Saxon Chronicle*, to which he refers, it was at Appledore (see p. 542, n. 4, *infra*); and doubtless the Danes reached it by sailing up the channel, formed by the Rhee wall (see p. 538, *supra*), which then connected the Limen with the sea.

[1] C. Roach Smith, *Report on Excavations . . . at Lymne*, pp. 39–40.

[2] *Proc. Geologists' Association*, viii, 1883, p. 93. Topley, indeed, frankly admits that one argument may be adduced in support of the theory that the Rother flowed out opposite Lympne. This argument is identical with that of Drew, which I have quoted in the text; but, as Topley's exposition is the more lucid, I give it here. He observes (*Mem. Geol. Survey,—The Geology of the Weald*, pp. 303–4) that on Romney Marsh the shingle 'has chiefly accumulated to the *windward* of tidal harbours, whilst the blown sand has accumulated to leeward of those harbours'; and then, remarking that, on the south of West Hythe, the 'fulls', or ridges of shingle, 'curve well round to the north-west, as though to a harbour here,' and that 'on the north of this there is again a little blown-sand', he admits that these facts lend some support to the popular view: but, he adds, 'no trace of the ancient channel is to be found along the northern side of the marsh.' But Topley seems not to know his own mind; for he afterwards says (*ib.*, p. 304) that 'it is by no means unlikely that the ancient Rother had more than one mouth. There may have been one at Lympne, one at Romney, and one near Rye.' However, in his final utterance on the subject (*Proc. Geologists' Association*, viii, 1883, p. 93) he says, 'there is no evidence of any old river along the northern side of the Marsh.'

[3] *Twenty-third Report East Kent Nat. Hist. Soc.*, 1881, p. 66. See also *Proc. Geologists' Association*, xv, 1898, pp. 216–7.

[4] Cf. John Harris, *Hist. of Kent*, 1719, p. 366.

[5] C. Roach Smith, *Report on Excavations . . . at Lymne*, p. 42.

remark that the *Itinerary* does *not* say that the port of the river Lemanis (or rather Lemana) was the Portus Lemanis, nor does it even mention the river: it simply gives the distance of the Portus Lemanis from Durovernum, or Canterbury.[1] Beale Poste misquotes Somner, who does not say a single word about 'the Lymne branch of the Rother'.[2] It is quite true that we find the words *portu Limneo* in the Chronicle of Ethelwerd;[3] but it is not easy to see how these words convey any more information about the geographical position of the port than the words *portus Lemanis*. As to Holloway's argument, all that Lambarde[4] says is that Robert Talbot,[5] 'a man of our time,' was of opinion that Shipway, near West Hythe, was so called 'because it lay in the way to the Haven where the ships were woont to ride.[6] And that haven,' adds Lambarde, 'taketh hee to be the same which . . . is called . . . of Antoninus *Limanis*, of our chroniclers Limene Mouth, and interpreted by Leland to betoken the mouth of the river of Rother.' The *Anglo-Saxon Chronicle* states distinctly that the mouth of the Limen was at Appledore;[7] and Leland was far too acute to be duped by the notion that it had ever been at Lympne: 'where the Ryver *Limene* should be,' he says, 'I can not tel, except yt should be that that cummeth above Appledor . . . and that ys Cowrs ys now changed.'[8] With regard to Drew's argument, allusion is made in two charters[9] to 'a piece of land at Sandtun, that was bounded on the south by the river Limen', namely, a charter of King Aethilberht of Kent, dated February 20, 732, and a charter of King Ecgberht of Kent, dated 833. In the latter it is stated that there were salt-pans 'in the same place', namely at Sandtun;[10] and in both the boundaries of the land are defined in almost identical terms,—'the boundaries of this piece of land are, on the east the King's land; on the south the river called the Limen; on the west and on the north the Hudan Fleot.'[11] That Sandtun was the patch of blown sand between West Hythe and Botolph's Bridge is a pure assumption on the part of Drew. Furthermore, he would have found it difficult to indicate the position of 'the King's land' on the east, seeing that on the east, if the Limen debouched opposite Lympne, there was only shingle or sea. Finally, it is certain that

[1] *Itin. Ant.*, ed. Wesseling, p. 473.
[2] *A Treatise of the Roman Ports and Forts in Kent*, p. 42.
[3] *Rerum angl. script.*, &c., ed. H. Savile, 1601, p. 846 (Chronicle of Ethelwerd, lib. iv, cap. iii, *s.a.* 893, line 57 ff.).
[4] *Perambulation of Kent* (written in 1570), 1826, p. 165.
[5] *The Itin. of John Leland*, iii, 1744, p. 158.
[6] Mr. G. R. Wright (*Journ. Brit. Archaeol. Association*, xl, 1884, p. 247) suggests that 'Shepway' may have been derived from the Saxon word, *sceap*, 'a sheep', and may have 'meant a sheep-way'.
[7] See p. 542, n. 4, *infra*.
[8] *The Itin. of John Leland*, vii, 1744, p. 132.
[9] J. M. Kemble, *Codex dipl. aevi Saxonici*, i, 1839, pp. 92–3, LXXVII; pp. 308–9, CCXXXIV.
[10] in loco qui dicitur sandtun. et in eodem loco sali coquenda, &c.
[11] termini vero terrae illius hec sunt. ab oriente terra regis. ab austro fluvius qui dicitur limenaee. ab occidente et in septentrione hudan fleot.

before 833 Romney Marsh Proper had been enclosed; and how a river could have flowed along the north of the marsh across the Rhee Wall, or how, if it had worked this miracle, it should have subsequently disappeared without leaving any trace of its existence, is more than I can understand.[1] At all events the level of the marsh, which is 6 feet 6 inches lower at Appledore Dowles than at West Hythe Oaks, proves that, even assuming the former existence of such a river, centuries must have elapsed from the time when it ceased to flow beneath the hills to the time when the shingle closed the marsh at West Hythe Oaks.[2]

Elliott[3] concluded, 'from several careful surveys of the whole district,' that the mouth of the Limen was at Appledore, where it entered the estuary; and, as Roach Smith[4] truly remarks, this conclusion is confirmed by the *Anglo-Saxon Chronicle*. 'We now,' says Elliott,[5] 'find the whole country about the mouth of the Limene, at Appledore, in a circuit of about a mile (and at no other part), at a few feet under the present surface, covered with trees of the oak, alder, and birch . . . evidently, from their position, having been drifted from a distance, and deposited where now found.' Lewin[6]

[1] Lewin (*Archaeologia*, xl, 1866, pp. 373–4) admits that the earlier of the two charters mentioned in the text 'appeared at first sight to negative the hypothesis that the marsh was under cultivation in the time of the Romans': but he adds that he consulted Elliott, who removed his doubts in the following letter:—'The grant refers to *Romney* and not to *Lymne*. The boundaries will do for Romney, but not for Lymne. If at Lymne, the salt-pans must have been in the marsh, and then on the east, south, and west would have been the sea, and on the north Lymne Hill. At Romney . . . the description agrees. Sandtun would be the Sand hills, called the Warren, to the east of Romney, and the boundaries of the land would be as stated, viz.:—the King's land on the east would be the territory to the east, about 100 acres, which was vested in the Crown until the reign of Elizabeth, when it was granted to Romney Corporation; the river on the south would be the Limen . . . Hudanfleot, referred to as on the north and west, would be the fleet which may still be traced there, though it has lost its name,' &c. Lewin (*The Invasion of Britain*, &c., 1862, p. lvi) remarks that in the neighbourhood of Romney 'are still pools of stagnant water . . . called Fleets'. As, however, the mouth of the Limen, in A.D. 893, according to the *Anglo-Saxon Chronicle* (see p. 541, *supra*) was at Appledore, we must assume that the Limen mentioned in the charter was simply the body of water conducted into the channel enclosed within the embankments of the Rhee Wall.

That 'the marsh was under cultivation [or, at all events, occupation] in the time of the Romans', is not a 'hypothesis' at all: it is a fact attested by the discovery of numerous Roman remains. See p. 551, *infra*.

Professor Montagu Burrows (*The Cinque Ports*, p. 12) speaks of 'Hudanfleot, afterwards called West Hythe', and says (*ib.*, p. 50) that 'Hudanfleot' means 'the haven of the estuary'. Needless to say, he gives no authority; and how 'the haven of the estuary' could have been both 'on the west and on the north' of 'the piece of land' referred to in the charters he does not explain.

[2] See pp. 545–6, *infra*.

[3] See Roach Smith, *Report on Excavations . . . at Lymne*, pp. 39–40.

[4] *Ant. of Richborough*, &c., pp. 236, 239. See also J. M. Kemble, *Codex diplo. aevi Saxonici*, i, 103, No. LXXXVI. The *Anglo-Saxon Chronicle* (ed. Thorpe, ii, 71, *s.a.* DCCC.XCIV) mentions 'the great army . . . which had before sat at the mouth of the Limen, at Appledore'.

[5] Roach Smith, *Report on Excavations . . . at Lymne*, p. 41.

[6] *The Invasion of Britain*, &c., 1862, p. lxiii; *Archaeologia*, xl, 1866, p. 369.

points out that this 'is the very lowest part of the marsh'; and he holds that 'the presence of oak trees . . . decides that the trees are not *in situ*,[1] for . . . there is something in the Marsh mould uncongenial to the oak'. The course of the river, Elliott tells us, is 'still traceable between Appledore and the Isle of Oxney, and thence into the estuary, about half a mile south of Appledore'. Once, as we have seen, according to Elliott, the estuary found an exit opposite Lympne: when this was closed, there remained only the channel between Romney and Lydd.[2]

4. It is now necessary to inquire what was the geographical position of the Portus Lemanis. The reader will, of course, see that this question is quite distinct from that which he has just been considering. Whether the Rother ever flowed along the north of the marsh or not, everybody admits that the sea once had access there even at low tide; and the question is whether the Portus Lemanis was this estuary, or rather that part of it which lay below Lympne Hill. This is the generally accepted view.[3] In support of it Appach[4] argues as follows:—First, the name 'Lympne' is obviously a corruption of *Lemanis*, and Leland found a tradition existing that Lympne had once been a port. Secondly, at Lympne, Stone Street, the Roman road from Canterbury, 'terminates abruptly,' and 'no trace whatever of its continuance southward into the marsh can be discovered'. 'For what reason,' asks Appach, 'could this road have

I have remarked elsewhere (pp. 609, and 622-3) on Lewin's inconsistencies. In his final utterance on the subject of Romney Marsh (*Archaeologia*, xl) he outdoes himself. On page 369 he says that the mouth of the Limen was at Appledore: on page 370 he says that 'the river Limen must have flowed along the foot of the hills, and have discharged itself at Lymne'.

[1] Drew (*Mem. Geol. Survey,—The Geology of the Country between Folkestone and Rye*, pp. 19-20), on the other hand, says that 'Forest trees flourished on this surface, for the moor-logs in the peat have all the appearance of having grown on the spot. If this be so, it follows that since that time there has been a depression of the land, because the peat that occurs at Appledore, and along the shore between Rye and Dungeness . . . is at too low a level for the plants to have grown at these places while the sea had access there . . . There is no reason to believe that any of the depression of land took place . . . from the time of the Romans downwards, for no human remains nor works of art have been found deep in the Alluvium.' Dowker (*Proc. Geologists' Association*, xv, 1898, p. 221) argues, in support of Drew's opinion, that if the trees had been carried down by the Rother, 'we should expect them to have been covered with mud or silt, which does not occur to any extent.'

[2] See p. 535, *supra*.

[3] See M. Burrows, *The Cinque Ports*, p. 11; *Archaeol. Journal*, liii, 1896, pp. 364-5; F. Haverfield (*Hist. Atlas of Modern Europe*, ed. R. L. Poole, 1896, pl. 15), &c. [Prof. Haverfield calls the harbour Portus Lemanae, not Portus Lemanis.] Stukeley, however (*Itin. curiosum*, 1776, p. 133), believed that the Portus Lemanis was 'about West Hithe'; and Somner (*Treatise of the Roman Ports and Forts in Kent*, p. 37) says that some of 'our English Chorographers' were of the same opinion. So also was the famous geographer, Konrad Mannert (*Geogr. der Griechen und Römer*, Zweyter Theil, Zweyter Heft, 1795, p. 161). Somner (p. 38) argued that the port was at New Romney; but in order to sustain this opinion he was forced to read *XXI* instead of *XVI* (Roman miles), —the distance, according to the *Itinerary* of Antonine (ed. Wesseling, p. 473) from Durovernum (Canterbury) to Portus Lemanis.

[4] *C. J. Caesar's Brit. Expeditions*, &c., pp. 43-5, §§ 3-10.

been made if Lympne was not then a port?' He goes on to observe that, according to the *Itinerary* of Antonine, 'Portus Lemanis was one stage distant from Canterbury;' that, besides Stone Street, the only Roman roads which converged at Canterbury were those which led to Reculver, Richborough, and Dover; and therefore that the Portus Lemanis must have been situated on Stone Street, and obviously at its termination. Thirdly, according to the *Itinerary*, the distance from Canterbury to the Portus Lemanis was 16 Roman miles, or about 25,872 yards;[1] and the actual distance from 'the margin of the marsh below Lympne measured along the Stone Street to the point where all the Roman roads at Canterbury would converge, if produced, is fifteen statute miles', or 26,400 yards.[2] Fourthly, the existence of Stutfall Castle proves that the Portus Lemanis was at Lympne; and, moreover, the castle 'had no southern wall because the sea came up to the foot of the fortifications'. Fifthly, in the *Table* of *Peutinger*, Lemanis is 'marked with a castle, like Richborough and Dover'.

These arguments may, at first sight, appear conclusive: in reality they are worthless. (1) Leland[3] does not mention any tradition about the port: he simply asserts that 'Lymme Hill or Lyme was sumtyme a famose Haven, and good for Shyppes that might cum to the Foote of the Hille'. Lambarde,[4] it is true, says that there was in his time a tradition that Shipway was so called because 'it lay in the way to the Haven where the ships were woont to ride'; and he calls this tradition 'the report of the countrie people, who hold faste the same opinion which they have by tradition receaved from their Elders'. Also he himself asserts that 'at the first, ships were accustomed to discharge at Lymme'. But Shipway 'lay in the way' to West Hythe, not to Lympne. As for the alleged tradition, everything depends upon the date of its origin; and this cannot be ascertained. The name 'Lympne' may be connected with *Lemanis*; but this does not prove that the Portus Lemanis was at the foot of the heights on which Lympne stands: if it had been east of Stutfall Castle, and the nearest town in Roman times or later had been on the site of Lympne, the origin of the name would be perfectly clear. (2) Appach insists that Stone Street 'terminates abruptly' at Lympne; but, as a matter of fact, a road diverges to the right from the straight course of Stone Street at New Inn Green, and terminates just north of Stutfall Castle.[5] Mr. Thurston of Ashford points out that if the course of Stone Street were continued in a straight line from New Inn Green, it 'would point to the Shipway [or Shepway] Cross, and continue down the present roadway which descends the hill to West Hythe; and', he adds, 'this is the only place along the hill where a roadway could possibly descend it in a straight line, and I believe it

[1] 26,080, according to Appach; but he assumed that a Roman mile was equal to 1,630 yards, whereas it was really 1,617. Cf. Smith's *Dict. of Greek and Rom. Ant.*, 3rd ed., ii, 159–60.

[2] According to Appach, whose arithmetic was a little shaky, 25,840.

[3] *Itin.*, vii, 1744, p. 132.

[4] *Perambulation of Kent*, p. 165.

[5] *Ordnance Survey of England*, Sheet 289.

was naturally selected as the road to the ships or port.'[1] (3) As for the argument based upon the distance given in the *Itinerary* from Canterbury to the Portus Lemanis, a moment's reflection will convince any reader who uses his map that it holds good for the theory that the Portus Lemanis was at West Hythe as well as for the view which Appach defends. (4) The situation of Stutfall Castle may no doubt be used as an argument to prove that the Portus Lemanis was at Lympne: but the castle is barely a mile and a half from West Hythe Oaks, which, as we shall presently see, was in all probability the western end of the port; and, although it was believed when Appach wrote that the castle had no southern wall, excavation has since proved that it had.[2] Appach's last argument depends, like the one which precedes it, upon the assumption that Stutfall Castle would have been useless unless it had stood in *immediate proximity* to the Portus Lemanis. What if Lemanis was 'marked with a castle'? Why should not the castle have protected the neighbouring part of 'the Saxon shore' and a harbour at West Hythe?

The late antiquary, W. H. Black,[3] remarked further, that the discovery of a Roman altar in Stutfall Castle, erected by the 'admiral of the British fleet' (*praefectus classis Britannicae*), proves that the Portus Lemanis was at Lympne; and, observing that 'the Saxon Chronicle tells us of the arrival of a fleet of Danes at "Limene mouth"', he argues that 'it is impossible to deny the identity of Lymne with that name'. But, whatever may be the etymological connexion between *Lympne* and *Limene*, it has been shown already that according to the very chronicle which Black cites, the mouth of the Limen was at Appledore;[4] and the discovery of the Roman altar is perfectly consistent with the view that the harbour which was the admiral's naval base was near West Hythe.

Elliott originally held that the Portus Lemanis was the estuary at Lympne;[5] and his opinion was quoted by superficial writers in support of this view several years after he had himself discarded it. For he finally came to the conclusion that, even as early as Caesar's time, there was no harbour at Lympne.[6] He tells us that 'recent investigations in taking a series of levels over the whole of Romney Marsh have established the fact that the estuary must have been closed at the eastern extremity (where the Portus Lemanis is commonly looked for) many centuries before the sea was shut out from ... Romney Marsh Proper; for at the extreme eastern end of Romney Marsh, by Hythe Oaks, the surface of the land is 18 inches higher than it is a mile westward, a state of things that could not have existed had there been any outlet towards the east after the closing of the Marsh westward. The inset and outset of the tides twice a day to and from the estuary would have counteracted the silting,

[1] C. Roach Smith, *Ant. of Richborough*, &c., p. 255, n. 1.
[2] *Athenæum*, Sept. 22, 1894, p. 394.
[3] *Archaeologia*, xl, 1866, p. 377.
[4] See p. 542, n. 4, *supra*.
[5] C. Roach Smith, *Report on Excavations ... at Lymne*, pp. 39–45.
[6] T. Lewin, *The Invasion of Britain*, &c., 1862, p. cviii.

and produced not an elevation, but a depression of the surface. There is . . . a regular and continuous fall of the land next the hills, from Hythe Oaks into Appledore Dowles . . . the lowest part of the Marsh being 6 feet 6 inches lower than the land at Hythe Oaks. There could have been no silting after the inclosure of the Marsh, and the present level is such as it was when the Marsh was reclaimed . . . The barrier which sealed up the eastern mouth of the estuary was the accumulation of shingle from the west, and (*sic*) which long before the historic period had reached the hills at Hythe Oaks. If Romney Marsh, at the foot of the castrum [Stutfall Castle], was dry land at that time [A.D. 368–9, when Theodosius [1] was in Britain] and occupied by the Romans (as we know to have been the case), Stuttfall could not have been the "Portus Lemanis" . . . as it was not accessible from the sea, and lay a mile and a half at least from it. The sea could not have flowed there without putting the whole of Romney Marsh Proper under water to the depth of eight or ten feet every springtide.' Similarly, Lewin [2] states, on the authority of Elliott, that 'the greater elevation of the soil towards the east of Romney Marsh Proper can be only accounted for by the fact that when the shingle "full" had been thrown quite across the Marsh at West Oaks . . . the sea still entered from the west, and that, thenceforth, the process of silting went on for many centuries . . . most rapidly towards the east, where the water was tranquil, and less rapidly towards the [site of the subsequently erected] Rhee Wall, in which direction was the scour of the current'.

'Many centuries' is a vague expression; but for 'many' substitute 'three', and, even for the time of Caesar, the argument still holds good,—unless Elliott's theory of the formation of the marsh is to be rejected.

But there are writers whom Elliott's reasoning (if indeed they have considered it) leaves unconvinced. According to Mr. George E. Fox, it has been proved by excavation that the existing *castellum* at Stutfall is not earlier than the time of Constantine; [3] but Sir Victor Horsley, while confirming this statement, tells us that he has himself found 'in the foundation of the chief gate an altar . . . marked with barnacles, having been clearly at one time under the sea'; and from this he infers that an earlier fort was 'overwhelmed by an incursion of the sea over Romney level'. Sir Victor also tells us that he has found 'in the concrete boulder formation of the south wall . . . a coin of Maximinus, who flourished 237 A.D.', and 'at the foot of the wall on the inner side, a Gaulish coin of Tetricus the elder, of a date about 260, and finally in the black soil of the camp, i.e. in the most recent and superficial layers, numerous coins of the Constantine family'.

[1] This Theodosius was not Theodosius the Younger, as Elliott says, but the father of Theodosius the Great.

[2] *The Invasion of Britain*, &c., 1862, pp. lvi–lvii.

[3] *Archaeol. Journal*, liii, 1896, p. 370.

[4] *Proc. Roy. Inst. of Great Britain*, xvi, 1900, pp. 36–7. Cf. C. Roach Smith, *Collectanea Antiqua*, vii, 1880, pp. 158–9, and *Corpus inscr. Lat.*, vii, 18.

I do not know whether Sir Victor Horsley concludes from these discoveries that there was a harbour at Lympne when the earlier hypothetical *castellum* at Stutfall was destroyed; but at all events that is the opinion of Mr. Fox. But the 'incursion of the sea' which Sir Victor Horsley believes to have overwhelmed the original fort, if it was not caused by an abnormally high tide rushing in between Romney and Lydd before the erection of the Rhee Wall, may have been due to a similar tide which burst the bar of shingle between Dymchurch and West Hythe. Even after the marsh had been artificially enclosed, such floods occurred. Stukeley[1] tells us that 'George Hunt, an old man, living in the farm-house . . . says, once the sea-bank broke, and his house with all the adjacent marshes was floated',[2] &c.

Lewin maintained that the Portus Lemanis was neither at Lympne nor at West Hythe, but at Hythe. This, it should be noted, was the conclusion at which he finally arrived:[3] when he wrote his book on the invasion of Julius Caesar, he held that in 55 B.C. there was a port at Lympne, although in the Appendix to that book he discarded this view, and argued that the only port was a pool harbour extending behind a shingle spit from West Hythe Oaks to a point opposite Shorncliffe. His final view, as we have already seen,[4] was that this harbour extended no further westward than Hythe itself: but in giving utterance to this opinion he did not explain why he had abandoned the one which preceded it, and indeed made no allusion to it at all.

He states that 'in the course of ages', after the shingle had reached West Hythe Oaks, it 'was again wrested aside and dashed against the hills at Hythe, between the present barracks and the more eastern of the two Hythe bridges over the canal'. He goes on to say that 'the part between Hythe Oaks and Hythe (now Duck Marsh) was thus barred from the sea, and became a lake into which flowed the rivulet called Slabrook and other springs, and these waters accumulating forced their way back at Hythe Oaks, and there opened a way for themselves . . . into the estuary in the west; but, as the flood was not considerable, the outlet was of no great breadth. The shingle spit . . . was again carried along eastward until it reached Shorncliff . . . Between Hythe and Shorncliff, however, was left behind (i.e. north of) the spit, a triangular space, into which flowed two streams . . . one from Saltwood and the other called Seabrook, and the waters within this spit were gradually swollen, until they forced a passage through the shingle, at a point near the end of the elm avenue at

[1] *Itin. curiosum*, 1776, pp. 132–3.

[2] See also T. Lewin, *The Invasion of Britain*, &c., 1862, p. lxvii. In his article in *Archaeologia* (xl, 1866, pp. 364–5) Lewin argues that if the Portus Lemanis had been at the foot of Lympne Hill, 'we should expect to find at least some vestiges, however faint, of the port itself'; but, he adds, 'I have never heard or read (though I have often inquired) that any remnant of a pier or sunken vessel, or even any anchor or other part of a ship's tackle was ever discovered in this part.'

[3] *Archaeologia*, xl, 1866, pp. 361–74.

[4] See p. 536, *supra*.

Hythe.' The change which his opinion underwent will be at once apparent to any one who compares the map which Elliott constructed for *The Invasion of Britain by Julius Caesar* (facing page liii) with that which accompanies the article in the fortieth volume of *Archaeologia*[1] (facing page 369). Lewin argues that it was so easy to exclude the sea from Duck Marsh that 'probably the inclosure was made by the Britons before the arrival of the Romans. On the south-east,' he explains, 'the shingle bank was continuous up to the hills . . . on the west the sea entered only from the marsh at the foot of the hills by a narrow channel; and all that was required was a short dam at this point between the shingle bed and the hills.' The remains of this dam, Lewin observes, are 'still distinguishable . . . at Hythe Oaks, but the part next the hills has been swept away by the military canal. This partial inclosure, prior to the inclosure of Romney Marsh, accounts for a fact otherwise inexplicable, viz. that Duck Marsh is not within the jurisdiction of Romney Marsh.'[2]

Perhaps. But the date of the construction of the dam is not known. May it not have been made after, or simultaneously with, the erection of the Rhee Wall, to secure Romney Marsh against all possibility of inundation, not to protect Duck Marsh, which, according to Lewin's earlier view, was originally overflowed by Hythe harbour? In other words, is it not possible that when the dam was made Hythe harbour extended westward as far as West Hythe Oaks? This, as I have already said, was not merely Lewin's original view: it was also the view which Elliott, his friend and adviser, retained *after* the publication of the article in *Archaeologia*. At all events this view finds expression in a map which Elliott prepared for Furley's *History of the Weald of Kent*, which was not published until 1871, five years after the appearance of Lewin's article. That being the case, and considering that Lewin did not explain the reasons which led him to change his opinion, I am unable to follow him.

In support of the theory that the Portus Lemanis was at Hythe Lewin argues, first, that Stone Street terminated at West Hythe; secondly, that the port could not have been at West Hythe; otherwise '*the whole of West Hythe . . . would have been deluged*'. 'The very name,' he adds, 'shows that Hythe was the principal town, and West Hythe an accretion to it.' Thirdly, he affirms that Roman remains have been found at Hythe; and, fourthly, that a branch from Stone Street led to Hythe. He also bases an argument upon the itinerary of Richard of Cirencester, which, as every scholar now knows, is a forgery.[3]

Stone Street does terminate, as Lewin says, at West Hythe; but the fact goes to prove that it gave access to a harbour which was at West Hythe.[4] Granting that West Hythe would have been 'deluged'

[1] Both these maps are reproduced, in part, in the map which faces p. 531, of this book. [2] *Archaeologia*, xl, 1866, pp. 371–2.

[3] *Ib.*, pp. 360–7. I omit those arguments by which Lewin endeavours to prove that the Portus Lemanis was not at Lympne.

[4] Cf. E. Guest, *Origines Celticae*, ii, 116–7, 358.

if the port had been there, what then ? Why should it not have been ? Lewin does not explain what he means by ' the whole of West Hythe '; and, in default of this explanation, it is impossible to understand his argument.[1] He himself, as we have seen, in his book on the invasion of Britain by Julius Caesar makes the port extend westward as far as West Hythe Oaks ; and Black shows that, so far from its being true that West Hythe is merely an ' accretion ' of Hythe, Hythe is merely East Hythe, and that it is so called in Ogilby's *Britannia*.[2] The discovery of Roman remains at Hythe does not prove that Hythe was the Portus Lemanis any more than the discovery of Roman remains at Dymchurch proves that the Portus Lemanis was there. Or rather, the discovery does not prove that the Portus Lemanis extended no further westward than Hythe ; for I freely admit that it extended in front of and to the east of it. It is not proved that a branch from Stone Street led to Hythe ;[3] and if there was such a branch, the fact does not prove that the harbour did not extend as far as West Hythe Oaks. Finally, Black points out that, whereas the distance of Lympne (and, he might have added, of West Hythe) from Canterbury corresponds with that of the Portus Lemanis from Durovernum, as given in the *Itinerary* of Antonine, the distance of Hythe by road from the same place is two miles further.[4]

5. The first step taken for the enclosure of Romney Marsh was the erection of the Rhee Wall. By whom and at what date this work was executed is not certainly known. It is generally attributed to the Romans ; but Lewin[5] assures us that Mr. Smiles, in his *Lives of the Engineers*, ' expresses an opinion that the Marsh was reclaimed by the Belgae.' What Mr. Smiles[6] really says is that ' the reclamation of this tract is supposed to be due to the Frisians ' ; and he does not tell us by whom the supposition is entertained, or on what grounds it is based. Lewin himself, asking whether [Appledore] ' Dowles ' is not

[1] As far as I can see, if the western end of the port had been at West Hythe, the ' deluged ' area would have been that between West Hythe Oaks and Hythe, which in the map prepared by Elliott for Lewin's book (*The Invasion of Britain*, &c., 1862, p. liii) is depicted as the western arm of the harbour, but which in the map that accompanies Lewin's article on the *Portus Lemanis* (*Archaeologia*, xl, 1866, p. 369) is represented as covered partly by the ' Duck Marsh ' and partly by shingle.

[2] Ogilby (*Britannia*, 1675, p. 40) speaks of ' *Hith alias Hide* or *East-Hith* '.

[3] Lewin refers, in support of his statement, to Harris's *Hist. of Kent*, p. 367 ; but what Harris says is simply this :—' that the present *Hythe* was used as a Port, even before the Departure of the Romans . . . Dr. *Plott* thinks reasonable to conclude ; from the paved Way made after the *Roman* Fashion all along up the Hill, not only to Saltwood Castle . . . but a Mile farther onwards, and leading into the *Stonestreetway*.'

[4] Does *Portus Lemanis* mean ' the port at the lagoons ' (E. Guest, *Origines Celticae*, ii, 117), the plural having been used because, while on the east of Hythe Oaks extended the pool harbour, the marsh was still flooded on the west before the erection of the Rhee Wall ? That *Lemanis* or *Lemannis* is not a nominative, but a locative plural, seems to be shown by the *Notitia dignitatum* (ed. O. Seeck, 1876, Oc. xxviii, 5), where *Lemannis castellum* is mentioned side by side with *Regulbi castellum*.

[5] *The Invasion of Britain*, &c., 1862, p. lxii, note.

[6] *Lives of the Engineers*, i, 1861, p. 7.

derived from the Celtic word *dol*, says that 'if a part of Romney Marsh was named by the Ancient Britons, the marsh itself must have been reclaimed by them'.[1] From the same word Appach[2] draws precisely the opposite inference. 'Apuldore Dowles,' he says, 'appears to be allied to the Welsh *dol*, a bend. If so, it would mean a bend or curve, and so a recess or bay; and Apuldore Dowles would mean the bay of Apuldore.' Whatever may be the value of this argument, the name 'Apuldore Dowles,' does not go to prove that Romney Marsh was 'inned' by the Britons; for, as Appach[3] truly remarks, there is no other local name in Romney Marsh Proper which shows any trace of a Celtic derivation.

Mr. W. A. S. Robertson,[4] on the other hand, states, on the authority of Professor Skeat, that 'Rumenea', the name by which, according to Lambarde,[5] Romney was known to the Saxons, is compounded of the Gaelic word *ruimen* (marsh) and the Saxon affix *ea* (river); and he concludes that 'before the Roman occupation there was in this great estuary sufficient land, uncovered by water, to be denominated . . . *Rum* or *Ruimen*'. Again, arguing that the καινὸς λιμήν, or 'new harbour', mentioned by Ptolemy,[6] was at Romney, he says that 'if it was called into existence by . . . the Rhee Wall, it follows that the Rhee Wall' was 'probably formed at least as early as the first century of the Christian era'.

If the 'new harbour' was at Romney! There is not the slightest evidence that it was there.[7] As for the word *ruimen*, how can Mr. Robertson prove that it was applied to Romney Marsh 'before the Roman occupation'? Moreover, supposing that the marsh was not embanked by the Britons, there was 'sufficient land uncovered by water to be denominated *Ruimen*' twice every day, when the tide was low, before the Rhee Wall was made; and the name lends no support to Mr. Robertson's theory.

I do not attach much importance to the argument, first propounded by Sir W. Dugdale[8] and often repeated since, that because the Britons, according to Tacitus[9]—or rather, according to a speech put by Tacitus into the mouth of a British chief—were employed by the Romans in draining and embanking marshes, therefore the Romans enclosed this particular marsh. But, considering that Roman remains have frequently been discovered in that part of the marsh which lies on the east of the Rhee Wall,[10] it is surely inexplicable that if the wall was built by the Britons, no Celtic remains have ever been found there.

[1] *Archaeologia*, xl, 1866, p. 369, note *b*.
[2] *C. J. Caesar's Brit. Expeditions*, p. 13, § 6.
[3] *Ib.*, p. 12, § 6.
[4] *Kentish Archaeology*, iv, 1880, p. 13.
[5] *Perambulation of Kent*, p. 208.
[6] *Geogr.*, ii, 3, § 3.
[7] See Mr. H. Bradley's article in *Archaeologia*, xlviii, 1885, pp. 379–82, 389.
[8] *Hist. of Imbanking and Drayning*, &c., 1662, pp. 16–7.
[9] *Agricola*, 31,—corpora ipsa ac manus silvis ac paludibus emuniendis inter verbera ac contumelias conteruntur.
[10] R. Furley, *Hist. of the Weald of Kent*, i, 29.

Appach[1] not only rejects the theory that the Britons built the Rhee Wall, but denies that Romney Marsh Proper was enclosed during the Roman occupation. He maintains that, in Caesar's time, 'the northern portion, at all events, and possibly the whole of the interval between the island of Romney and the high ground of Kent was open sea.' For, he argues, 'Lympne was the ancient Portus Lemanis . . . that place could not have been a port unless there had been free access to it from the Channel, and it is clear from the manner in which the marsh and shingle were deposited, that there was always open sea between Lympne and the Channel until the interval between the ancient island at Romney and the high ground of Kent had been closed by the gradual growth of the marsh and shingle.'

The assumption upon which this argument rests has been already disproved: the Portus Lemanis was not at Lympne. Appach's theory forces him to assume that the sediment which formed the marsh was deposited at an incredibly rapid rate. He maintains[2] that 'the upper portion of Romney Marsh, for a depth of thirty feet . . . below its present surface (which would give sufficient water for the heaviest of Caesar's ships at the lowest Spring tides) might very well have been deposited' in 'about five hundred years'. But, according to Elliott,[3] the average rate at which the silt was deposited was not more than about one-eighth of an inch *per annum*.

Dowker, on the other hand, although he once regarded it as 'evident that at the period of Caesar's invasion the marsh was little better than a swamp, great part being under water at high tide', maintained that the discovery of Roman pottery on the west of Dymchurch disproved Appach's theory.[4] But he did not take account of dates. Appach himself[5] noted the discoveries which had been made near Dymchurch; but he observed that while some of the objects discovered had been pronounced by the Society of Antiquaries to be 'decidedly Roman', others had been attributed by the same body to subsequent periods; and he concluded that the marsh had not been enclosed before the middle of the fifth century.

This theory is pulverized by one fact which Appach ignores. Dymchurch is not the only place in Romney Marsh Proper where Roman remains have been found: they have been discovered in Eastbridge, at Newchurch, at Ivychurch, and indeed over the whole

[1] *C. J. Caesar's Brit. Expeditions*, pp. 42–3.

[2] *Ib.*, pp. 137–8, § 11.

[3] *Archaeologia*, xl, 1866, pp. 367–8.

[4] *Archaeol. Journal*, xxxiii, 1876, pp. 60, 63. Cf. Roach Smith, *Ant. of Richborough*, &c., p. 245; *Journ. Brit. Archaeol. Association*, i, 1845, pp. 40–2; and A. J. Dunkin, *Report of the . . . Brit. Archaeol. Association*, Sept., 1844, pp. 116–9. Besides pottery, many human skeletons, and also tusks of boars and horses' teeth were discovered. Roach Smith (*Retrospections*, i, 1883, p. 207) concludes from these discoveries that the marsh 'could not possibly have been submerged in the time of the Romans'. Not, certainly, at the time when the articles in question were deposited there: but why not before? 'The time of the Romans' amounted to nearly four centuries.

[5] *C. J. Caesar's Brit. Expeditions*, p. 136, § 9.

area.[1] On the other hand, Welland Marsh, Guildford Marsh, and Denge Marsh—those parts of Romney Marsh, popularly so called, which extend westward of the Rhee Wall—have yielded none.[2] The inference is certain: Romney Marsh Proper was enclosed during the Roman occupation of Britain.

6. The conclusions which we have now reached are, first, that the Rother did not, in the time of Caesar, enter the sea at Lympne, but debouched into the estuary near Appledore; secondly, that the marsh was then closed at West Hythe Oaks, and therefore that there was no harbour at Lympne; thirdly, that the Rhee Wall had not then been built, and therefore that the marsh was still flooded at spring tides by the inrush of the sea between Romney and Lydd; fourthly, that the Portus Lemanis was a pool harbour extending from West Hythe to a point nearly opposite Shorncliffe; and, lastly, that the Rhee Wall was built in Roman times.

But, as the reader will hereafter see, if these conclusions are erroneous, the error will not lead us astray when we have to determine the place where Caesar landed in Britain.

PORTUS ITIUS

I. REVIEW OF THE CONTROVERSY

The greater part of the vast literature which has accumulated on the question of the identity of the Portus Itius is obsolete;[3] and it is now sometimes taken for granted that the choice is restricted to Wissant and Boulogne. Nevertheless, as I am determined to set the question at rest, I shall examine the claims of three other ports, which, in recent times, have found advocates whose names command respect,—the estuary of the Somme, Ambleteuse, and Calais.

The question began to be seriously discussed in the fifteenth century. The Italian geographer, Raymond de Marliano, identified the Portus Itius with Calais;[4] and in the following century the famous Ortelius[5] did the same. Chifflet[6] and other scholars, well

[1] *Archaeologia*, xl, 1866, p. 372. According to Elliott (*ib.*, p. 365), a coin of Carausius, who ruled in Britain from A.D. 287 to 293, was found near Dymchurch.

[2] R. Furley, *Hist. of the Weald of Kent*, i, 29. Against these facts Appach's argument (*C. J. Caesar's Brit. Expeditions*, p. 134, § 3) that if Romney Marsh had existed 'in the earlier period of the Roman settlement' Stone Street, assuming that it existed, 'would have been carried onward to Romney, the seaport,' is of no avail. There is no evidence that Romney was 'the seaport' until long after the departure of the Romans.

[3] See A. E. E. Desjardins, *Géogr. de la Gaule rom.*, i, 348–50, and footnotes.

[4] *Veterum Galliae locorum ... descriptio* (printed in *C. Iulii Caesaris ... comm.*, Lutetiae, 1544), s.v. *Itius portus*.

[5] *Thesaurus geogr.*, 1596, s.v. *Iccius*.

[6] *Portus Iccius Iulii Caesaris demonstratus*, 1627.

known in their day, chose St. Omer, situated, as they believed, at the head of a wide and shallow gulf, which was erroneously assumed to have covered the low-lying lands between Sangatte and Dunkirk.[1] Adrien de Valois[2] declared for Étaples; and numerous other absurd suggestions were defended with more or less ingenuity. From Dieppe to Ghent there was not a harbour, a roadstead, or a fishing port, which had not its champion. But the controversy soon began to centre itself between Wissant and Boulogne. Camden[3] was the first to declare for Wissant. Du Fresne[4] (commonly called Du Cange), one of the most illustrious French scholars of the seventeenth century, defended its claims against Sanson; and d'Anville,[5] Henry,[6] Walckenaer,[7] and Sir Richard Colt Hoare[8] followed his example. Cluver[9] wrote briefly but effectively on the other side; and Scaliger[10] characteristically exclaimed that those who did not, like himself, decide for Boulogne, were lunatics. During the last half-century, although Wissant has not lacked able defenders, the case for Boulogne has been tending to prevail. But the arguments of Haigneré, of Napoleon the Third, of Desjardins, and finally of Rudolf Schneider failed to silence opposition. Men so able as George Long, Dr. Guest, Dean Merivale, Dr. Hodgkin, Karl Müller, and Alphonse Wauters remained unconvinced: Freeman[11] roundly asserted that 'since Dr. Guest's exposition of the matter it is hardly necessary to say that "Portus Itius" or "Iccius" is not Boulogne': Professor Ridgeway and Mr. H. E. Malden, in their animated controversy[12] on the question of Caesar's landing-place, agreed in identifying the harbour from which he sailed with Wissant: more recently Dr. Emil Hübner[13] has done the same; and the well-known Caesarian scholar, Professor H. J. Heller, at the close of a pungent criticism[14] of Schneider's dissertation, concluded that the identity of the Portus Itius was still an open question. Mr. H. F. Tozer,[15] indeed, has recently pronounced the question to be insoluble; and Mommsen,[16] who in 1889 still adhered to his old belief, that 'among the many possibilities most may perhaps be said in favour of the view that the Itian port . . . is to be sought near Ambleteuse', nevertheless remained convinced that 'it requires the implicit faith of local

[1] See pp. 517–8, *supra*.
[2] *Notitia Galliarum*, 1675, p. 249.
[3] *Britannia*, ed. R. Gough, 1789, i, 221.
[4] *Dissertatio de Portu Iccio*, 1694.
[5] *Mém. de litt. tirés des registres de l'Acad. Roy. des Inscr. et Belles-Lettres*, xxviii, 1761, pp. 397–409.
[6] *Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, 1810.
[7] *Géogr. des Gaules*, i, 448–57.
[8] Giraldus de Barri, *The Itinerary of Archbishop Baldwin*, 1806, i, lxxix.
[9] *Germania antiqua*, 1631, lib. ii, cap. xxviii, pp. 440–7.
[10] *Notitia Galliae*, 1651, p. 856.
[11] *Norman Conquest*, i, 1870, p. 486, n. 1.
[12] *Journal of Philology*, xvii, 1888, pp. 163–78; xix, 1891, pp. 138–45, 193–9, 200–10; xx, 1892, pp. 63–4.
[13] Pauly's *Real-Encyclopädie*, iii, 1897, p. 864.
[14] *Philologus*, xlix, 1890, p. 693.
[15] *Hist. of Anc. Geogr.*, pp. 230–1.
[16] *Röm. Gesch.*, iii, 1889, pp. 269–70, note (Eng. trans., v, 1894, p. 63, note).

topographers to proceed to the determination of the locality with such data'.

Evidently, then, unless the problem is to be abandoned in despair, there is room for another treatise. But this treatise must justify its existence. I have not 'the implicit faith of local topographers': but there are more data than Mommsen had leisure to examine; and the locality can be determined with absolute certainty.

There is indeed a summary way of dealing with the question which has long since satisfied practical men: doubt is confined to the minds of scholars and of those who look to them for guidance. Men who are familiar with war and who have a sufficient knowledge of the conditions of navigation in the Straits of Dover know that there was only one port on the north-eastern coast of Gaul which would have answered all Caesar's requirements, and that Caesar would not have made a foolish choice. Accordingly the greatest of modern soldiers affirmed without hesitation that the greatest soldier of Rome had sailed to Britain from Boulogne. But this reasoning, perhaps because of its simplicity, has not seemed conclusive to the learned world.

II. THE DATA FURNISHED BY CAESAR, STRABO, AND PTOLEMY

Caesar says that, before his first expedition to Britain, he sent Gaius Volusenus to reconnoitre the British coast and ascertain what harbours were capable of accommodating a large fleet, and that he himself marched with his whole force for the country of the Morini, 'because the shortest passage to Britain was from their country' (*quod inde erat brevissimus in Britanniam traiectus*); and he goes on to say that he ordered ships from the neighbouring districts, and likewise the fleet which he had built in the previous summer for the war with the Veneti, to assemble there. He set sail soon after midnight with about 80 transports, some ships of war, and some small fast-sailing vessels (*speculatoria navigia*) from a port which he does not name, and sent his cavalry to 'a further port' (*in ulteriorem portum*) about 8 Roman miles off, with orders to embark there in eighteen transports, which had been prevented by contrary winds from reaching the port whence he himself sailed, and to follow him. In another chapter he speaks of the *ulterior portus* as *superior portus*; and it is admitted that this port was either north or east of the one from which he himself sailed. On the fourth day after he landed in Britain (the day of landing being doubtless reckoned as the first day [1]) the eighteen transports set sail. They were getting close to Britain and were descried from the Roman camp when a storm suddenly arose, and none of them could keep on its course, but some were carried back to the place from which they had started, that is to say, to the *superior portus*; while the rest were driven down 'in great peril to the lower and more

[1] See p. 602, n. 5, *infra*.

westerly part of the island' (*ad inferiorem partem insulae, quae est propius solis occasum magno suo cum periculo deicerentur*), whence, after anchoring for a time and shipping a quantity of water, they were compelled to stand out to sea, and ran for the Continent. When Caesar returned to Gaul, two of his ships were unable to reach 'the same ports' (*eosdem portus*) as the rest of the fleet, and were carried 'a little further down' (*paulo infra*). Before his second expedition he assembled a fleet of about 540 transports and 28 ships of war at the Portus Itius, 'from which port he had ascertained that the passage to Britain was most convenient,[1] being about 30 [Roman] miles from the Continent' (*quo ex portu commodissimum in Britanniam traiectum esse cognoverat, circiter milium passuum* XXX *transmissum a continenti*). His entire flotilla amounted to more than 800 sail, as it included privately owned vessels. The transports were of small draught and comparatively broad, and were constructed for rowing as well as sailing. Caesar was delayed at the Portus Itius for about 25 days by north-westerly winds.[2] His entire army, which was with him all that time, amounted to 8 legions and 4000 cavalry. He set sail about sunset, accompanied by 5 legions and 2000 cavalry, with a light south-westerly wind, or, to speak more accurately, the wind called *Africus*, which may have blown from any quarter between south-west and W. by S. ½ S. Labienus was left behind with 3 legions and 2000 cavalry 'to protect the ports' (*ut portus tueretur*), which implies that on the second expedition, as on the first, Caesar thought it necessary to keep more than one port under his control; and during his absence Labienus built 60 ships.[3]

Strabo,[4] evidently referring to Caesar's first expedition, says that

[1] It has been argued that *commodissimum* in this passage means not 'most convenient', but simply 'very convenient'. I have not the slightest doubt that the former is the right interpretation, just as in *B. G.*, iv, 21, § 3, *brevissimus* (in Britanniam traiectus) unquestionably means 'the shortest', and not 'a very short' (passage to Britain): but if I were wrong my mistake would be unimportant. It will hardly be denied that if Caesar had found a port from which the passage was more convenient than from the Portus Itius, he would have chosen it. See p. 574, *infra*.

[2] Caignart de Saulcy (*Les campagnes de Jules César dans les Gaules*, 1862, p. 181) infers from this that the Portus Itius must have been so situated that vessels sailing thence for Dover *would have had the north-west wind right in their teeth*; and he remarks that, if Wissant was the Portus Itius, this condition was fulfilled. But it is hardly necessary to say that the condition is imaginary. The Portus Itius must have been so situated that while the north-west wind (or rather the wind called *Corus*, which may have blown from any quarter between N.W. and W. by N. ½ N.) was blowing, Caesar's vessels could not have sailed thence to that part of the Kentish coast which he wished to reach; and it is certain that they could not sail closer than within about seven points of the wind. See *The Voyage and Shipwreck of St. Paul*, by James Smith, 4th ed., 1880, p. 215; and, on the winds as described by various ancient writers, Vitruvius, *De Architectura*, i, 6, §§ 5, 9–10, P. F. J. Gossellin, *Recherches sur la géogr.*, iv, 1813, p. 410, and diagram facing p. 416, and J. Vars, *L'art nautique dans l'ant.*, 1887, pp. 31–4.

[3] *B. G.*, iv, 21, §§ 1–4; 22, §§ 3–4; 23, § 1; 28; 36, § 4; v, 2, §§ 2–3; 5; 7, § 3; 8, §§ 1–2, 6; 23, § 4.

[4] *Geogr.*, iv, 5, § 2.—τὸ Ἰτιον, ᾧ ἐχρήσατο ναυστάθμῳ Καῖσαρ ὁ Θεός, διαίρων εἰς

he sailed from 'the Itian' [naval station ?] (*τὸ Ἴτιον*), and that the length of his voyage to the point which he reached 'about the fourth hour' was 320 stades, which is equivalent to 40 Roman miles.

Ptolemy[1] mentions the Itian promontory. Its longitude, he says, was 22° 15′, and its latitude 53° 30′; and he places it on the west of Gesoriacum, or Boulogne. The longitude of Gesoriacum, he says, was 22° 30′, and its latitude 53° 30′.

III. CAESAR SAILED FROM THE PORTUS ITIUS ON BOTH HIS EXPEDITIONS

It is necessary to inquire whether Caesar sailed from the same port on both his expeditions; for he mentions the *ulterior portus* only in connexion with the first; and if on that occasion he sailed from the Portus Itius, the search for the Portus Itius is conditioned by the existence of the *ulterior portus*. Drumann,[2] remarking that Caesar chose the Portus Itius in 54 B.C. because he had ascertained that the passage from it to the island was *the most convenient*, argues that 'before it was consequently unknown to him', and that 'at first he sought *the shortest passage*'. Long,[3] on the other hand, insists that when Caesar says that he had ascertained that the passage from the Portus Itius was the most convenient, he apparently means 'that he had by his first voyage found out that this was the best place to sail from'. 'His first voyage,' Long continues, 'was very lucky, and there was no reason to change his place of embarkation, particularly as he intended to land, and did land, at the place where he had landed before. Besides this, when he speaks (v. 8) of his landing-place on the second voyage, he says, "qua optimum esse egressum superiore aestate cognoverat"; the same form of expression that he uses in speaking of the place of embarkation (v. 2), except that he does not there use the words "superiore aestate".' I may observe that it is not quite true that Caesar in 54 B.C. 'intended to land, and did land, at the place where he had landed before'.[4] On the other hand, Mr. H. E. Malden has remarked (though he has since abandoned the conclusion to which his remarks led him) that Caesar 'names the second [port] and does not name the first . . . he especially mentions that he disembarked on both occasions at the same place, he gives himself every opportunity for saying that he sailed from the same port, if he did so, but yet he never says it'.[5] Strabo admittedly implies that in the first expedition Caesar's point of departure was the Portus Itius: but his testimony does not settle the question; for he may only have been putting his own construction on Caesar's

τὴν νῆσον· νύκτωρ δ' ἀνήχθη, καὶ τῇ ὑστεραίᾳ κατῆρε περὶ τετάρτην ὥραν, τριακοσίους καὶ εἴκοσι σταδίους τοῦ διάπλου τελέσας.

[1] *Geogr.*, ii, 9, § 1.

[2] *Gesch. Roms*, 1837, iii, 294, n. 13.

[3] *C. J. Caesaris comm. de b. G.*, ed. 1880, p. 277.

[4] See pp. 662, 664–5, *infra*.

[5] *Journal of Philology*, xvii, 1888, p. 164.

words. Rudolf Schneider[1] concludes that it is impossible to *prove* that the Portus Itius was the starting-point of both voyages, but that it most probably was, because Caesar, before his first expedition, had stayed long enough in the country of the Morini to find out the most convenient harbour. I go further, and shall prove, in the course of this discussion, that, on his first as on his second expedition, Caesar sailed from the Portus Itius.[2]

IV. THE VALUE OF CAESAR'S ESTIMATE OF THE DISTANCE BETWEEN THE PORTUS ITIUS AND BRITAIN

Rudolf Schneider[3] insists that it is idle to lay stress on Caesar's estimate of the distance from the Portus Itius to Britain, first, because he had no means of making an accurate calculation, and, secondly, because we cannot tell whether he reckoned the distance to the nearest point of Britain or to his own landing-place. As regards correspondence with Caesar's estimate, Schneider continues, there is nothing to choose between Boulogne, Wissant, and Calais: Dover is 34 Roman miles from Boulogne, 25 from Wissant, 28 from Calais. It might perhaps be argued that, if Caesar took his own landing-place as the terminus, it would be hardly safe to ignore his estimate. For Wissant is 27 Roman miles from Deal, and Boulogne 39; and, assuming that Caesar landed near Hythe, Wissant is 32 Roman miles from that port, and Boulogne 37. It appears, however, to me almost certain that Caesar's estimate referred to the distance from the Portus Itius to the nearest frequented port of Britain;[4] and it must not be forgotten that the ancient

[1] *Portus Itius*, p. 5.

[2] J. F. Henry argues (*Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, 1810, pp. 54–5) that Caesar could only have estimated the distance from the Portus Itius to Britain by making the voyage; that, as he was carried out of his course on the second voyage, the one by which he estimated the distance must have been the first; and consequently that in 55 as in 54 B.C. he must have sailed from the Portus Itius.

But Henry forgot that Volusenus, whom Caesar sent in 55 B.C. to reconnoitre the British coast, may have made the estimate. Or Caesar may have accepted the estimate of merchants, of seamen, or of Commius: it is useless to guess.

[3] *Portus Itius*, p. 9.

[4] In support of the view that Caesar reckoned the distance to the nearest port of Britain, Heller (*Zeitschrift für allgemeine Erdkunde*, xviii, 1865, pp. 172–3) argues (1) that if he had intended to indicate the distance to his landing-place, he would probably have written, not (quo ex portu commodissimum) *in Britanniam* (traiectum esse cognoverat, &c.) but *ad eum locum quo est descensum* ('to the spot where the disembarkation took place'); (2) that when Strabo estimated the length of Caesar's voyage at 320 stades, or 40 Roman miles, he must either have found (milium passuum) *XXXX*, which is not in any extant MS., in his copy of the *Commentaries*, or have concluded, from other information, that Caesar had underestimated the distance; and in either case the fact that he expressly mentions the time which Caesar took to reach his *anchorage* shows that he did not take into account the additional 7 miles which separated the anchorage from the landing-place.

It will be seen, however, that, although I agree with Heller's conclusion, the proofs by which I shall establish the identity of the Portus Itius are wholly independent of it.

writers generally overestimated the distance from one port to another.[1] Moreover, it is not absolutely certain that Caesar estimated the distance of the Portus Itius from Britain at 30 Roman miles. *XXX* is indeed found in all the extant *MSS.*;[2] but as Strabo unquestionably used the *Commentaries* when he wrote his notice of Caesar's voyage, and estimated its length as 320 stades, it is not improbable that he found in his copy the number *XXXX*.[3] Schneider, however, points out that Strabo's estimate of the length of the south coast of Britain differs from Caesar's; and the accuracy of the *MSS.*, as regards the number *XXX*, may perhaps, as he says, be supported by a comparison of Pliny with Caesar. Pliny[4] says that the shortest passage between Ireland and Britain is 30 miles, and Caesar[5] says that the passage from Ireland to Britain is equal in length to the passage from Britain to Gaul. On the whole, we may conclude that Caesar's estimate of the distance between the Portus Itius and Britain does not help us to decide whether the Portus Itius is to be identified with Wissant or with Boulogne. But, in considering the arguments for the identity of the Portus Itius with the mouth of the Somme, Caesar's estimate must obviously be taken into account.[6]

V. THE ESTUARY OF THE SOMME

The advocate of the Somme was the late Astronomer-Royal, Sir George Airy. His arguments shall be considered for the benefit of those who are influenced by his great reputation; but one fact, which he ignores, is alone sufficient to wreck his theory. If Caesar sailed from the mouth of the Somme, the *superior portus*, from which his cavalry transports sailed was, as Airy of course maintains, the mouth of the Authie, and the place where he landed in Britain was, as Airy likewise maintains, Pevensey. Therefore, on Airy's theory, the cavalry transports, when they were approaching Britain and were seen in the offing from Caesar's camp, were approaching Pevensey; and the gale which prevented them from reaching their destination and drove some of them 'in great peril' (*magno cum periculo*) westward down the coast, carried the others back to the mouth of the Authie.[7] But, as the harbour-master of Dover remarked to me, and as any one may see for himself who has the most rudimentary knowledge of seamanship, it would have been utterly impossible for them to fetch the mouth of that river.

But to timid reasoners this may appear too summary a method of disposing of Airy's theory. Let us then hear what he has to say.

First, Airy maintains that Caesar, when he says that he 'set out for the country of the Morini' (*in Morinos proficiscitur*), merely implies that he arrived 'near it or close to it', not necessarily that

[1] See pp. 592–3, *infra*.
[2] See p. 619, *infra*.
[3] Cf. Mommsen, *Röm. Gesch.*, iii, 1889, pp. 269–70, note (Eng. trans., v, 1894, p. 63, note).
[4] *Nat. Hist.*, iv, 16 (30), § 103.
[5] *B. G.*, v, 13, § 2.
[6] See pp. 561–3, *infra*.
[7] See pp. 554–5, *supra*.

he actually entered it. He insists that in every instance in which Caesar 'uses the inflexions or derivatives of "proficiscor"' 'another sentence or another clause is required to denote arrival at the journey's end'.[1]

Now Caesar uses *proficisci* with *in* thirty-five times. If the reader will turn to the lists of those passages in Meusel's *Lexicon Caesarianum* (ii, 96, 1240), he will find that in almost every instance in which Caesar says that he himself or any one else 'started for' or was about to 'start for' this or that place, the context proves that the place was reached. Of course the proof is generally furnished by 'another sentence or another clause', or by more than one other sentence. But this is not always the case.[2] And for the passage in question similar proof is forthcoming. Immediately after telling us that he marched for the country of the Morini, Caesar goes on to say that he ordered his fleet to assemble there.[3] As Long sensibly remarks,[4] 'when a man says that he "marches for" or "towards the country of the Morini because the passage from there to Britain was the shortest"; that he ordered all his ships to come there; and that while he was waiting "in these parts" (*in his locis* [5]) to get his ships ready, ambassadors from a large part of the Morini came to him, there is only one conclusion, which is, that he was in the country of the Morini and sailed from it.' If Caesar had removed his ships from the country where he had assembled them and had sailed from some other place, he would surely have said something to warn his readers against drawing the conclusion which, to every one except Airy, has always appeared inevitable.

Secondly, Airy points to the passage [6] in which Caesar relates that while he was collecting ships for the first expedition envoys came to him from the Morini: 'the visit of the ambassadors,' he argues, 'without any mention of hostile occupation, seems to imply that neither Caesar nor any part of his army was in the country of the Morini at the time of preparing the naval expedition, and appears to render it most improbable that he had passed through their country.' [7]

No unbiassed reader would assent to this conclusion. The Morini naturally sent ambassadors to Caesar because they wished to

[1] *Archaeologia*, xxxiv, 1852, p. 235.

[2] Speaking of Druidism, Caesar says (*B. G.*, vi, 13, §§ 11–2) *disciplina in Britannia reperta atque inde in Galliam translata existimatur, et nunc qui diligentius eam rem cognoscere volunt plerumque illo discendi causa proficiscuntur.* With this word the chapter ends; but it is undeniable that those who wished to study the tenets of Druidism did go to Britain.

[3] *Ib.*, iv, 21, §§ 3–4.—Ipse cum omnibus copiis in Morinos proficiscitur, quod inde erat brevissimus in Britanniam traiectus. Huc naves undique ex finitimis regionibus et quam superiore aestate ad Veneticum bellum fecerat classem iubet convenire.

[4] *C. J. Caesaris comm.*, &c., p. 278, note.

[5] Lewin (*The Invasion of Britain*, &c., 1862, pp. v–vi), justly ridicules Airy's desperate contention (*Athenæum*, Sept. 10, 1859, p. 337) that *in his locis* is 'a studiously indefinite expression'.

[6] *B. G.*, iv, 22, § 1.

[7] *Archaeologia*, xxxiv, 1852, p. 235.

deprecate his wrath. Similarly in 53 B.C. the Ubii sent envoys to him when he was in their country, not as an enemy but as a friend.[1] Besides, Airy, not having a really intimate knowledge of the *Commentaries*, overlooked another important point:—the Morini did not act as one undivided state; some only of their *pagi*, or subdivisional communities, sent ambassadors: others sent none.[2]

Thirdly, Airy refers to Caesar's statement, that, on returning to the Portus Itius from his second expedition, he directed Gaius Fabius to winter in the country of the Morini with one legion.[3] 'It appears to me,' he says, 'that the order (after his second return) for legions to march *from the Portus Itius*[4] "in Morinos" makes it certain that he was not in their country.'[5]

The words 'from the Portus Itius' beg the question. One legion (not 'legions') was sent into the country of the Morini, not from the Portus Itius but from Samarobriva, in the country of the Ambiani, where, as the context shows, all the legions were temporarily assembled.[6] Similarly in the following year, 53 B.C., Caesar concentrated all his legions at Durocortorum, or Reims, immediately before distributing them in their respective quarters for the winter.[7]

Fourthly, Airy refers to the well-known passage in which Strabo[8] says that 'the Itian' [naval station ?] is *παρὰ* (*τοῖς Μορινοῖς*). *Παρά*, he seriously affirms, means 'near to', not 'in' (the country of the Morini[9]).

If Airy's sense of humour had not been dormant, it would surely have occurred to him that, in a matter of pure scholarship, it was unlikely that all Greek scholars should be wrong while he alone was right. Dr. Guest took the trouble to refute him;[10] and if he had referred to other passages in Strabo,[11] he would have seen for himself that *παρ' οἷς* means 'in whose country'.

[1] *B. G.*, vi, 9, § 6.

[2] *Ib.*, iv, 22, §§ 1, 5.—Dum in his locis Caesar navium parandarum causa moratur, ex magna parte Morinorum ad eum legati venerunt . . . reliquum exercitum Titurio Sabino et Aurunculeio Cottae legatis in Menapios atque in eos pagos Morinorum a quibus ad eum legati non venerant ducendum dedit.

[3] *Ib.*, v, 24, § 2.

[4] The italics are mine.

[5] *Essays on the Invasion of Britain by Julius Caesar* (privately printed), 1865, p. 27.

[6] *B. G.*, v, 24, §§ 1-2.—Subductis navibus concilioque Gallorum Samarobrivae peracto, quod eo anno frumentum in Gallia propter siccitates angustius provenerat, coactus est aliter ac superioribus annis exercitum in hibernis conlocare legionesque in plures civitates distribuere. Ex quibus unam in Morinos ducendam C. Fabio legato dedit, alteram in Nervios Q. Ciceroni, tertiam in Esuvios L. Roscio, quartam in Remis cum T. Labieno in confinio Treverorum hiemare iussit, tres in Belgio conlocavit, &c.

[7] *Ib.*, vi, 44, §§ 1, 3.

[8] *Geogr.*, iv, 5, § 2.—*τῶν ὁμορούντων τοῖς Μεναπίοις Μορινῶν, παρ' οἷς ἐστι καὶ τὸ Ἴτιον*, &c.

[9] *Athenæum*, Sept. 5, 1863, p. 302.

[10] *Origines Celticae*, ii, 1883, p. 358.

[11] *e.g.*, iv, 2, § 2.—*παρὰ μὲν οὖν τοῖς Πετροκορίοις σιδηρουργεῖά ἐστιν ἀστεῖα καὶ τοῖς Κούβοις Βιτούριξι, παρὰ δὲ τοῖς Καδούρκοις λινουργίαι*; and iv. 3, § 3,—*τὴν δ' ἐπὶ τῷ Ῥήνῳ πρῶτοι τῶν ἁπάντων οἰκοῦσιν Ἑλουήττιοι, παρ' οἷς εἰσιν αἱ πηγαὶ τοῦ ποταμοῦ ἐν τῷ Ἀδούλᾳ ὄρει*. See also viii, 3, § 11.

Airy was far too vigilant a controversialist not to see that there were well-grounded objections to his theory; and he attempted to anticipate them. Caesar, as we have seen, states that when his fleet was returning to Gaul after the first expedition, two of the ships failed to make the same harbours as the rest, and 'were carried a little lower down' (*duae* [naves] *eosdem quos reliqui portus capere non potuerunt et paulo infra delatae sunt*[1]); and he goes on to say that the troops who landed from these two ships were attacked by the Morini while they were marching from the place where they had disembarked to his own camp. The words *paulo infra delatae sunt* are interpreted by almost every commentator except Airy[2] as meaning that the two ships were carried a little further down the coast than the other ships; and if this interpretation is correct, it is obvious that the harbours in which the other ships came to land were in the country of the Morini. But Airy is unmoved by this consensus of opinion. 'The word "delatae",' he says, 'is repeatedly used by Caesar for "drifted", and "infra delatae" is "drifted down", the word "down" apparently relating not to any geographical direction, but to the force of the wind.'[3]

As a matter of fact, Caesar uses the past participle of *defero* in the sense of 'drifted' four times,[4] namely, in the passage which we are now considering and in *B. G.*, v, 8, § 2 (*longius delatus aestu orta luce sub sinistra Britanniam relictam conspexit*), *B. C.*, iii, 14, § 2 (*una ex his* [navibus] . . . *delata Oricum atque a Bibulo expugnata est*), and *B. C.*, iii, 30, § 1 (*praetervectas Apolloniam Dyrrachiumque naves viderant . . . sed quo essent eae delatae . . . ignorabant*). None of these passages lends the slightest support to Airy's theory; and the other three passages in which he uses the adverb *infra*[5] in a local sense, namely, *B. G.*, vi, 35, § 6 (*transeunt Rhenum . . . XXX milibus passuum infra eum locum ubi pons erat perfectus*[6]), *B. G.*, vii, 61, § 3 (*nuntiatur . . . magnum ire agmen adverso flumine sonitumque remorum in eadem parte exaudiri et paulo infra milites navibus transportari*[7]), and *B. C.*, i, 64, § 6 (*reliquas legiones expeditas educit magnoque numero iumentorum in flumine supra atque infra constituto traducit exercitum*[8]), are fatal to it.

Again, the passage in which Caesar says that the distance from the Portus Itius to Britain is about 30 [Roman] miles,[9] assuming that it is genuine, might well have disconcerted a less resourceful

[1] *B. G.*, iv, 36, § 4.
[2] General Creuly (*Rev. arch.*, nouv. sér., viii, 1863, p. 310) agrees with Airy.
[3] *Archaeologia*, xxxiv, 1852, p. 237.
[4] See H. Meusel, *Lex. Caes.*, i, 843. [5] *Ib.*, ii, 167.
[6] 'They crossed the Rhine . . . 30 miles below the place where the bridge had been built.'
[7] 'It was announced . . . that a large column was moving up the river, and that the sound of oars was audible in the same direction; and that troops were being ferried across the river in barges below.'
[8] 'He led out the remaining legions in light marching order; stationed a large number of baggage-cattle in the river on the upper and the lower side [of the ford]; and made the army cross over.' Cf. *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, pp. 163–4. [9] *B. G.*, v, 2, § 3.

reasoner; for the distance from the mouth of the Somme to St. Leonards, off which place Airy maintains that Caesar anchored on his first voyage,[1] is about 65 Roman miles; and to Pevensey Level, where he makes Caesar land, a little more. But Airy confidently grapples with the difficulty. 'I conceive,' he remarks, 'that the sentence has been mistranslated. The Portus Itius and the continent are placed in contradistinction. The *convenient passage* was from the Portus Itius, the distance of 30 miles was from the continent.'[2]

Read the sentence again,—*portum Itium . . . quo ex portu commodissimum in Britanniam traiectum esse cognoverat, circiter milium passuum XXX transmissum a continenti.* Classical scholars are agreed that these words can only mean what Airy insists that they do not mean, namely, that the distance from the Portus Itius to Britain was about 30 Roman miles. If not from the Portus Itius, from what port? What would have been the good of specifying the distance if Caesar had been thinking of some other port which he did not use?[3]

Furthermore, the distance from the mouth of the Somme to Pevensey Level is about twice the distance from Boulogne to Dover, to Hythe, or to Lympne; and Caesar says that the reason why he marched for the country of the Morini was that the passage from their country to Britain was the shortest.

But it would seem that Airy was not quite convinced of the soundness of his own reasoning. 'If,' he says, 'any reader thinks that the reasons for excluding the Portus Itius from the land of the Morini are not sufficiently cogent, the whole is easily reconciled with the hypothesis that the Portus Itius was the mouth of the

[1] Sometimes Airy makes Caesar anchor off St. Leonards; sometimes off Bexhill. We may give him his choice.

[2] *Archaeologia*, xxxiv, 1852, pp. 237–8. I find, to my amazement, that Desjardins agrees with Airy. 'The text,' he argues (*Géogr. de la Gaule rom.*, i, 360–1), 'does not say precisely that the *Portus Itius* is 30 miles from Britain; it only says (1) that Caesar had ascertained that it was a very convenient port; and (2) that Britain was about 30 miles from the continent. Here we have two distinct statements.'

It is worth mentioning that Airy, in quoting the passage, omits *transmissum*, while Desjardins retains it. It was originally deleted by Faërn, who has been followed by various editors, in defiance of the *MSS*. See C. E. C. Schneider, *Comm. de bellis C. I. Caesaris*, ii, 1849, p. 11.

[3] Airy seems to have felt the necessity of bolstering up his argument; for he remarks (*Essays on the Invasion of Britain*, &c., p. 27) that 'before the Triangulation of the year 1787, it was a fair and an insoluble question, whether the distance from the Continent to Britain was less than twenty or greater than forty miles'. Perhaps; but long before the aforesaid Triangulation sailors used to make wonderfully good guesses about this 'insoluble question'. Cluver tells us that while staying with Sir Thomas Waller, Warden of the Cinque Ports, he questioned all who could give him trustworthy information, and particularly seamen, as to the passages between England and France. The unanimous reply was that the distance between Dover and Calais was 28 English miles, and that *the most convenient passage was between Dover and Boulogne*, and was 32 English miles (*Germania antiqua*, 1631, lib. ii, cap. xxviii, p. 445). Similarly, the Arab geographer, Edrisi, who died about 1180, affirmed that the distance between Wissant and England was 25 Roman miles (*Geogr. Nubiensis . . .*, 1619, pp. 253–4).

Somme by supposing that in the time of Caesar the Morini stretched south-west of the Somme . . . the geography which limits their territory to the north of the Somme is 120 years later. Any one who reflects on the change of boundary of Russia, of Prussia, of Turkey, and of other European States, within a period of much less than 120 years, will find no difficulty in admitting this change in the limits of the Morini.' [1]

It is sufficient to answer that there is no analogy between the political history of Europe in the earlier half of the nineteenth century and that of Gaul in the 120 years that followed the invasion of Britain by Caesar. The Gallic peoples during that period were not at war with one another; and there is not the slightest reason to suppose that the Morini possessed a wider seaboard in Caesar's time than 120 years later. Lewin's reply to Airy is worth quoting [2]:—'He offers, as a solution of the difficulty, that in the time of Caesar the Morini stretched south-west of the Somme. If so, then the Somme itself, from which Caesar sailed, and to which he returned, was, according to the Astronomer-Royal, in the country of the Morini; and yet, a few lines before, the Astronomer-Royal had stated that the order (after Caesar's second return) for legions to march into the country of the Morini made it *certain* that he was *not in their country*! Thus to avoid Scylla, it is laid down, as *certain* that Caesar did not sail from the Morini; and then, to avoid Charybdis, the reader is invited to assume that the place of embarkation was amongst the Morini.'

Finally, Airy affirms that the mouth of the Somme was by far the best harbour which Caesar could have selected, and that its capability for his purpose 'is proved by the . . . experience of William of Normandy, who at one tide floated out of it 1400 ships'.[3]

Now William the Conqueror assembled his fleet and embarked his army not in the mouth of the Somme but in the mouth of the Dive: [4] he was merely obliged, as Lewin says,[5] 'to take temporary shelter . . . at the mouth of the Somme.' But this blunder is of no great consequence. The Somme might have served Caesar's purpose if only it had not been twice as far from that part of Britain to which he intended to go as Boulogne.

VI. AMBLETEUSE

The *Commission de la Topographie des Gaules* [6] identify the Portus Itius with Ambleteuse; and Mommsen [7] is disposed to agree with them. They argue that Strabo [8] affirms the existence of two ports

[1] *Essays on the Invasion of Britain*, &c., p. 27.
[2] *The Invasion of Britain*, &c., 1862, pp. xii–xiii.
[3] *Archaeologia*, xxxiv, 1852, p. 236.
[4] See E. A. Freeman's *Norman Conquest*, iii, 386–99.
[5] *The Invasion of Britain*, &c., 1862, pp. xi–xiii, xvii.
[6] *Dict. arch. de la Gaule*, ii, 45–7.
[7] *Röm. Gesch.*, iii, 1889, p. 270, note (Eng. trans., v, 1894, p. 63, note).
[8] *Geogr.*, iv, 5, § 2.

in the country of the Morini; that one of the two was evidently Gesoriacum; and that the Portus Itius was therefore something different. The passage in Strabo to which the commission refers will be most conveniently examined in a later section.[1] Meanwhile it is enough to say that if it proves that the Portus Itius was not Gesoriacum, it does not prove that the Portus Itius was Ambleteuse.

General Creuly[2] decides for Ambleteuse on the ground that its distance from Wissant corresponds with Caesar's statement of the distance which separated his own port of embarkation from the *ulterior portus*, and that the intervention of Cape Grisnez between Ambleteuse and Wissant would have justified Caesar in describing the latter as the *ulterior portus*. He remarks that if the Portus Itius is identified with Boulogne, the *ulterior portus* must have been Ambleteuse. But, referring to Vergil's well-known line—*tendebantque manus ripae ulterioris amore*[3]—he argues that the word *ulterior* implies the intervention between the Portus Itius and the *ulterior portus* of an 'objet disjonctif', such as a promontory; and he insists that no such 'objet' intervenes between Boulogne and Ambleteuse. But Heller[4] observes that a passage in the *Germania*[5] of Tacitus—(Gerunt et ferarum pelles), *proximi ripae negligenter, ulteriores exquisitius*—would seem to show that *ulterior* means much the same as *longinquior*.[6] Besides, if the distance from Ambleteuse to Wissant justifies us in identifying Ambleteuse with the Portus Itius and Wissant with the *ulterior portus*, the distance from Boulogne to Ambleteuse, as I shall presently show, equally justifies us in identifying Boulogne with the Portus Itius and Ambleteuse with the *ulterior portus*.[7]

Not a single valid argument ever has been or can be adduced in favour of Ambleteuse. The harbour is far too small to have contained Caesar's fleet; and the merest tiro in his army could have decided at a glance between its merits and those of Boulogne.[8]

[1] See pp. 577–9, *infra*.

[2] *Rev. arch.*, nouv. sér., viii, 1863, pp. 307–8.

[3] . . . the spirits of the dead 'stretched out their hands in longing for the further shore' (*Aen.*, vi, 314).

[4] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, p. 164.

[5] (They also wear the skins of wild beasts), 'the riparian tribes in a careless fashion, those of the interior with more elegance' (c. 17).

[6] As a matter of fact, if the *Portus Itius* was the estuary of the Liane, there *was* an 'objet disjonctif' between it and Ambleteuse, namely, the headland north of Boulogne harbour.

[7] Creuly also observes (*Rev. arch.*, nouv. sér., viii, 1863, p. 307) that, while Ambleteuse was 31 Roman miles from Fort Sutherland on Romney Marsh, Boulogne was 36. But Caesar, as I shall prove (pp. 622–44), did not land on Romney Marsh; and the futility of arguments of this kind has been already pointed out (pp. 557–8).

[8] It is worth noting that while Creuly pins his faith to Ambleteuse, he is not so foolish as to ask us to believe that Caesar's 800 ships found room there. They anchored, he tells us (p. 310), in the roadstead. But Caesar says expressly (*B. G.*, v, 2, § 3; 5, § 2) that they assembled in the Portus Itius.

VII. CALAIS

I only consider the claims of Calais because their one modern advocate, General von Göler,[1] was a distinguished Caesarian scholar. There is no evidence that Calais was ever used as a harbour in, or for twelve centuries after, the time of Caesar. If Caesar started from the Portus Itius on his first expedition, it is impossible, on the theory that Calais was the Portus Itius, to find the *ulterior portus*. Moreover, it would have been impossible for Caesar to sail, on his second expedition, from Calais to any point of the Kentish coast between Walmer and Sandwich.[2] For, as the wind was from the south-west, he would have had to sail within seven points and a half of the wind on a flood tide, which would have tended to carry him into the North Sea, and with shallow flat-bottomed vessels which made excessive lee-way.[3] Finally, Calais Harbour is not natural but artificial; and it is certain that it did not exist in the time of Caesar.[4]

VIII. WISSANT

Wissant is between Cape Grisnez and Cape Blancnez, both of which, in Caesar's time, projected somewhat further out to sea than they do now.[5] Dr. Guest argues that the sandy waste, more than two miles long and varying in breadth from a quarter to half a mile, which lies between the uplands and the sand-hills, was once covered by the sea;[6] and he conjectures that the 'pool-harbour' thus formed communicated with the English Channel by 'the gap through which flows the Rieu des Aiguilles', a rivulet which crosses the sandy plain. At the same time he admits that it is very difficult to say what the limits of the ancient harbour were.[7]

[1] *Gall. Krieg*, 1880, p. 143.

[2] See J. F. Henry, *Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, p. 123.

[3] In order to counteract the effect of the flood, it would of course have been necessary to keep the ships' heads much closer to the wind than within seven points and a half, which would have been impossible.

[4] See *Mém. de la Soc. des ant. de la Morinie*, i, 1833 (1834), p. 253; A. E. E. Desjardins, *Géogr. de la Gaule rom.*, i, 349–50, n. 3; J. J. Chifflet, *Portus Iccius*, pp. 25–6; and V. de St. Martin, *Nouv. Dict. de Géogr. univ.*, i, 1879, p. 568.

[5] *Mém. couronnés par l'Acad. Roy . . . de Bruxelles*, vi, 1827, pp. 149–50; Allent, *Appendice à l'essai sur les reconnaissances militaires*, pp. 667–8 (in tome i [1829] of *Mémorial du Dépôt Gén. de la Guerre*); V. de St. Martin, *Nouv. Dict. de Géogr. univ.*, ii, 542.

[6] *Archaeol. Journal*, xxi, 1864, p. 223. This suggestion, which was hailed by Freeman (see p. 553, *supra*) as a brilliant and conclusive discovery, was by no means new. Guest had been anticipated by Du Fresne (D. Haigneré, *Recueil hist. du Boulonnais*, ii, 1897, p. 431, n. 1) and by the Abbé de Fontenu (*Mém. de litt. tirez des registres de l'Acad. Roy. des Inscr. et Belles-Lettres*, xiii, 1734–7 [1740], plan between pages 416 and 417).

[7] According to de Saulcy (*Les campagnes de Jules César dans les Gaules*, p. 172, n. 1), the inhabitants of the country believe that it extended only from the mouth of the 'ruisseau du Phare' to that of the 'ruisseau d'Herlan',—a distance of less than a mile and a quarter.

Dr. Guest's theory, which was regarded by Mr. Freeman and Dean Merivale as conclusive, is a theory and nothing more. Mariette, the famous Egyptologist, states that the dunes themselves (without which Dr. Guest's harbour could not have been) were not formed before the time of Edward the Third ;[1] and M. H. Rigaux concludes, from a recent minute exploration of the coast between Cape Grisnez and Sangatte, that the dune which extends from the 'ruisseau de Guiptun', near Tardinghem, to the 'ruisseau d'Herlan' at Wissant did not exist in Caesar's day.[2] Moreover, pottery, pre-Roman and Roman, has been found in the sand behind the dunes between Wissant and Tardinghem as well as east of Wissant ;[3] numerous finds have proved that the coast between Sangatte and Dunkirk has undergone subsidence and extended further seaward in Roman times than now ;[4] and it may be concluded that the sandy plain at Wissant was not then covered by the sea. It would appear, then, that Dr. Guest's pool-harbour was imaginary. Haigneré,[5] moreover, remarks that if there ever had been such a harbour, it must have been speedily choked up by sand blown from the very dunes which *ex hypothesi* formed it ; and this argument is confirmed by the fact that irruptions of blown sand, before the dunes were 'fixed' by being planted with coarse grass, engulfed many buildings at Wissant.[6] It has, however, been pointed out by Mr. E. C. H. Day [7] that 'a shoal having less than a fathom of water on it at the lowest tides, extends from Cape Grisnez, in a north-easterly direction, in such a manner as to cut off a channel about half a mile in width, and having a depth of from two to three fathoms of water in it, directly abreast of Wissant. The shoal,' he adds, 'during the course of centuries of exposure to the heavy seas that break upon the coast, must have undergone some considerable amount of destruction. Formerly, therefore, this shoal must have formed a natural breakwater, and have rendered the channel within it a convenient harbour.' But, assuming the correctness of Mr. Day's deduction, this 'harbour' would have been exposed to the fury both of the west and of the north-east wind. M. Léon Lejeal,[8] who tells us that a French engineer, M. J. Voisin, supposes that the shoal was once connected with the mainland, and thus formed a partially-sheltered harbour, concludes

[1] *Lettre à M. Bouillet . . . sur l'article Boulogne de son Dictionnaire*, 1827, p. 20, n. 30. Cf. *Annales de géogr.*, ii, 1893, p. 313. I have not been able to discover any *historical* evidence which would show that no dunes, small or great, existed at Wissant before the time of Edward III ; but it is certain that immense quantities of sand were accumulated there in the fifteenth, sixteenth, and seventeenth centuries. See *Boulogne-sur-mer et la région boulonnaise*, i, 372.

[2] *Annales de la Soc. géol du Nord*, xxviii, 1899, pp. 85–7.

[3] *Annales de géogr.*, ii, 1893, p. 312 ; *Annales de la Soc. géol. du Nord*, viii, 1882, p. 1 ; xxviii, 1899, pp. 86, 88 ; J. Gosselet, *Esquisse géol.*, &c., 4e fasc., 1903, p. 406.

[4] *Ib.*, pp. 411, 416.

[5] *Étude sur le Portus Itius*, p. 89.

[6] *Annales de géogr.*, ii, 1893, p. 314 ; *Boulogne-sur-mer et la région boulonnaise*, i, 372 ; R. Blanchard, *La Flamande*, p. 314.

[7] *Geol. Mag.*, iii, 1866, pp. 113–4.

[8] *Boulogne-sur-mer et la région boulonnaise*, i, 372–3.

that there is nothing to show that it was large enough to shelter the fleet 'que voulut y ancrer l'imagination d'une archéologie en délire';[1] and M. Leblanc, who in the year 1863 was engineer of the port of Calais and was intimately acquainted with the geology of Wissant, ridicules such a notion. 'Toutes les fois,' he remarks, 'que j'allais de Calais au Gris-Nez . . . je traversais Wissant, en étudiant cette question, et je me disais à moi-même : quelle preuve peut on avoir d'une pareille absurdité ?'[2] Hariulf, a chronicler who lived in the eleventh and twelfth centuries, described the harbour of Wissant as an 'inlet' (*ingressum maris*),[3] which would seem to imply that it was simply a creek formed by one of the rivulets which meander across the sand. Henry, the historian of Boulogne, who was bent upon proving the identity of Wissant with the Portus Itius, would certainly have anticipated Guest's theory if he could have done so with truth ; but, after a careful examination of the site, he came to the conclusion that Caesar's ships must have been drawn up on an exposed beach.[4]

1. Long[5] argues that the distance of Wissant from Sangatte corresponds with the distance between the port from which Caesar started on his first expedition and the *superior portus* ; that its distance from the English coast agrees 'at least as well as any other place' with Caesar's estimate of the distance from the Portus Itius to Britain ; that its name, which [according to Michel Baudrand,[6]

[1] It must have been very narrow when the coast extended further seaward, if indeed it existed.

[2] *Mém. de l'Acad. d'Arras*, xxxv, 1863, p. 273. Haigneré (*Étude sur le Portus Itius*, pp. 87–8) very properly warns his readers not to place any reliance upon a certain 'Vue du port de Wissant', a copy of which exists in the Museum at Boulogne. 'C'est une œuvre de fantaisie.'

[3] *Chronique de l'abbaye de Saint-Riquier*, 1894, p. 241 (lib. iv, cap. xxiii).

[4] *Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, p. 55.

[5] *C. J. Caesaris comm.*, &c., pp. 278, 285.

[6] P. Ferrarius, *Lexicon geogr.*, 1670, i, 370.

[7] 'The fact,' says a writer in the *Edinburgh Review* (Oct., 1900, p. 442), 'that it was in mediaeval times a "frequented port" is, strictly speaking, not a fact at all . . . the contemporary references to it which have been collected by French scholars show that it was neither a town nor a harbour, but an open beach, which travellers in a hurry could use with a favourable wind.' These remarks are inaccurate : see p. 580, *infra*. The mediaeval port, or portlet, if local tradition is to be trusted, was, however, simply the mouth of the rivulet, variously called the Rieu de Sombre, Rieu d'Herlan, and Ruisseau du Moulin, which flows through the modern town of Wissant, enlarged and deepened (F. A. F. Mariette, *Lettre à M. Bouillet*, &c., p. 30, n. 20 ; D. Haigneré, *Étude sur le Portus Itius*, p. 91) ; and the tradition has been confirmed by the explorations of M. Rigaux (*Annales de la Soc. géol. du Nord*, xxviii, 1899, p. 88). In the dunes which border on the creek formed by the rivulet there have been found certain old balks of oak, mentioned by Haigneré (*op. cit.*) and C. de Saulcy (*Les campagnes de Jules César dans les Gaules*, p. 172), which may have belonged to the quays of the mediaeval harbour.

According to Le Quien, the inhabitants of Wissant in his time (early in the eighteenth century) affirmed that the entrance of the harbour had been at the mouth of the Rieu de Ghibelen,—the rivulet nearest to Cape Grisnez (*Mém. de litt. tirez des registres de l'Acad. Roy. des Inscr. et Belles-Lettres*, xiii, 1734–7 [1740], p. 417). I agree with Haigneré that the harbour, such as it was, was at

a writer of the seventeenth century] French sailors used to call Esseu and the Flemings Isten, 'is near enough to Itius to add to the probability of the identity of the two places'; that there are traces of a Roman road from Wissant to Thérouanne; that in the neighbourhood of Wissant 'fresh water was abundant, the soil rich, and the beach the best that there could be for such ships as Caesar's'; and that, if Wissant was not, strictly speaking, a port at all, 'Caesar did not want a port in the modern sense of the word. He wanted his ships at the nearest place to Britain . . . His vessels would be hauled up on the beach till the wind was fair. He had no port on the British coast, and he hauled up all his ships after they were damaged by a storm.' 'This long sandy beach,' he says, 'was the best place along all this coast for Caesar's purpose.'

Of these arguments the first, *mutatis mutandis*, is equally applicable to Boulogne. The argument from nomenclature is worthless:[1] 'Wissant' is not derived from 'Itius'; it is said to be merely a corruption of 'Weiss-sand' (Whitesand).[2] There is no evidence for the alleged Roman road. The Roman road which, according to Henry,[3] led from Thérouanne to Wissant, really led to Sangatte.[4] Dr. Guest, who carefully explored Wissant and its neighbourhood, found that the soil, which Long calls 'rich', is 'notoriously barren'.[5]

the mouth of the Rieu d'Herlan. As he says (*Étude*, &c., p. 91), 'Pour s'assurer de l'endroit où était le port, il n'y avait qu'à se demander où était le village. Or, le village a toujours occupé l'emplacement sur lequel il est encore bâti de nos jours, savoir: partie à l'est du *ruisseau* d'Herlan . . . où les maisons se reconstruisent avec les débris des précédentes,' &c. See also *Bull. de la Soc. de géogr. de Lille*, xix, 1893, p. 199.

[1] C. de Saulcy, in an article in which he endeavours to prove that the Portus Itius was Wissant (*Les campagnes de Jules César dans les Gaules*, p. 161), frankly admits that not one of the natives of Wissant whom he interrogated had ever heard that there was such a name as 'Esseu', or that the Flemings called Wissant 'Itzen' (or 'Isten'). A. Wauters, indeed, referring to B. E. C. Guérard's *Cartulaire de Folcuin*, p. 161, a work which I have failed to procure, affirms (*Bull. de l'Acad. Roy . . . de Belgique*, 2e sér., xlvii, 1879, p. 114) that in a charter of the ninth century property in a place called Istem was granted to the abbey of St. Bertin: but he fails to prove the identity of Istem with Wissant; and even if that identity could be established, no competent etymologist would admit that it supplied an argument for identifying Wissant with the Portus Itius.

[2] This derivation, which is now generally accepted, is mentioned by Lambert of Ardres, a chronicler of the thirteenth century, who speaks of *Britannicum secus portum, qui ab albedine arenae vulgari nomine appellatur Witsant* (J. P. von Ludewig, *Reliquiae manuscriptorum*, &c., viii, 1728, p. 383. Cf. J. F. Pommeraye, *Hist. de l'abbaye royale de S. Ouen de Rouen*, 1662, p. 457), but is disputed by Le Quien (*Dissertation sur le Port Iccius*, pp. 342–3, printed in *Mém. de litt. et d'hist.*, viii, 1749, by P. Desmolets). Remarking that Flodoard, a writer of the tenth century, calls Wissant *Guicsum*, he maintains that *Guicsum* is identical with *Vvicsum*, which would mean 'the port of Sum', just as *Quantovic* (Étaples) means 'the port of the river Canche'. I agree, however, with Desjardins (*Géogr. de la Gaule rom.*, i, 352, note) that it is not certain that by *Guicsum* Flodoard meant Wissant.

[3] *Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, p. 83.

[4] J. Malbrancq, *De Morinis*, i, 1639, p. 27; *Mém. de la Soc. des ant. de Picardie*, iii, 1856, pp. 469–70; A. E. E. Desjardins, *Géogr. de la Gaule rom.*, i. 356–7, note.

[5] *Origines Celticae*, ii, 362–4.

And, in reply to the last of Long's arguments, it is sufficient to say that, although Caesar did perforce beach his ships on the coast of Britain, yet he suffered heavily from not having a port; and the mere fact that he sent Volusenus to ascertain what ports on the British coast were capable of accommodating a large fleet proves that his original intention was to land in a port, and not on an open beach. Long's assertion, that the beach at Wissant 'was the best place along all this coast for Caesar's purpose', Dr. Guest, who agrees with him in identifying the Portus Itius with Wissant, treats with utter scorn; but his criticism is founded upon the groundless assumption that the sand-dune between Wissant and Tardinghem then existed.[1] However, Long admits that 'it would not be possible *now* to draw up a fleet like Caesar's on the beach'. 'But,' he persists, 'if there have been such great changes on this coast that Dr. Guest's huge harbour is filled up, why may not my beach have undergone some change also?'[2] The reply is obvious. What Long calls 'my beach' may have undergone changes: but, unless it can be proved not only that ships could have been hauled up on the beach of Wissant in Caesar's time, but also that there then existed at Wissant a harbour large enough to accommodate Caesar's fleet, the claim of Wissant to be identified with the Portus Itius cannot be admitted.

But Long is not the only writer who maintains that the Portus Itius was not a port properly so called; and this question is so important that we must fairly examine the arguments that have been adduced in support of Long's view.

Heller argues that since Caesar beached his ships on his return from the second expedition, we may conclude that the Portus Itius was not a harbour in the strict sense, as, if the shelter of a harbour had been available, he would not have taken the trouble to draw them up on shore.[3]

But Heller forgets that the ancients never left their ships at anchor for any lengthened period, but invariably laid them up high and dry for the winter.[4] Moreover, if eight hundred ships had been beached at Wissant, would it not have been necessary, in order to protect them from storm-driven spring tides, to construct an enormous naval camp, the earth necessary for which did not exist?

Professor Ridgeway insists that, if Strabo is to be believed, the Portus Itius can only be identified with Wissant.[5] Strabo[6] calls

[1] *Origines Celticae*, ii, 363. Guest apparently forgot that if Wissant was the Portus Itius, Caesar's ships, when they returned from the second expedition, must have been hauled up on the beach (*B. G.*, v, 24, § 1).

[2] *The Reader*, Oct. 10, 1863, p. 414.

[3] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, p. 181.

[4] Cf. Livy, xxxvi, 45, § 8,—ad Canas classis venit; et, cum iam hiems appeteret, fossa valloque circumdatis naves subductae; *B. C.* iii, 23, § 3,—(Libo) adeo loci opportunitate profecit uti ad Pompeium litteras mitteret, naves reliquas, si vellet, subduci et refici iuberet; and Horace, *Carm.*, i, 4, 1-2,—

Solvitur acris hiems grata vice veris et Favoni,
Trahuntque siccas machinae carinas.

[5] *Journal of Philology*, xix, 1891, pp. 141-2. [6] *Geogr.*, iv, 5, § 2.

Caesar's place of embarkation τὸ Ἴτιον. This word, the professor observes, is obviously an adjective, and, as it agrees with a neuter word understood, it cannot agree with λιμήν or κόλπος (a harbour), but must agree with ἄκρον or ἀκρωτήριον (a headland). Evidently, then, Strabo's τὸ Ἴτιον is the same as Ptolemy's Ἴτιον ἄκρον. Similarly Strabo[1] speaks of Cape Finisterre as Νέριον, while Ptolemy[2] calls it Νέριον ἀκρωτήριον. Now Strabo does not call τὸ Ἴτιον a harbour, but only a roadstead (ναύσταθμον), a term which Thucydides[3] applies to Cape Malea. Thus, if Strabo was right, the Portus Itius was the roadstead sheltered by the Itian promontory.

The professor's argument is not convincing. Granted that Ἴτιον must agree with ἄκρον, on him lies the burden of proving that the headland in question was not Cap d'Alprech, which shelters the estuary of the Liane, and the geographical position of which corresponds closely enough with that which Ptolemy assigns to Ἴτιον ἄκρον.[4] When the professor remarks[5] that 'the advocates of both Wissant and Boulogne support the claim of Grisnez' against Alprech he is mistaken. Desjardins is only one of many French writers who 'support the claim' of Alprech against Grisnez. Moreover, supposing that the professor is right in identifying the Itian promontory with Grisnez, he is wrong in assuming that the word ναύσταθμον necessarily excludes the idea of a harbour. Sometimes it is used to denote a port already described as a λιμήν, or harbour properly so called, in order to draw attention to the fact that that harbour was a naval station. Thus Strabo,[6] immediately after mentioning the Piraeus and the other two harbours of Athens, says that the ναύσταθμον was capable of accommodating the four hundred ships which composed the Athenian fleet. And Pausanias,[7] speaking of Nauplia, the port of Argos, which, according to Strabo,[8] was the ναύσταθμον of the Argives, says, 'there are harbours in Nauplia' (λιμένες εἰσὶν ἐν Ναυπλίᾳ).[9] To anybody who knows anything about ancient navigation, the suggestion that Caesar would have kept 800 ships riding at anchor for several weeks in an open roadstead, exposed to the fury of the north-west wind, while,

[1] *Geogr.*, iii, 3, § 5. [2] *Ib.*, ii, 6, § 2. [3] iii. 6.

[4] See p. 556, *supra*. [5] *Journal of Philology*, xix, 1891, p. 142.

[6] *Geogr.*, ix, 1, § 15.—τὸ μὲν οὖν παλαιὸν ἐτετείχιστο καὶ συνῴκιστο ἡ Μουνυχία παραπλησίως ὥσπερ ἡ τῶν Ῥοδίων πόλις, προσειληφυῖα τῷ περιβόλῳ τόν τε Πειραιᾶ καὶ τοὺς λιμένας πλήρεις νεωρίων . . . ἄξιόν τε ἦν ναύσταθμον ταῖς τετρακοσίαις ναυσίν, &c.

[7] *Descr. Graeciae*, ii, 38, § 2. [8] *Geogr.*, viii, 6, § 2.

[9] Evidently ναύσταθμον had a wider range of meaning than *statio navium*. Plutarch (*Aristides*, 23) implies that a ναύσταθμον could be burned, from which Haigneré (*Recueil hist. du Boulonnais*, iii, 1897, p. 455) infers that 'un ναύσταθμος n'est pas une rade foraine, ni une anse, mais un lieu fermé, où se trouvent des arsenaux maritimes'. This was sometimes the meaning of the word, but only rarely. Pliny (*Nat. Hist.*, iii, 8 [14], § 89) mentions a harbour in Sicily, called Portus Naustathmus. See also Stephanus, *Thesaurus Graecae linguae*, v, 1842–6, col. 1383–4. Professor Haverfield (*Eng. Hist. Rev.*, xviii, 1903, p. 335) insists that Strabo (iv, 5, § 2) meant by ναύσταθμον 'the whole region of the Itian highland in which Caesar had his *portus Itius* and his *ulterior portus*'.

a few miles off in the Liane, there was an ample sheltered harbour available, must appear simply ridiculous. And, assuming that Strabo did intend to convey that τὸ Ἴτιον was merely a roadstead, the answer is that Strabo is refuted by Caesar, who says that his ships assembled *ad portum Itium*,[1]—'in the Itian harbour.' The Portus Itius must have been a port, properly so called; and the more discerning advocates of Wissant naturally accept this view.[2]

Long maintains, further, that, although Caesar does not say directly that the passage from the Portus Itius to Britain was actually the shortest, yet he does so indirectly; for he tells us that he went to the country of the Morini 'because the shortest passage to Britain was from their country'; and the port in their country which he selected was the Portus Itius.[3] But, as all who are familiar with the *Commentaries* will admit, Long throws an undue strain upon Caesar's language. Caesar tells us, in general terms, that the shortest passage to Britain was from the country of the Morini: but it is bad logic to conclude from this statement that the passage from the Portus Itius must have been actually the shortest as the crow flies. Caesar would never have chosen the passage which was in this sense the shortest if it had been on other grounds objectionable: obviously what he meant to say was that of the regular passages to Britain that from the country of the Morini was the shortest; and the passage from the Portus Itius being, as he says, 'the most convenient,' was, for all practical purposes, the shortest.

It is clear, then, that Long failed to establish the identity of the Portus Itius with Wissant. Let us see what better informed advocates of the same theory have to say.

2. Not to mention the arguments which are common to him and Long, Dr. Guest gives the following reasons for his belief:—that the (assumed) harbour of Wissant was large enough to hold Caesar's

[1] *B. G.*, v, 2, § 3; 5, § 2.

[2] It is quite true, as General Creuly observes (*Rev. arch.*, nouv. sér., viii, 1863, p. 306) that the author of *Bellum Africum* (c. 10, § 1) applies the name of *portus* to a mere anchorage (cf. Col. Stoffel, *Hist. de Jules César,—Guerre civile*, ii, 110–1, and pl. 20). But *Bellum Africum* was not written by Caesar; and the question is, what Caesar meant by the word *portus*. Now there are certainly two instances in which he applies that word to a harbour very different from the estuary of a river. The harbour of Nymphaeum (now the bay of Medua) on the eastern coast of the Adriatic has a comparatively wide entrance, and is exposed to the full force of the south wind; but against all other winds it is perfectly safe, and it might fairly be called a *portus* and not a *statio* (*B. C.*, iii, 26, § 4. Cf. Col. Stoffel, *Hist. de Jules César*, &c., pl. 14 *bis*). The harbour of Alexandria was formed, as Caesar says (*B. C.*, iii, 112, § 2. Cf. Stoffel, pl. 19), by the island which extended opposite the city, and was divided into two portions by the mole which connected the island with the mainland: the western portion must have been exposed to south westerly winds, but the other offered complete shelter. The conclusion is that the word *portus* had a somewhat elastic signification, but would not have been applied by Caesar to Wissant unless the anchorage there had been protected, as Dr. Guest imagined, by sand-dunes.

[3] *The Reader*, Sept. 19, 1863, p. 317.

fleet; that it lay beneath Cape Grisnez, which he identifies with the Itian promontory; and that William of Poitiers, a chronicler of the eleventh century, called it 'Portus Icius'.[1] He will not admit that William was simply stating his own opinion: 'I think,' he says, 'this name may have been handed down to him by the Romanised Gauls, inasmuch as the name of Ician seems to have been long kept afloat in the recollection of the Celtic population of these islands'; and he points out that 'the old Irish name for the English Channel is *Muir n' Icht*', or 'the Itian sea'. But the fact on which he lays most stress is the proximity of Wissant to Cape Grisnez. He freely admits, indeed, that Cap d'Alprech may, in Caesar's time, have been a more considerable promontory than it is at present;[2] but he cannot conceive that the promontory which Ptolemy selected for especial mention should have been any other than the famous cape which is and must always have been the most conspicuous feature of the north-eastern coast of France, and which marks the point where the coast, making a sharp angle, begins to trend towards the east. 'Cape Grisnez,' he concludes, 'there can hardly be a doubt, was the Ician promontory, and if so, the great port which lay beneath it must have been the Ician Port.'[3]

'The great port which lay beneath it,'—these words, Dr. Guest, beg the whole question. That the harbour of Wissant was large enough to hold Caesar's fleet would be true, if Dr. Guest's conjectural tracing of its outline were correct: but the fact, if it were a fact, would simply remove one of the objections which have been brought against Wissant; it would not prove that Wissant was the Portus Itius. For the harbour of Boulogne was also large enough, and was also, as will presently appear, in other respects far more convenient. The argument that William of Poitiers called Wissant the Portus Itius has no weight. Maistre Wace, who wrote about a century after William of Poitiers, believed that Caesar had sailed from Boulogne.[4] Moreover, Hericus, a monk of the ninth century,[5] identified Bibracte with Autun; but it is now universally admitted, and it is certain, that Hericus was wrong.[6] It may be admitted that *a priori* it would seem much more likely that the Itian promontory was Cape Grisnez than that it was Cap d'Alprech; but if

[1] M. Bouquet, *Recueil des hist. des Gaules*, xi, 1767, pp. 40c, 75c.

[2] This is undeniable. See J. F. Henry, *Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, pp. 66–71; D. Haigneré, *Étude sur le Portus Itius*, p. 85, n. 1; and *Dict. arch. de la Gaule*, ii, 45. Henry calculated from the loss known to have been suffered by Cap d'Alprech and the promontory on which the Tour d'Odre stood during the two centuries and a half that preceded the year 1810, that in Caesar's time they must have extended from 700 to 800 metres further seaward than in 1810. This, however, I believe to be an exaggeration.

[3] *Archaeol. Journal*, xxi, 1864, pp. 227–8.

[4] *Le Roman de Brut*, ed. Le Roux de Lincy, 1836, vv. 3937–40. Similarly Geoffrey of Monmouth (*Hist. Britonum*, iv, § 3) and Matthew Paris (*Chronica majora*, ed. H. R. Luard, i, 1872, p. 73) supposed that Caesar, after his second expedition, had returned to Boulogne.

[5] *Nouv. Biogr. gén.*, xxiii, 1858, p. 802.

[6] See my *Caesar's Conquest of Gaul*, pp. 387–94.

the former identification is to be accepted, it is necessary to assume that Ptolemy made a gross blunder. It is of course quite true, as Dr. Guest says,[1] that Ptolemy did make mistakes; but still the fact remains that the geographical position which he assigns to the Itian promontory is, allowing for a slight error in longitude, that of Cap d'Alprech. As Mr. Peskett puts it, 'Ptolemy, proceeding northward, places the headland between the Somme and Boulogne'; [2] and I may add that if you only know Cap d'Alprech by the map, you will be surprised, when you actually see it, to find how bold a headland it is. Moreover, even if Ptolemy was mistaken, it does not follow that the Itian harbour was Wissant. Professor Rhys, who believes that the Gauls as well as the Irish called the Channel 'the sea of Icht', remarks that 'in that case Portus Ictius would designate Caesar's place of embarcation, somewhat in the same way that Dover might in English be termed the Channel Harbour. The former probably had a Gaulish name of its own, which may have become the Latin one also as soon as the Romans began to be a little more at home in the north of Gaul; so that it would be labour in vain to try to detect *Ictius* in any place-name still current on the French coast.' [3] Let us, however, assume, for the sake of argument, that Professor Rhys is mistaken. Even then it does not follow that the Portus Itius was Wissant. For it will not be denied that Boulogne was, in Caesar's time as in the time of the emperors, a frequented harbour; and it is certain that Wissant was not a harbour capable of containing Caesar's fleet. Therefore Boulogne, which is only nine statute miles south of Cape Grisnez, was obviously the nearest

[1] *Archaeol. Journal*, xxi, 1864, p. 227.

[2] *Journal of Philology*, xx, 1892, p. 192. Gosselin (*Recherches sur la géogr.*, iv, 87–90) attempts to prove that Ptolemy confused two itineraries, and accordingly located the promontory between the Somme and Gesoriacum instead of on the north of the latter.

Henry (*Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, pp. 3–6, 33), referring to Pomponius Mela (iii, 7, §§ 59, 68), maintains that by the word *promontorium* the ancients sometimes designated not merely a cape but also all its 'collateral dependencies'; and accordingly he argues that the Ἴτιον ἄκρον comprised Capes Grisnez and Blancnez, and Cap d'Alprech!

Desjardins (*Géogr. de la Gaule rom.*, i, 371–2) remarks that not only was Cap d'Alprech a more prominent headland 2000 years ago than to-day, but it is actually 9 metres, or about 30 feet, higher than Cape Grisnez; and he insists that the ancients, being unable to form an exact idea of the outline of a coast, took note of those geographical features which appeared to them remarkable, and would therefore have been more inclined to mention Cap d'Alprech than Cape Grisnez. I cannot help thinking that Desjardins would not have resorted to this argument if he had not persuaded himself that the identification of the Portus Itius with Boulogne depended upon the identification of Cap d'Alprech with the Itian promontory. The ancients did not know how to make accurate maps; but they had sufficient powers of observation to be able to see that Cape Grisnez marked the great bend in the coast of North-Eastern Gaul.

It is amusing to find that, whereas Desjardins in his first volume (p. 371) affirmed that the identification of the Itian promontory with Cape Grisnez, if it were admitted, would necessarily involve the identification of the Portus Itius with Wissant, in his third volume (p. 355) he queries his own identification of the promontory with Cap d'Alprech.

[3] *Celtic Britain*, 1904, p. 303.

important harbour to that promontory. Why, then, if Cape Grisnez was the Itian promontory, should Boulogne not have been called the Itian harbour? Even on the desperate theory that when Caesar spoke of a harbour, he did not mean a harbour but only a roadstead, that roadstead was not at Wissant; for if Caesar's ships had waited there, either at anchor or on the beach, exposed to the north-west winds for twenty-five days, they would have been in extreme peril.

Dr. Guest admits of course that Boulogne, not Wissant, was the permanent harbour of the Romans in North-Eastern Gaul under the empire; but in this fact he sees no objection to his theory. He believes that the Romans, when they had to choose a permanent harbour, rejected Wissant and chose Boulogne because of the sterility of the country in the neighbourhood of the former. 'Wissant,' he remarks, 'or rather the port adjacent to Wissant, may have answered Caesar's purpose, when he had hundreds of ships to supply the wants of his commissariat; but when a port was to be provided to meet the ordinary purposes of traffic, it was necessary to select one that possessed local resources.'[1]

The reason which Dr. Guest gives for the choice of Boulogne is sound enough as far as it goes; but what support does it lend to the theory that Caesar used Wissant as a temporary harbour? The sterility of the neighbourhood would hardly have recommended it. It must have had some great advantage to compensate for this defect if it was really to be preferred, even as a temporary harbour, to Boulogne. But it is impossible to point out one single advantage which Wissant could have had, for Caesar's purpose, over Boulogne, save only that, as the crow flies, it was a little nearer to Britain.

Dr. Guest, indeed, assures us that 'Caesar had no time for weighing the comparative merits of the ports north and south of him, and for determining which of them was "the most convenient"'.[2] No time! Had he not five days to spare for Volusenus's reconnaissance? A single day would have sufficed to ride along the coast from Wissant to Boulogne; a few minutes spent at each of those places would have sufficed 'for determining which of them was "the most convenient"': but the greatest general of Rome could not spare even one day for a duty which the worst would not have neglected; so he pitched upon Wissant, because, as Dr. Guest tells us, 'it afforded him the shortest passage'! So argued the man who, according to Freeman, 'settled the whole matter,' the man who, from Freeman's point of view, appeared to stand, side by side with Stubbs, 'at the head of living students of English history.'[3]

3. Heller is not as ardent an advocate of Wissant as Guest; but he has written some very ingenious papers in defence of Guest's view. Many of his reasons are virtually identical with those of the English scholar; but from Caesar's narrative of his second voyage he deduces a fresh argument, which deserves special attention. Caesar,

[1] *Archaeol. Journal*, xxi, 1864, pp. 224–5.
[2] *Ib.*, pp. 221–2.
[3] *Norman Conquest*, i, xv.

as we have seen, set sail about sunset with a light south-westerly wind. About midnight the wind dropped: the fleet, borne by the tide, drifted out of its course;[1] and 'at daybreak Caesar saw Britain lying behind on the port quarter' (*orta luce sub sinistra Britanniam relictam conspexit*[2]). From the last statement Heller infers that Caesar's ship must have drifted to some point off the North Foreland: otherwise, he argues, the word *relictam* would be meaningless. For, he remarks, Caesar believed that one side of Britain faced the north. Therefore it must be assumed that he had no knowledge of that part of the coast which trends northward beyond the mouth of the Thames: he must have thought that the coast, at the North Foreland, turned sharply towards the west. Otherwise he could not have believed that he had left Britain behind; nor could he have believed this unless he had drifted to some point off the North Foreland. Now Caesar started on his voyage about the 6th of July.[3] On that day the sun set about 8.16; and on the following morning it rose about 3.54. There must have been light enough to show the British coast as early as 3.15 or 3.20. Heller maintains that Caesar could by daybreak have reached a point about 2 German [or 9½ English] miles south-east of the North Foreland, not quite as far north as the latitude of Ramsgate, if he had sailed from Wissant; but he insists that if he had sailed from Boulogne, he could not have drifted further northward than the latitude of Deal, in which case he could not have said that he 'saw Britain left *behind* on the port quarter'.[4]

This argument rests upon a strained interpretation of the word *relictam*. It is probably true that Caesar could not have drifted as far north as the latitude of Ramsgate if he had sailed from

[1] *Longius delatus aestu* (*B. G.*, v, 8, § 2). According to Long (*Decline of the Roman Republic*, iv, 1872, p. 204), 'the expression "too far" (*longius*) means that he was carried too far north and past the place where he had landed the year before.' But as the direction of the current was ENE. (magnetic), the smallest drift would have been too far.

[2] *B. G.*, v, 8, § 2.

[3] See pp. 728–30, *infra*.

[4] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, pp. 122–3. There is an obvious objection to this argument, to which Heller replies by anticipation. One of the three sides of Britain, says Caesar, looks southward towards Gaul; and one of the 'angles' (*alter angulus*) of this side is by Kent (*B. G.*, v, 13, § 1). If Caesar landed, as Heller believes, between the South Foreland and the North Foreland, he had himself seen this angle, which is formed by the South Foreland; and if he believed that the coast, at the North Foreland, turned sharply towards the west, and had no knowledge of that part of the coast which trends northward beyond the mouth of the Thames, it is clear that he must have regarded the North Foreland and not the South Foreland, as marking the commencement of the northern side, in which case one might think that he would have described the coast between the South Foreland and the North Foreland as a separate side, and would have represented Britain not as triangular but as irregularly quadrilateral. But Heller argues that the word *angulus*, as used by Caesar, does not mean an 'angle' in the geometrical sense of the word, but only a strip of coast between two angles; and he compares a passage in Livy's description of the siege of Saguntum (xxi, 7, § 5),—*Angulus muri erat in planiorem patentioremque quam cetera circa vallem vergens.*

Boulogne ;[1] but even if he had only drifted as far north as the latitude of Walmer, he would have been perfectly justified in using the word *relictam*. For that word does not imply that Caesar believed himself to have left the *northern* coast of Britain behind : it simply implies that, as the current was carrying him in a north-easterly direction[2] and therefore sweeping him every minute further and further away from Britain, ' he saw Britain lying behind on the port quarter.' There is a parallel passage in the twenty-first chapter of the *Acts of the Apostles*, which shows that this was his meaning. In the second and third verses of that chapter the writer, after describing the voyage of himself and St. Paul from Ephesus by way of Cos and Rhodes to Patara, says, ' Having found a ship crossing over unto Phoenicia, we went aboard, and set sail. And *when we had come in sight of Cyprus, leaving it on the left hand*, we sailed unto Syria, and landed at Tyre ' (*καὶ εὑρόντες πλοῖον διαπερῶν εἰς Φοινίκην ἐπιβάντες ἀνήχθημεν, ἀναφάναντες δὲ τὴν Κύπρον καὶ καταλιπόντες αὐτὴν εὐώνυμον ἐπλέομεν εἰς Συρίαν καὶ κατήλθομεν εἰς Τύρον*). If the reader will look at his map, he will see that the writer of the *Acts*, when he came in sight of Cyprus and left it on the left hand, was in precisely the same position with regard to Cyprus as Caesar would have been in with regard to Britain if, drifting in a north-easterly direction, he had descried the coast of Britain from some

[1] Caesar started ' about sunset ' (*ad solis occasum*) : the wind dropped ' about midnight ' (*media circiter nocte*) ; and the drift ceased at daybreak (*orta luce*). The sun set at 8.16 p.m. ; and day broke about 3.20 a.m. It would be absurd to suppose that the voyage must have begun at the moment when the sun dipped under the horizon : we may fairly assume that it began at any time between 7 and 8. Similarly the drift may be assumed to have begun at any time between 11 p.m. and 1 a.m. Let us suppose that the wind lasted 5 hours, and the drift 3 hours. For some time before the wind dropped it must have been gradually dying down ; but, as the vessels kept steerage way, it may be assumed, so Captain Iron, the harbour-master at Dover assures me, that, even during the drift, there was not a dead calm. Major Rennell, indeed, affirms (*Archaeologia*, xxi, 1827, p. 503) that when the wind dropped the ships were ' left to the resources of their oars ' : but Caesar does not confirm this ; and if the oars had been used, why should the ships have drifted out of their course ? Captain Iron says that with a light south-westerly wind the ships could easily have sailed 6 knots an hour. The voyage took place about the time of new moon (see pp. 728–30, *infra*), that is to say, a day or two before spring tide. For the first two hours the ships had to encounter the ebb tide, the rate of which, however, was not more than one knot an hour ; and the flood, the rate of which increased from about three-quarters of a knot to nearly 3 knots, helped them from about 9 or 9.30 p.m. We may estimate, then, that in the 5 hours they sailed not less than 25 knots ; while in 3 hours, aided by a faint breeze, they would have drifted about 6 knots (see p. 656, *infra*). I think, then, that the entire distance which Caesar sailed up to the moment when he ' saw Britain lying behind on the port quarter ', may be estimated at not less than 31 knots, or about 57½ kilometres. After making this calculation, which must be taken for what it is worth, I find that, according to Napoleon's map (*Hist. de Jules César*, Atlas, pl. 16), the distance was 57 kilometres. The reader may check my estimate by referring to the Admiralty *Tide Tables*, pp. 112–9, and *Tidal Streams, English and Irish Channels*.

[2] See *Tide Tables for the British and Irish Ports*, p. 119.

point in the latitude of Deal.[1] And if Heller will use his common sense he will see that if a ship about the latitude of Deal were drifting away from Britain, that ship would have left Britain behind just as really as if it had passed Cape Wrath and were drifting towards Iceland.

Lastly, even if Heller's explanation of the word *relictam* were correct, the argument which he builds upon it would be unsound; for obviously that argument would only hold good if Caesar had drifted north of the latitude of the North Foreland. Heller himself admits that he had hardly drifted so far north as the latitude of Ramsgate; and at this point, on Heller's own theory, he could no more have said that he had left *the northern coast* of Britain *behind* than if he had been in the latitude of Deal.

Both Heller[2] and Guest[3] deduce an argument in favour of Wissant from a well-known passage of Strabo.[4] It runs as follows:— 'There are four regular passages from the Continent to the island, namely, from the mouths of the Rhine, the Seine, the Loire, and the Garonne. People who cross from the country near the Rhine do not sail from the mouth of that river, but from the country of the Morini . . . *and* in their country is the Itian (harbour), which Caesar used as his naval station, when he was crossing to the island' (*τέτταρα δ' ἐστὶ διάρματα, οἷς χρῶνται συνήθως ἐπὶ τὴν νῆσον ἐκ τῆς ἠπείρου, τὰ ἀπὸ τῶν ἐκβολῶν τῶν ποταμῶν, τοῦ τε Ῥήνου καὶ τοῦ Σηκοάνα καὶ τοῦ Λείγηρος καὶ τοῦ Γαρούνα. τοῖς δ' ἀπὸ τῶν περὶ τὸν Ῥῆνον τόπων ἀναγομένοις οὐκ ἀπ' αὐτῶν τῶν ἐκβολῶν ὁ πλοῦς ἐστιν, ἀλλὰ ἀπὸ τῶν ὁμορούντων τοῖς Μεναπίοις Μορινῶν, παρ' οἷς ἐστι καὶ τὸ Ἴτιον, ᾧ ἐχρήσατο ναυστάθμῳ Καῖσαρ ὁ θεός, διαίρων εἰς τὴν νῆσον*). I have italicized the word *and*, because the meaning of *καί* has been disputed. Dr. Guest argues that the port from which the inhabitants of the country near the Rhine sailed must have been Boulogne; and, he continues, 'every unprejudiced reader . . . will be of opinion that he (Strabo) distinguishes it from his "Itium".' In other words, Guest would translate the doubtful clause by 'in whose country there is *also* the Itian (harbour)'.

I, for one, fully agree with Dr. Guest; but some scholars are unable to do so. Long,[5] remarking that a similar use of *καί*, particularly in clauses which begin with a relative, as *παρ' οἷς*, is common in Strabo and also in Thucydides, affirms that 'the purpose of *καί*, when it is so used, is to mark emphatically some thing or circum-

[1] For this reason I attach no importance to Heller's remark (*Philologus*, xlix, 1890, p. 692), that, if Caesar had only drifted as far northward as the latitude of Deal, he would have written, not *sub sinistra Britanniam relictam conspexit*, but *longius se a Britannia recessisse animum advertit*.

[2] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, p. 176; *Philologus*, xlix, 1890, pp. 691–2.

[3] *Origines Celticae*, ii, 362.

[4] *Geogr.*, iv, 5, § 2.

[5] *The Reader*, Sept. 19, 1863, p. 317. 'What Strabo says,' writes Long (*ib.*, p. 414), 'is quite irrelevant to the matter in discussion, which must be decided by Caesar's text.' After which Long proceeds to devote a column and a half to arguing for his own view of what Strabo said.

stance in addition to one which has been mentioned'. Guest [1] retorts that Xylander, in his Latin version of Strabo, first published in Casaubon's edition of 1597, and revised by Siebenkees, who did not alter Xylander's translation of the passage in question, and Groskurd, in his German version of 1831—'the most careful and conscientious translation of Strabo that has yet appeared'—both render καί by 'also'. This array of authorities does not disconcert Long. He remarks [2] that the old Latin versions of Strabo and other Greek writers, although they were very useful in their day, 'are not of much value when there is any great difficulty.' Groskurd's translation—'wo auch der Hafen Ition ist'—he regards as ambiguous. Had Groskurd desired to express, in his translation, that Strabo meant to affirm the existence of another harbour, besides those which he had just mentioned, he would have written, not 'wo *auch*', but 'wo *ebenfalls*' (der Hafen Ition ist). But, says Long, 'Strabo mentions four usual points of transit from Gallia to Britain, and if in this passage he means that there was another besides the Itius, then there would be five points of transit instead of four, and Strabo would contradict himself.' [3] Long then quotes two passages in support of his interpretation of καί. 'Strabo,' he remarks, 'says that Sinuessa is in the gulf of Setia, and adds ἀφ' οὗ καὶ τὸ ὄνομα.[4] Groskurd translates καί by "auch", which has no meaning here ... Again, Strabo, speaking of the high Alps, says περὶ ὃ δὴ καὶ συνίσταντο οἱ λῃσταί; [5] which Groskurd translates, "die Gipfel, um welche denn auch die Räuber sassen." Xylander simply says "ubi degebant latrones", which I prefer to Groskurd's version, though Xylander's version is not quite exact.'

To these arguments Guest made no reply; but Heller [6] did so. He admits that Thucydides, in relative sentences, often did use καί in the sense which Long claims for it; but this sense, he maintains, is restricted to phrases of which the meaning is unmistakable.

If Strabo did really mean to say that the Itian port was different from that port of the Morini which was commonly used as the point of departure for Britain, then I can only say that I believe, with

[1] *Origines Celticae*, ii, 368–70.

[2] *The Reader*, Oct. 10, 1863, p. 414.

[3] Long forgets that Strabo does not expressly say that 'the Itius' was a usual point of transit; he only says that Caesar used it as his naval station. If 'the Itius' was identical with the port used by the passengers who 'cross from the country near the Rhine', it was 'a usual point of transit'; but it is precisely this identity which is the subject of dispute.

[4] *Geogr.*, v, 3, § 6. [5] *Ib.*, iv, 6, § 9.

[6] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, p. 176. Heller puts the matter very clearly,—'aber diese Ausdrucksweise ist auf gewisse leicht erkennbare Wendungen beschränkt. Jedesmal jedoch, wo καί weder die intendirende Kraft (in der Bedeutung "sogar") besitzen kann, noch eine Hinzufügung begleitender Umstände vermittelt ("zugleich auch", "denn auch") noch auch verallgemeinernde Bedeutung hat ("auch immer"), kann es, wie hier, nur das Hinzutreten einer neuen Person oder Sache einleiten . . . es darf deshalb gar kein Zweifel darüber aufkommen, dass Strabo in der That den *portus Itius* von dem gewöhnlichen Hafen der Moriner hat unterscheiden wollen.'

Rudolf Schneider,[1] that Strabo was mistaken. As Caesar was the only other ancient writer who mentioned the Portus Itius, and as he did not say exactly where it was, it would have been quite natural for Strabo to suppose that the Portus Itius was not the same as the well-known port of the Morini.

4. Finally, it has been affirmed by Henry [2] and many other writers that the so-called 'Camp de César' and the various hillocks known as 'mottes' which are to be found in the neighbourhood of Wissant were defensive works erected by Caesar or his lieutenants for the protection of the Portus Itius; and de Saulcy tells us that an inhabitant of Wissant, whose trustworthiness he had proved, informed him that about two kilometres north-east of the village, at Haute-Sombre, there existed a camp several hectares in extent, in which, he says, 'il faudra reconnaître le camp des trois légions et des deux mille cavaliers de Labienus.' [3]

All these allegations have been disproved. The so-called tradition which ascribed the 'Camp de César' to the invader of Britain originated in the eighteenth century: [4] at all events it is not mentioned by any of the earlier advocates of Wissant; and the camp has been proved to be of post-Roman date.[5] Moreover, its area is not more than 50 ares 30 centiares, or 6,016 square yards, less than one acre and a quarter, which would not have sufficed to accommodate more than 500 men.[6] As for the 'mottes', they have been excavated, and have been proved to be simply *tumuli*, which contained skeletons, flint implements, and bone pins. And the Abbé Haigneré [7] has shown, in an amusing paragraph, that the so-called camp of Labienus, which, needless to say, is not marked on the *Carte de l'État-Major*, is purely imaginary.

Every argument which has been adduced in favour of Wissant has now been examined; and if I could have accepted them or any one of them, I would gladly have done so, for I myself once argued that the Portus Itius was at Wissant. But my knowledge was then imperfect. It is not possible to prove that the Portus Itius was at Wissant: it is possible to prove that it was not.

1. Although Wissant is nearer to England than Boulogne, yet Caesar would have gained nothing, even in regard to his mere voyage, by making Wissant his place of departure. Captain Iron, the harbour-master of Dover, unhesitatingly affirmed, after we had studied the chart together, that the fleet would have 'made a better run' from Boulogne than from Wissant. The reader will have no difficulty in understanding this if he will consult the Admiralty Chart (*Dungeness to the Thames*), and the Atlas entitled *Tidal*

[1] *Portus Itius*, p. 13. Schneider, like myself, accepts Heller's interpretation of Strabo's meaning.
[2] *Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, pp. 46, 48.
[3] *Les campagnes de Jules César dans les Gaules*, pp. 183–4.
[4] D. Haigneré, *Recueil hist. du Boulonnais*, i, 96.
[5] Napoléon III, *Hist. de Jules César*, ii, 1866, p. 168.
[6] *Congrès arch. de France*, xxviie session, 1860 (1861), pp. 69–70.
[7] *Étude sur le Portus Itius*, pp. 72–7; *Recueil hist. du Boulonnais*, i, 96–8.

Streams in the English and Irish Channels. It must be remembered that both in 55 and in 54 B.C. Caesar started from Gaul when the tide was running down the Channel; and that on his first voyage the tide turned north-eastward between 4.30 and 5 a.m., when he had been three or four hours at sea, and on his second about 9.30 p.m., two hours or so after he had set sail. Thus, on each occasion, the latter and greater portion of the voyage was made on the flood tide.[1]

Wauters does indeed succeed in proving that, in the middle ages, Wissant was very frequently used as a place of embarkation by travellers, merchants, and even troops sailing for the opposite coast;[2] and the point of his argument is that if, in the middle ages, a large army could embark at Wissant, Caesar's army could have done the same; and that if Wissant was a convenient point of departure for a voyage to Britain in the middle ages, it was not less convenient in the time of Caesar. Haigneré[3] retorts, truly enough, that the quantity of merchandise which passed through the port at any one time was very small, and that, as a rule, not more than two or three vessels left the port simultaneously; but when he affirms[4] that the largest army which ever sailed from Wissant was a force of 2,000 men, which John of Hainault led in 1327 to assist Edward the Third against the Scots,[5] he lays himself open to criticism. Wauters[6] replies that the force with which the Earl of Leicester sailed from Wissant in 1173 must have been very large; for in the battle of Fornham, in which the earl suffered defeat soon after he had landed in Suffolk, 10,000 of his men were killed. This statement, which was accepted by the late Bishop of Oxford,[7] was made on the authority of Benedict of Peterborough,[8] who also describes the army of the Earl of Leicester as *infinitus exercitus.* But (if we are to accept the statement of a mediaeval monk as to the number of men who were killed in a battle) Benedict does not say that *the army* set sail from Wissant, while Ralph de Diceto[9] merely says that the Earl of Leicester embarked in a ship at Wissant, accompanied by a numerous band (*venit apud Witsant, ubi . . . plurima comitante caterva, navem ascendit*); and, assuming that the troops

[1] On the day of Caesar's first landing in Britain high water at Dover was about 6.21 a.m. (see pp. 610–1, *infra*); and when he was approaching the British coast in 54 B.C., the tide turned south-westward about ten miles east of Deal soon after daybreak, which was about 3.15 a.m. See pp. 655–9, *infra.* Supposing that Caesar landed in Britain in 55 B.C. on the 27th, not, as I believe (see p. 601, *infra*), on the 26th of August, it still remains true that the latter part of the voyage was made on the flood.

[2] *Bull. de l'Acad. Roy. . . . de Belgique*, 2e sér., xlvii, 1879, pp. 134–61.

[3] *Recueil hist. du Boulonnais*, ii, 439–40.

[4] *Étude sur le Portus Itius*, p. 32.

[5] See *Œuvres de Froissart,—Chroniques*, ii, 1867, p. 109 (ed. Kervyn de Lettenhove).

[6] *Bull. de l'Acad. Roy. . . . de Belgique*, 2e sér., xlvii, 1879, pp. 144–5.

[7] *Const. Hist. of England*, i, 1880, p. 540.

[8] *Gesta regis Henrici secundi Benedicti abbatis*, ed. W. Stubbs, i, 1867, p. 60.

[9] *Radulfi de Diceto . . . opera hist.*, ed. W. Stubbs, i, 1876, p. 377.

all embarked at Wissant, there is no evidence that all the transports which carried them sailed together. It is generally admitted even by the partisans of Wissant (though not by Dr. Guest) that the mediaeval port was merely the creek formed by the Rieu d'Herlan, otherwise called Rieu de Sombre; and if it is true that an army which lost 10,000 men in a single battle embarked at Wissant in 1173, the bulk of the ships which carried it must have been anchored in the roadstead. The frequency with which Wissant was used as a place of embarkation in the middle ages undoubtedly proves that it was convenient, and this fact has been slurred over by the advocates of Boulogne; but it nevertheless remains certain that Caesar would not have found it convenient to sail from Wissant when *the greater part of his voyage would have to be made upon the eastward stream, and with a south-west wind.* At the same time I admit that we do not know from what quarter the wind was blowing in his first voyage: we only know that when he set sail it was favourable.[1]

2. There is another objection to Wissant, which Dr. Guest, if he had been consistent, would have been the first to urge. Like all the other advocates of Wissant, he identifies the *superior portus* with Sangatte. Yet he tells us himself that it is hard to see how there could ever have been a harbour at Sangatte.[2] Similarly, H. L. Long, himself an advocate of Wissant, who was well acquainted with the coast between Boulogne and Calais, observes that 'as a port, in our acceptation of the term, Sangatte has fewer pretensions . . . than even Wissant; but still it is, and always has been a small station'.[3] The theory that it was a naval station is no doubt supported by the fact that it was the terminus of a Roman road: but Caesar speaks of a *portus*; and when Dr. Guest has to confute George Long, he is most emphatic in insisting that *portus* means 'a harbour' in the strictest sense of the word.[4]

Let us, however, assume that Sangatte may conceivably have possessed a harbour in Caesar's time. Even so, it is impossible to admit that Sangatte can have been the *superior portus*. For, if the eighteen ships which carried the cavalry had started from Sangatte, the conditions of wind and tide which would have rendered a voyage from Wissant to Kent less favourable than from Boulogne would obviously have been more unfavourable still.

3. There is one passage in Caesar's narrative which, to a sailor, would be alone sufficient to prove that Wissant was not the port

[1] *B. G.*, iv, 23, § 1.

[2] 'It has no port, nor is it easy to see how it ever could have had one . . . Possibly Cape Blanc-Nez may have projected further seawards two thousand years ago than at present, and so have afforded it something like a shelter from the south-west wind.' *Archaeol. Journal*, xxi, 1864, p. 221, note. But Cape Grisnez would equally have afforded Wissant 'something like a shelter from the south-west wind'; yet Dr. Guest implies that at Wissant there could have been no harbour unless it had been protected by sand-dunes. And what about the north-west, the north, and the north-east wind?

[3] *Gentleman's Magazine*, xxvi, 1846, p. 256.

[4] *Origines Celticae*, ii, 363.

from which Caesar sailed in 55 B.C. We have seen that the gale which drove some of the cavalry transports from the point where they were first sighted westward down the coast carried the others back to the port from which they had started. I will assume that the latter were laid to on the port tack:[1] if they could not work to windward, a glance at the map will show that they could not have returned to any point east of Wissant. The gale must obviously have blown from some point between the east and the north; and, if Caesar sailed from Wissant, the place from which the transports started must, as we have seen, have been Sangatte. Now it is absolutely incredible that a gale which drove some of these ships from a point near the South Foreland[2] westward down the coast should have carried the others back to Sangatte. Caesar says that the former were 'in great peril', and that, when they anchored, the waves broke over them. A sailor would at once understand what their peril was. They were in no danger of being driven ashore; for while the gale was at its height they stood out to sea.[3] They ran before the wind; and they were in danger either of broaching to or, possibly, of being 'pooped'.[4] From this we should conclude that the wind, when it struck the ships somewhere east or north-east of the South Foreland,[5] blew from about the north-east: indeed, as the waves broke over the ships, it may have blown from north-east by east; for, if it had blown from the north-east or north-east by north, the ships, if they anchored close in shore, west of Folkestone, would have been in a sheltered position.[6] The most easterly point at which they can be assumed to have been when they were caught by the gale is NW. 4° N. of Sangatte. Therefore if the wind had blown from the north-east, the ships that were carried back to the port from which they had started would have had to sail within less than eight points and a half of it in order to reach Sangatte. But, as Falconer[7] says, a ship laid to in a gale makes from 5½ to 6½ points of lee-way. Reduce this estimate to four, and you will see that the transports would have had to lie within less than four points and a half of the wind in order to make Sangatte. No ancient ship could have done this. Close-hauled and under short canvas, as they would necessarily have been, the transports,

[1] This assumption, with the condition that the ships could work to windward, is approved by Captain Iron and by Commander Boxer, R.N., the harbour-master of Folkestone.

[2] I assume what I shall afterwards prove (see pp. 595–665, *infra*), that Caesar landed in 55 B.C. between Walmer and Deal. If he had landed near Hythe or Lympne, the force of my argument would of course be increased.

[3] *B. G.*, iv, 28, § 2.

[4] See James Smith, *Voyage and Shipwreck of St. Paul*, 4th ed., 1880, p. 113, and Adm. W. H. Smyth, *The Sailor's Word-Book*, 1867, p. 598.

[5] A wind blowing from the north-east off Walmer or Kingsdown would be diverted a point or two southward off the south coast.

[6] The statement in the text, which will commend itself to every one who reflects that the heights between Folkestone and Hythe would have afforded protection from the wind, is made with the approval of a Deal boatman and an ex-warrant officer who knows every inch of the Kentish coast.

[7] *Marine Dictionary*, 1815, p. 220.

as Commander Richmond remarked to me, would ' just have sagged to leeward '. It may be objected that the tide would have helped them if it was running up the Channel. But the flood tide is almost neutralized by a north-easterly gale, and simply makes the sea more vicious : the ships would have moved so slowly that they could not have crossed the Channel in one tide ; and when it turned and began to sweep them westward, their prospect of reaching Sangatte would have been more hopeless than ever. With a north-easterly gale, or even one which blew from north-east by north, it would have been absolutely impossible, so Commander Richmond and the harbour-master of Dover have separately and independently affirmed, for the vessels to fetch that anchorage.[1]

4. Desjardins[2] shows that, whereas four Roman roads, meeting at Gesoriacum, are mentioned in the itineraries,[3] not a single Roman road led to Wissant. The advocates of Wissant have, indeed, replied that this proves nothing, since, in Caesar's time, there were no Roman roads in any part of Gaul.[4] But this reply is nugatory. Since no Roman roads led to Wissant, it is clear that if Wissant was the Portus Itius, this harbour, which Caesar had ascertained to be ' the most convenient ' port of departure for Britain, was regarded by his successors as useless. Such a hypothesis is not tenable.

5. The mention of roads suggests another objection to Wissant. We have seen that Caesar's army, consisting of five legions and 2,000 cavalry, remained weatherbound at the Portus Itius in 54 B.C. for about twenty-five days ; and that with them were three other legions and 2,000 cavalry, who were left behind under the command of Labienus to guard the ports during Caesar's absence.[5] Thus for twenty-five days a force amounting to at least 32,000 men and 4,000 horses had to be fed ; and of these not less than 12,500 men and 2,000 horses for about ten weeks more.[6] No calculation is needed to show that these multitudes could not possibly have been supplied

[1] See Addenda, p. 740.

[2] *Géogr. de la Gaule rom.*, i, 356–7, 388.

[3] *Itin. Ant.*, ed. Wesseling, pp. 356–63, 376–7 ; *La Table de Peutinger*, ed. Desjardins, p. 12, col. 3, p. 13, col. 1–3, p. 22, col. 1–3. The advocates of Wissant have pointed to two roads which connected Wissant with Thérouanne. One of these, known as the *voie de Leulene*, leads from Thérouanne to Sangatte, and, near Guines, throws out a branch, which terminates at Wissant ; the other, called the *chemin vert*, leads to Wissant direct. Roman remains have been found on the *voie de Leulene*, but none on the branch ; while both on the *chemin vert* and on the branch road excavations have been made which proved that neither was a Roman road. See D. Haigneré, *Étude sur le Portus Itius*, pp. 100–1, 103 ; and *Mém. de l'Acad. d'Arras*, xxxv, 1863, pp. 272-3.

[4] C. du Fresne, *Dissertatio de portu Iccio*, p. 95. See also A. Wauters (*Bull. de l'Acad. Roy. . . . de Belgique*, 2e sér., xlvii, 1879, p. 130).

[5] *B. G.*, v, 5, § 3 ; 7, § 3 ; 8, §§ 1–2.

[6] I say ' at least ' advisedly. In order to understate my case, I have assumed that the legions were of the exceptionally low average strength of 3,500 men (*B. G.*, v, 49, § 7 ; Rice Holmes, *Caesar's Conquest of Gaul*, pp. 563–7), and have not counted auxiliaries, although there were certainly both slingers and archers (*B. G.*, iv, 25, § 1). Probably we should be within the mark if we estimated the force at 40,000 infantry and auxiliaries, besides the 4,000 cavalry.

by the country in the neighbourhood of Wissant, even if it were as fertile as (according to Dr. Guest himself) it is 'notoriously barren'.[1] Their food must have come from a distance; and to transport it to Wissant without roads would have been a task of extreme difficulty. Dr. Guest assumes that Caesar's fleet would have supplied his wants.[2] But the fleet could only have procured grain from a port. Surely, then, Caesar would have found it most convenient to start from a port which was in communication by road or by river with the interior. Such a port was Boulogne, which enjoyed both these means of transit. What would have been gained by abandoning it for the isolated Wissant?

Again, it will be remembered that Labienus built sixty ships during Caesar's absence in Britain;[3] and we have seen that most of the modern advocates of Wissant admit that there was no harbour there, except the tiny creek formed by the ruisseau d'Herlan, or possibly a roadstead which may have been partially sheltered by the Banc de Laine. Not one of them has attempted to explain how Labienus could have found the means of building sixty ships upon an exposed beach. But let us admit that his genius could have improvised dockyards.[4] Let us even admit that the harbour of Dr. Guest's imagination did really exist. Still, sixty ships cannot be built without timber. How was all this timber to be brought to Wissant without roads and without a river? Even assuming that there was a Gallic road, it is doubtful whether Labienus could have impressed the amount of carriage necessary to transport the timber from the forests. But a few miles off at Boulogne the difficulty would have disappeared.[5]

6. Another objection is so obvious that it must impress every candid inquirer. If Wissant was the Portus Itius, why was Wissant never once mentioned during the first millennium of our era? There is no evidence worthy of the name that it was used as a port before the year 1013.[6] It is surely inexplicable that the port which Caesar regarded as the most convenient for his purpose should have been found so inconvenient or so superfluous by his successors that during the imperial epoch it fell into entire disuse. Wauters indeed retorts that if Wissant was eclipsed by Gesoriacum under the Empire, so was the Gallic town of Bibracte by the Gallo-Roman Augustodunum, and that, although the naval station was Gesoriacum,

[1] See E. B. Hamley's *Operations of War*, 4th ed., 1878, pp. 34, 37.
[2] *Archaeol. Journal*, xxi, 1864, pp. 224–5.
[3] *B. G.*, v, 23, § 4.
[4] See A. E. E. Desjardins, *Géogr. de la Gaule rom.*, i, 362.
[5] I am aware that, according to Froissart (*Chroniques*, ed. Kervyn de Lettenhove, v, 1868, pp. 182–3), timber was conveyed after the battle of Crecy from the forests of the Boulonnais to Wissant by men and horses. But Wissant was then connected with the interior by roads.
[6] The very earliest mention of Wissant to which its advocates can point refers to the year 566. But the anonymous life of St. Vulgan, in which the reference is to be found, is a work of no authority. See *Mém. de l'Acad. d'Arras*, xxxv, 1863, p. 253, and A. E. E. Desjardins, *Géogr. de la Gaule rom.*, i, 351–2, note.

Wissant *may* have been a great commercial port.[1] But he omits to explain how a great commercial port could have been left unnoticed by history, or how it could have existed without a river and roads to connect it with the interior. Nor is there any analogy between Wissant and Bibracte. The hill-fort of Bibracte gradually fell into disuse because when Gaul settled down under the Roman dominion it was no longer required.[2]

7. Finally, Mariette[3] argues that the mere name of Wissant, which, like the names of many other villages in the Boulonnais, is of German origin, proves that it was not founded before the fifth century, and consequently that there could have been no frequented harbour there in Caesar's time.

It has now been demonstrated that Caesar did not sail from Wissant. That it was the point of departure of his first expedition is out of the question; for in that case the *portus ulterior*, from which the cavalry transports set sail, must have been Sangatte; and we have seen that they could not have returned to Sangatte when they were dispersed by the gale. The *portus ulterior* can only have been Ambleteuse; and therefore Caesar sailed in 55 B.C. from Boulogne. But nobody will believe that, having had experience of the advantages of Boulogne, he would have discarded it in favour of a place which, for his purpose, was in all respects inferior.

Nevertheless, to satisfy doubters, I shall state the case for and against Boulogne.

IX. BOULOGNE

The reasons which point to the identification of the *Portus Itius* with Boulogne are, speaking generally, that Boulogne, and Boulogne only, satisfies all the requirements of Caesar's narrative.

To begin with, the passage for sailing-vessels from Boulogne to the south-eastern part of Britain is, and always has been, in circumstances such as Caesar described, not only very convenient but by far the most convenient. This is the testimony of seafaring men, both English and French, who have practical experience of the winds and the currents in the Channel: it was admitted, or rather strenuously maintained, by Henry,[4] who advocated the claims of Wissant; and any man who studies the Admiralty Chart—*Dungeness*

[1] *Bull. de l'Acad. Roy. de Belgique*, 3e sér., xviii, 1889, pp. 415, 421.

[2] Cf. E. Lavisse, *Hist. de France* (tome i, by G. Bloch, 1901, pp. 197–8).

[3] *Lettre à M. Bouillet*, &c., pp. 26–7. Haigneré (*Étude sur le Portus Itius*, p. 122) argues that if Caesar started on his first voyage from Wissant, it is impossible to account for the fact that, on the return voyage, two of his ships failed to make the same harbours as the rest, that is to say, Sangatte and Wissant. Those two ships could not, he insists, have drifted further down the coast, that is to say, southward of Cape Grisnez, unless the wind had been unfavourable; and if the wind was unfavourable, how was it that the remaining ships succeeded in making the harbours? Captain Iron, however, attaches no importance to this objection.

[4] *Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, p. 130.

to the Thames—the *Channel Pilot*, and the Atlas, published by the Admiralty, which is entitled *Tidal Streams in the English and Irish Channels*, may convince himself of its truth. Captain Pollet, the harbour-master of Calais and Boulogne, furnished information to Ernest de Saulcy, who was determined, by hook or by crook, to make out a plausible case in support of Wissant; but he avowed his own opinion that Caesar must have sailed from Boulogne.[1] Captain Iron, the harbour-master of Dover, in conversation with me, has done the same.

Secondly, the whole of Caesar's fleet could easily have assembled in the port of Boulogne; and they certainly could not have assembled in any other port, properly so-called, on the coast of the Morini,[2] except the mouth of the Canche, which was several miles further from Britain than Boulogne, and was in no respect more convenient. Desjardins[3] has shown that the estuary of the Liane was much broader and deeper in Caesar's time than it was in the nineteenth century before the harbour was modernized, and that, as the headland which sheltered it has suffered greatly from erosion, it extended further seaward; and not only was it ample in extent, but it was the only port protected from every wind.[4] No one has described its merits more eloquently than Henry, the advocate of Wissant; and no one was more competent to form an opinion. He describes Gesoriacum as 'le havre le plus commode et le mieux situé de toute la Gaule-Belgique, pour le commerce, la construction et l'équipement des vaisseaux'.[5] But, although it is certain that there would have been ample room in the Liane for Caesar's 800 small vessels,[6] Airy insists that it would have been impossible for them to clear the harbour in a single tide.[7] Now Caesar does not say that they did clear the harbour in a single tide; nor is it necessary to assume that they did. Captain Iron has, however, assured me that Caesar's fleet of shallow vessels could have cleared the harbour in a single tide even if the depth of the water then had been no greater than in 1877. In that year the depth *at low tide* was 1 metre 60, or more than 5 feet[8]; and it may be regarded as certain that the draught of Caesar's vessels in the second expedition was

[1] *Bull. de la Soc. acad. de . . . Boulogne-sur-mer*, i, 1873, pp. 132–3.

[2] I mean, of course, on any part of the coast which can be regarded as lying within the limits required by Caesar's narrative. The estuary of the Authie is about 11 miles further south than that of the Canche; and the estuary of the Somme is, as we have seen (pp. 558–63, *supra*) inadmissible.

[3] *Géogr. de la Gaule rom.*, i, 376–80, and pl. xv. See also T. Lewin, *The Invasion of Britain*, &c., 1862, pp. c–ci, and *Boulogne-sur-mer et la région boulonnaise*, i, 1899, p. 708. 'Au xive siècle,' says M. Lejeal (*ib.*, p. 369), 'la mer pénétrait encore jusqu'à Isques.'

[4] D. Haigneré, *Recueil hist. du Boulonnais*, ii, 416, 420–4.

[5] *Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, p. 63.

[6] See a map in the British Museum, called *Plan général du port de Boulogne, avec les dispositions proposées . . . pour sortir du port dans une marée 300 batimens portant une armée de 60,000 hommes*, 1822. This from the small modern port.

[7] *Archaeologia*, xxxiv, 1852, p. 236.

[8] E. Reclus, *Nouv. Géogr. Univ.*,—*La France*, 1877, p. 792.

much less than five feet.[1] The estuary of the Liane has been silted up so much since Caesar's time that it would hardly be an exaggeration to say that its depth then was three times as much as in 1877 ;[2] and it has been ascertained from the sinking of artesian wells at the cement works of M. Demarle at Capécure[3] that at that place the ancient bed of the river is 19 metres below the level of spring tides.[4]

Thirdly, the distance of Ambleteuse from Boulogne corresponds closely enough with Caesar's estimate of the distance of the *ulterior portus* from the Portus Itius. This does not prove the identity of the Portus Itius with Boulogne : but, if it is not a fact, the Portus Itius was not Boulogne ; and it is therefore necessary to examine the arguments of those who have denied it. 'On measuring,' says Airy,[5] 'upon the beautiful Admiralty Chart the distance between the centre of the entrance to Boulogne and the centre of the entrance to Ambleteuse, I find it to be not quite 4½ nautical miles, or 5½ Roman miles ; instead of the 8 miles given by Caesar.' This estimate is accurate ; but it is also irrelevant ; for Airy measures the distance by sea ; and Caesar must have meant the distance by land. 'It was quite immaterial,' says Lewin,[6] 'what was the distance by sea, for the eighteen transports were windbound, and could not reach him ; but, as he could not dispense with the vessels, he had to consider what portion of his force could be most conveniently despatched thither, and as the transports lay eight miles off, he thought it best, in order to save time, to send thither his cavalry . . . by the nearest road from the port of Boulogne, through Wimille and Slacq to the church at Ambleteuse, the distance is

[1] For the second expedition the vessels were specially constructed of light draught (*B. G.*, v, 1, § 2) ; and those which Napoleon built for the flotilla of 1804 did not draw more than 3 feet of water (Nap. III, *Hist. de Jules César*, ii, 172). Even in Caesar's first expedition the draught of the transports could not have been great, as the men were able to jump off them into the sea and wade ashore.

[2] A. E. E. Desjardins. *Géogr. de la Gaule rom.*, i, 378, 380.

[3] Capécure is on the left bank, about two miles from the mouth of the river.

[4] *Bull. de la Soc. acad. . . . de Boulogne-sur-mer*, i, 1873, p. 278, n. 1 ; D. Haigneré, *Recueil hist. du Boulonnais*, i, 328–32, ii, 422. The latter passage is worth quoting :—'On travaillait en 1861 au creusement du sas-éclusé dont la munificence du gouvernement . . . a doté le port de Boulogne . . . les ouvriers arrivèrent dans la couche la plus basse des sables qu'ils déblayaient dans la fouille ! Nous y trouvions le radier de l'ancien port semé d'antiquités gauloises et de débris romains, portant, sur sa surface, de tuf glaiseux, la trace visible du roulis des vagues, avec une dépression marquée, formant une sorte de chenal qui se dirigeait vers l'ouest,' &c. Airy, who insists (*Archaeologia*, xxxiv, 1852, p. 236) that Boulogne harbour would have been too small for Caesar's purpose, neglected to inform himself that there was much more space in the estuary in Caesar's time than there is now.

Henry's objection (*Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, pp. 58–9), that there would not have been enough water in the harbour at sunset, when Caesar set sail on his second voyage, therefore collapses ; but even if his statement were true, the inference which he draws from it would be refuted by himself : for he tells us (p. 52) that in 55 B.C. the ships 'ont dû partir du mouillage '. If so, why not in 54 B.C. also ?

[5] *Essays on the Invasion of Britain*, &c., p. 28.

[6] *The Invasion of Britain*, &c., 1862, pp. xiii–xvi.

twelve kilometres.'[1] It is amusing to find that Airy, who lays so much stress upon the accuracy of Caesar's (assumed) estimate of the distance by sea from the Portus Itius to the *ulterior portus*, maintains on the preceding page (*Essays on the Invasion of Britain*, &c., p. 27) that Caesar's estimates of distances by sea were valueless.

Fourthly, Caesar's narrative of the adventures of his cavalry transports is easily comprehensible on the hypothesis that the port from which he himself sailed was Boulogne, but on no other.[2] As we have seen, the storm of the 30th of August, 55 B.C., which prevented them from making the Kentish coast near Caesar's camp, drove some of them westward to a point on the south coast, and carried the rest back to the point whence they had started, namely, the *ulterior portus*. That port, if Caesar sailed from Boulogne, was Ambleteuse; and there is no difficulty in believing, nor has it ever been denied, that the wind, before which some of the ships ran from the neighbourhood of the South Foreland[3] in the direction of Dungeness, would have carried the others, which were laid to, back to Ambleteuse.

Fifthly, Caesar, as we have seen, sailed from the Portus Itius with a south-westerly wind;[4] and it is needless to tell any one who will consult the map that to sail with a south-westerly wind, especially with flat-bottomed vessels which made a great deal of lee-way, and on the easterly going stream, from Boulogne either to Sandwich, Deal, Walmer, Hythe, or Lympne, would have been easier than to sail from Wissant.

Sixthly, it is universally admitted that Boulogne, which Pliny[5]

[1] The distance by the new military road is, as Lewin warns his readers, much less.

Mariette (*Lettre à M. Bouillet*, &c., pp. 16, 51) actually holds that the *ulterior portus* was the harbour of Bononia (see p. 591, n. 1, *infra*), as distinguished from that of Gesoriacum! The *ulterior portus*, he says, is generally assumed to have been eight Roman miles from the Portus Itius, simply because the eighteen ships which carried Caesar's cavalry were detained eight miles from the Portus Itius by contrary winds. But, he insists, Caesar does not say that the place where the eighteen ships were detained was a harbour: he merely indicates the harbour where the cavalry embarked, without saying where it was; it was not the same place as that at which the vessels had been detained some days before.

I only notice this theory because Mariette was a really eminent man. If it were necessary to refute it, it would be sufficient to say, first, that, as Caesar tells us (*B. G.*, iv, 22, § 4) that the eighteen ships (which he reserved for his cavalry) were detained by contrary winds at a place eight miles from the harbour which sheltered the rest of the fleet, and in the next sentence but two says that he ordered the cavalry to advance to the *ulterior portus* (which he had not mentioned before), and embark, the inevitable conclusion is that the place where the eighteen ships had been detained was the *ulterior portus*; secondly, that if the *ulterior portus* had been virtually in juxta-position with the port from which Caesar sailed, he would certainly have taken care that they sailed along with him.

[2] See pp. 558, 581–2, *supra*.

[3] See pp. 616, 651, *infra*.

[4] *B. G.*, v, 8, § 2.

[5] *Nat. Hist.*, iv, 23 (37), § 122. Wauters (*Bull. de l'Acad. Roy . . . de Belgique*, 2e sér., xlvii, 1879, pp. 125–6) actually argues that because Lambert of Ardres, who wrote in the thirteenth century, called Wissant the *portus Britannicus*,

calls the *portus Morinorum Britannicus*, was the permanent naval station of the Romans in the imperial epoch; and that it was the harbour from which they habitually sailed for the coast of Kent.[1] An inscription preserved in the Boulogne museum[2] proves that this station was established at least as early as the reign of Claudius, while Suetonius[3] tells us that Claudius embarked at Gesoriacum for Britain. Indeed there is indirect evidence that the station existed in the time of Augustus; for the road which ran from Mediolanum (Milan) past Lugdunum (Lyons), Durocortorum (Reims), and Ambiani (Amiens), to Gesoriacum[4] was constructed by Agrippa. It has been argued that, although Gesoriacum was the recognized harbour from the time of Augustus, the fact does not prove that it was the harbour from which Caesar sailed. But to those who admit that it has been proved that no other port existed which would have served Caesar's purpose the fact will appear conclusive.[5]

Seventhly, Desjardins[6] has pointed out that Gallic ports were always either in the mouths of rivers or otherwise sheltered from storms. Such a port was Gesoriacum; and if Wissant was a Gallic harbour at all, it was a solitary exception to the rule.

Lastly, Rudolf Schneider[7] lays great stress upon the fact that, according to Pomponius Mela,[8] no harbour on the northern coast of Gaul was better known than Gesoriacum; and he reminds us that Pliny[9] mentioned no other harbour in the country of the Morini. Unless, he argues, the Portus Itius was identical with Gesoriacum, Mela, Pliny, and the later writers must have forgotten its existence. Now nothing would be easier than to make a dialectical reply to this argument,—Is it not equally remarkable that none of these

therefore Wissant was the *portus Morinorum Britannicus* of Pliny! He forgets that Lambert was not referring to the time of the Roman Empire: he simply meant that in his own time Wissant was a frequented port of departure for England.

Courtois insists (*Bull. de la Soc. des ant. de la Morinie*, iii, 1862, p. 391) that Pliny distinguishes the *portus Morinorum Britannicus* from Gesoriacum. As well might a modern leader-writer be said to distinguish London from 'the metropolis'.

[1] See A. E. E. Desjardins, *Géogr. de la Gaule rom.*, i, 363–8, 371–2, 383–8. Roman tiles, bearing the inscription CL. BR., have been found at Bréquerecque, east of Boulogne, on the banks of the Liane; and inscriptions found at Tintelleries and Bréquerecque prove that CL. BR. stands for *classis Britannica* (*ib.*, p. 364, and V. J. Vaillant, *Classis Britannica*, 1888, pp. 16–7).

[2] A. E. E. Desjardins, *Géogr. de la Gaule rom.*, i, 367–8.

[3] *Divus Claudius*, 17.

[4] *Itin. Ant.*, ed. Wesseling, pp. 356–63; A. E. E. Desjardins, *Géogr. de la Gaule rom.*, i, 387.

[5] Zosimus (*Hist. Nov.*, ed. L. Mendelssohn, 1887, vi, 2, § 2) says that Bononia was the first port to be met with in Germania (Inferior), that is to say, by a traveller coming from the east; and much stress has been laid upon this passage by the advocates of Boulogne: but it only proves what we knew already, namely, that if Wissant had ever been a Gallic port, it fell into complete disuse under the empire.

[6] *Géogr. de la Gaule rom.*, i, 383.

[7] *Portus Itius*, p. 12.

[8] *Chorographia*, iii, 2, § 23.

[9] *Nat. Hist.*, iv, 16 (30), § 102; 23 (37), § 122.

writers even hints that Gesoriacum was the Portus Itius? This was the reply which I made myself on another occasion. But the reply was sophistical. Schneider's argument depends upon the assumption that the Portus Itius was one of the great harbours of Gaul; and considering that it could accommodate 800 vessels, this assumption is certainly reasonable. At all events it is impossible to suggest any other explanation of the fact that after Strabo no writer mentioned the Portus Itius for more than a thousand years, except this,—that the Portus Itius and Gesoriacum were one.

It would be waste of time to repeat the arguments, which have already been stated by implication in the section on Wissant, based upon the unique advantages that Boulogne possessed in being connected with the interior by river and road.[1]

It remains only to consider the objections which have been made to the identification of the Portus Itius with Boulogne.

1. The very fact that Boulogne was called Gesoriacum is regarded by Long[2] as presumptive evidence that it was not called Portus Itius.

Desjardins,[3] who evidently regards this as a serious objection, has taken great pains to remove it. He argues that the Portus Itius was not exactly the same as the imperial harbour of Gesoriacum, but that it comprised that part of the estuary of the Liane which lies between Bréquerecque and Isques; and, he triumphantly remarks, the name 'Isques' is derived from *Itius*. But 'Isques' cannot have been derived from *Itius*: the names 'Ausques','Quesques', 'Clerques', 'Setques', and 'Wisques' are derived from *Alciacum*, *Kessiacum*, *Quertliacum*, *Sethiacum*, and *Wiciacum*; and the inference is that not *Itius* but *Isiacum* would have been transformed into 'Isques'.[4] Rudolf Schneider,[5] who is too honest and too hard-headed to be deluded by Desjardins's attempt to draw a distinction between Gesoriacum and Portus Itius, frankly admits that the unrecorded change of name has not been explained. But is there anything to explain? 'Portus Itius' is not, properly speaking, a name at all: it does not designate a town; it means simply 'the Itian harbour'. Long saw nothing inexplicable in the fact that Gesoriacum was called by Pliny *portus* [Morinorum] *Britannicus*:

[1] Napoleon III (*Hist. de Jules César*, ii, 171–2) maintains that the fact of the great Napoleon's having selected Boulogne for the embarkation of the troops with which he intended to invade England is a strong argument in favour of Caesar's having done the same. I lay no stress upon this argument because it is superfluous if it can be shown that Wissant would not have served Caesar's purpose equally well; and that this has been shown those who have read so far will not deny. It is hardly necessary to add that Boulogne was only one, though the principal, of several ports selected by Napoleon.

[2] *Decline of the Roman Republic*, iv, 432. 'If,' says Long, 'it was named Gesoriacum in Caesar's time, why did he name it Itium?' The obvious answer is that he did not name it 'Itium'. He named it, or rather its harbour, *portus Itius*,—'the Itian harbour,' or, as Professor Rhys expresses it, 'the Channel harbour.'

[3] *Géogr. de la Gaule rom.*, i, 383–4, 473.

[4] *Dict. arch. de la Gaule*, ii, 45–7.

[5] *Portus Itius*, p. 19.

why, then, should he have found it impossible to believe that its harbour was called by Caesar *portus Itius* ?[1] Was not the port of Athens called the Piraeus ?

2. Long, after making the amazing remark that 'such a port as Boulogne would have been quite useless in Caesar's second expedition', says that 'the Romans estimated the distance from Boulogne to the British coast at fifty Roman miles; but this is too much . . . However, they were right in making the distance more than the distance from Itius to the nearest point of the British coast; and the conclusion is that Gesoriacum and Itius were different places.'[2]

The conclusion is simply that, assuming the identity of Gesoriacum with the Portus Itius, Caesar's estimate of the distance from Gesoriacum to Britain was different from that of later writers. Besides, the only one of 'the Romans', as far as we know, who 'estimated the distance from Boulogne to the British coast at fifty Roman miles' was Pliny;[3] and it may be presumed that by 'the British coast' he meant Rutupiae, or Richborough, which was a port, if not the chief port, of arrival in his day. For he estimated the shortest passage at fifty Roman miles: according to Dion Cassius,[4] the shortest passage was 450 stades; and this, according to the *Itinerary* of Antonine,[5] was precisely the length of the passage from Boulogne to Richborough.

Long goes on to say that in the *Itinerary* of Antonine the distance from Boulogne to Richborough 'was estimated at 450 stadia; and, if we follow d'Anville in estimating this maritime stadium at ten to the Roman mile, the distance is fairly given. So if we take the 320 stadia which Strabo gives as the length of Caesar's voyages, we have thirty-two Roman miles; or, if we take the reading which Eustathius, copying Strabo, has in his commentary on Dionysius, 300 stadia, we have exactly thirty Roman miles, as in Caesar's text. The conclusion is that, in addition to the fact of Boulogne (Gesoriacum) and Ouissant (Itius) having different

[1] Desjardins also finds it necessary to explain why the name *Gesoriacum* was succeeded by *Bononia*. His explanation (*Géogr. de la Gaule rom.*, i, 373) is that the port of Gesoriacum was different from the port of Bononia. Remarking that, according to Eumenius (*Paneg. Constantii*, c. vi), the port of Gesoriacum was blocked by the emperor Constantius Chlorus, in order to prevent the escape of Carausius, he says that 'sans doute' this port was then abandoned for the new (and hypothetical) port of Bononia, 'aux Tintelleries,' further down the Liane. This, he says, explains why the name Bononia was alone used (except in the itineraries) after the time of Constantine. I have noticed that Desjardins uses the words 'sans doute' when there is a doubt which he is unable to remove. As he insists that the ports of Bononia and Gesoriacum were different, he must, I think, have been off his guard when he quoted, in support of his contention, an anonymous writer, who mentions the arrival of Constantine at 'Bononia, which the Gauls originally called Gesoriacum' (*Bononiam, quam Galli prius Gesoriacum vocabant* [M. Bouquet, *Recueil des hist. des Gaules*, i, 1738, p. 563B]). And in his own edition of the Peutinger Table (p. 13, col. 2) I find the words *Gesogiaco quod nunc Bononia*.

[2] *The Reader*, Sept. 5, 1863, p. 254.

[3] *Nat. Hist.*, iv, 16 (30), § 102.

[4] *Hist. Rom.*, xxxix, 50, § 2.

[5] Ed. Wesseling, p. 463.

names, the ancient authorities place them at different distances from the British coast.' [1]

Again Long's conclusion is at fault. To begin with, Strabo did not estimate the maritime stadium at ten, but at eight to the Roman mile.[2] Assuming, however, that he did estimate it at one-tenth of a Roman mile, there is no reason to suppose that when Caesar estimated the distance from the Portus Itius to Britain, he meant the distance to Richborough; and the only conclusion that can be drawn from Long's data is that 'the ancient authorities' reckoned the length of Caesar's voyage less than the distance between Richborough and Boulogne. And when Long says that the estimated distance, 450 stadia, from Boulogne to Richborough 'is fairly given', it is amusing to find him admitting that Caesar's estimate of 30 miles 'exceeds the distance from Wissant to the nearest part of the English coast, and it is about the true distance from Boulogne to the same part of the English coast'. Thus, on Long's own showing, Caesar's estimate of the distance from the Portus Itius to the British coast corresponds with the actual distance from Boulogne to the same, and the estimate of the *Itinerary* is equally true. The reader who has followed him so far will hardly be surprised by his remark, that 'even a real good harbour would have been useless to Caesar'.[3]

3. Heller,[4] after quoting the statement of Pliny as to the length of the shortest passage from Boulogne to Britain, and the statement of the *Itinerary* of Antonine as to the distance from Boulogne to Richborough, argues (*a*) that, as they overestimated the distance from Gaul to Britain, Caesar probably did the same; (*b*) that if Pliny had identified Boulogne with the Portus Itius, he would not have estimated the distance of Boulogne from the nearest point of Britain at 50 miles, but would have followed Caesar and written 'about 30'; (*c*) that if Caesar had started from Boulogne, he would, according to the usual tendency of the ancients, have overestimated the distance from Boulogne to Britain, and would therefore have reckoned it at considerably more than 'about 30 miles', seeing that the actual distance from Boulogne to Dover is 33.

The first of these arguments, if it had come from a tiro, might have been passed over with a smile; but one would hardly have expected it from Heller. The third is based upon a misleading

[1] *The Reader*, Sept. 5, 1863, p. 254.

[2] The arguments of d'Anville, intended to prove that ten maritime stades were equivalent to one Roman mile, may be found in his *Traité des mesures anciennes*, 1769, pp. 71–6. Everybody knows that there were stades of various lengths, one of which was one-tenth of a Roman mile (*Itin. Hierosol.*, ed. Wesseling, p. 609); but the stade by which Strabo usually reckoned was one-eighth of a mile (*Geogr.*, vii, 7, § 4.—λογιζομένῳ, ὡς μὲν οἱ πολλοί, τὸ μίλιον ὀκταστάδιον. Cf. Pliny, *Nat. Hist.*, ii, 23 [21], § 85.—Stadium centum viginti quinque nostros efficit passus). See J. Wex, *Métrologie grecque et rom.* (trans. P. Mouat), 1886, p. 16; F. Hultsch, *Griech. und röm. Metrologie*, 1882, pp. 49, 59–60; and Ideler in *Abhandlungen der Königlichen Akad. der Wissenschaften zu Berlin*, 1826 (1829), p. 15; 1827 (1830), p. 127.

[3] *The Reader*, Sept. 5, 1863, p. 254.

[4] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, pp. 174–5.

statement ; [1] and even if we could be sure that Caesar overestimated the length of his voyage, it would be inconclusive, for, as we have seen,[2] it is not improbable that he estimated it at 40 Roman miles. And as for Pliny, 'the shortest passage,' which he estimated at 50 miles, was probably, I repeat, the passage from Boulogne to Richborough.

4. H. L. Long,[3] if I do not misunderstand him, argues that there could have been no port at Ambleteuse in the days of Caesar. Speaking of 'the immense irruption of blown sand', he maintains that 'this dune . . . acts as a dam to the drainage of the valley ; an interruption which must have produced swamps in former days, and is now but imperfectly corrected by an artificial channel, the embouchure of which forms the little harbour of Ambleteuse'.

This argument obviously depends upon the untenable assumption that the sand-dune existed in Caesar's time ; and it is shaken by the fact that Roman antiquities have been discovered at Ambleteuse.[4] Moreover, according to the writer of the article *Ambleteuse* in M. Vivien de Saint-Martin's *Nouveau Dictionnaire de Géographie Universelle* (i, 1879, p. 115), Ambleteuse, under the rule of the English, had an excellent harbour,[5] and was not choked up by the accumulation of sand until after 1549.[6]

5. General Creuly,[7] referring to the attack made by 6,000 of the Morini upon the Roman soldiers who disembarked from the two ships which failed to make the harbours in 55 B.C. and were carried 'a little further down', insists that Caesar's account of this episode [8] is incompatible with the view that the Portus Itius was Boulogne. For, he argues, the 6,000 Morini could not have belonged to the *pagus Gesoriacus*, that is to say, the district of Boulogne, since the inhabitants of this region had submitted to Caesar, and, moreover, it was so sparsely populated that 6,000 men could not have assembled on the spur of the moment. He also reminds us that on the day following the attack Labienus marched against the rebellious Morini, and soon subdued them, as, owing to a drought, they were unable to take refuge in the marshes which had served them as an asylum

[1] Cf. R. Schneider, *Portus Itius*, p. 10. No doubt the ancients did commonly overestimate distances ; but any one who had time to go through Strabo could pick out exceptions. Thus he tells us (i, 4, § 4) that the distance from Massilia to 'the middle of Britain' (*εἰς μέσην τὴν Βρεττανικήν*) is 5,000 stades, and (ii, 1, § 40) that the distance from Carthage to Massilia is not more than 9,000. The latter, *in a straight line*, is about 10,500 : the former, measured only as far as Portsmouth Harbour, about 5,200.

[2] See p. 558, *supra*.

[3] *Gentleman's Magazine*, xxvi, 1846, p. 252.

[4] D. Haigneré, *Étude sur le Portus Itius*, p. 108.

[5] See also J. F. Henry, *Essai . . . sur l'arrondissement communal de Boulogne-sur-mer*, p. 190, and *Boulogne-sur-mer et la région boulonnaise*, i, 369.

[6] Haigneré (*Recueil hist. du Boulonnais*, i, 377) questions whether there is any mention of Ambleteuse as a port earlier than the sixteenth century ; but it is certain that a charter was granted to the town in the year 1209. See *Bull. de la Soc. acad. de Boulogne-sur-mer*, i, 1864–72 (1873) pp. 139–46.

[7] *Rev. arch.*, nouv. sér., viii, 1863, p. 309.

[8] *B. G.*, iv, 37.

in the preceding year ;[1] and he denies that there were any marshes in the *pagus Gesoriacus* large enough to serve such a purpose.

Creuly can only make a show of sustaining these objections by resorting to Airy's fantastic theory,[2]—that Caesar, when he said that two of his ships were carried 'a little further down', meant not 'down the coast' but 'in the direction of the wind'. If the inhabitants of the district of Boulogne had submitted, why should they not have rebelled ? The Aduatuci submitted and afterwards rebelled :[3] the Nervii submitted and afterwards rebelled ;[4] the Britons submitted and afterwards rebelled :[5]—but it is needless to multiply examples. For Caesar expressly states that the Morini who attacked his soldiers *had* submitted to him before he sailed for Britain.[6] I am not concerned to defend the accuracy of his statement, that their number was 6,000 : but Creuly admits that 6,000 Morini did assemble somewhere in their own country ; and how can he prove that the district of Boulogne was more sparsely populated than any other ? As to the marshes, there is no evidence that they were in the immediate neighbourhood of the spot where the soldiers were attacked ; but if they were, what is there to prevent us from identifying them with the marshes south of Boulogne, between Camiers and Dannes ?[7]

6. Heller's objection,[8] that if Caesar had sailed from Boulogne in 54 B.C. it would have been impossible for his ship to drift so far by daybreak on the following morning as to justify him in saying that he 'saw Britain lying behind on the port quarter' (*sub sinistra Britanniam relictam conspexit*[9]), has been already answered.

7. Caesar, describing his return to Gaul in 54 B.C., says that 'at daybreak he reached land' (*prima luce terram attigit*), and that his 'ships were hauled up on the shore' (*subductis navibus*[10]). It has been argued that 'both of these expressions point to the conclusion that he did not enter the mouth of a river', and that 'if the Portus Itius was in the estuary of the Liane, to haul up the ships over the banks on to the meadows would surely have been a difficult operation'.[11] The author of this argument forgot that the ships need not have been hauled up on to the meadows at all unless they had gone far up the river, and that they may have been docked. But an expert whom he has since consulted assures him that, even if it had been necessary to haul up the ships over the banks on to the meadows, the operation would have involved no serious difficulty.

[1] *B. G.*, iv, 38, §§ 1-2.
[2] See p. 561, *supra*.
[3] *B. G.*, ii, 31-3 ; v, 39, § 3.
[4] *Ib.*, ii, 28 ; v, 38-9.
[5] *Ib.*, iv, 21, § 5 ; 27 ; 30.
[6] *Ib.*, iv, 37, § 1.—'About 300 soldiers had landed from these two vessels and were making the best of their way to camp, when the Morini, who had been quite submissive when Caesar left them on his departure for Britain, surrounded them,' &c. (*Quibus ex navibus cum essent expositi milites circiter CCC atque in castra contenderent, Morini, quos Caesar in Britanniam proficiscens pacatos reliquerat, spe praedae adducti . . . circumsteterunt* &c.)
[7] See T. Lewin, *The Invasion of Britain*, &c., 1862, p. xlii.
[8] See pp. 574-7, *supra*.
[9] *B. G.*, v, 8, § 2.
[10] *B. G.*, v, 23, § 6 ; 24, § 1.
[11] Rice Holmes, *Caesar's Conquest of Gaul*, 1899, pp. 438-9.

If this inquiry had merely established the probability of the identification of the Portus Itius with the harbour of Boulogne, it would not be possible to justify the labour which has been expended upon it except on the ground that it will save those who may wish to inform themselves a vast amount of research, and provide them with complete equipment for arriving at an independent conclusion. But that conclusion, if it is reached conscientiously by an unbiassed mind, can only be one.

THE PLACE OF CAESAR'S LANDING IN BRITAIN

I. INTRODUCTION

After I had completed the researches which I undertook for the purpose of writing this article, I saw that if an able soldier, or even an intelligent civilian, who had a sufficient knowledge of ancient warfare, were to ask himself where Caesar landed in Britain, he could solve the problem after a brief inspection of the Ordnance Map. He would perceive that there was only one part of the Kentish coast on which Caesar could have expected to land, in the face of an enemy, and then to march into the interior, without incurring unnecessary loss. If he were told that a study of the tides had proved that Caesar must have landed elsewhere, he would reply, 'There must be something wrong in your calculations. Perhaps you have neglected to allow for the influence which strong winds and other causes exert upon the tidal currents. Perhaps you have misinterpreted or unduly strained certain parts of Caesar's narrative. It is even possible that Caesar himself may, from lapse of memory, have mis-stated the day on which he first landed. Any one of these suppositions is credible: but it is incredible that the experienced officer whom he sent to reconnoitre the British coast should have advised him to land below a range of hills when open country was more easily accessible; still more that he should have accepted the advice. It is absolutely certain that Caesar did not commit an act of folly which any general who knew his business would have avoided.'

But such a summary mode of treating the question would not convince the scholars who must be convinced before it can be set at rest; and the conclusion at which they have arrived is that it is insoluble. So said Mommsen:[1] so say Mr. Warde Fowler,[2] Mr. Tozer,[3] and, apparently, Dr. Hodgkin;[4] so said the late eminent geographer, H. Kiepert, according to whom the numerous attempts which have been made to determine Caesar's landing-place 'have, because of the vagueness of many expressions of the principal

[1] *Röm. Gesch.*, iii, 1889, pp. 269–70, note (Engl. trans., v, 1894, p. 63, note).
[2] *Julius Caesar*, 1892, p. 196.
[3] *Hist. of Anc. Geogr.*, pp. 230–1.
[4] *Pol. Hist. of England*, i, 1906, pp. 23–4.

source [the *Commentaries* of Caesar], not attained more than a hypothetical value, even after three centuries of learned quarrels'.[1] Not because of the vagueness of Caesar's expressions, but because those who have commented upon them have not taken the trouble to inform themselves. The indications which Caesar gives are sufficient to enable any attentive reader to determine the place where he landed with such certainty that every doubt shall be removed,—if he knows how to use them; if, that is to say, he possesses sufficient collateral knowledge to enable him to understand what he reads. It is not enough to be a Latin scholar. It is necessary also to study the ancient geography of the coast of Kent; to be acquainted with the tidal phenomena of the English Channel; to have at least an elementary knowledge of seamanship; to know Caesar's writings intimately, and not merely read the Fourth and Fifth Books for the occasion; and, above all, to gain that understanding of the principles of ancient warfare which can only be acquired by one who has studied the history of modern campaigns, and has learned, by experience or from intercourse with practical men, how things actually happen. No genius is needed; only industry, backed by common sense and by some intelligence and acumen: but such industry as may, perhaps, be thought disproportionate with the object. Not even Mommsen, with his colossal power of work, could spare the necessary time.

II. THE DATA FURNISHED BY CAESAR AND OTHER ANCIENT WRITERS

The data which we find in the *Commentaries* are the following. Before starting on his first voyage, Caesar sent a military tribune, named Volusenus, whom he believed to possess the necessary qualifications, in a ship of war, to make a thorough reconnaissance of the British coast, and to ascertain what harbours were capable of accommodating a large flotilla.[2] Volusenus returned to Gaul four days after his departure, having made all the observations that it was possible for him to make without landing; for he had not ventured to put himself in the power of the natives. Caesar himself marched his army into the country of the Morini, as the shortest passage to Britain was from their coast. The fleet which he ordered to assemble consisted of about 100 country-built merchant vessels, collected from the neighbouring districts, as well as some ships of

[1] *Formae orbis antiqui*, 1894, xxvi.

[2] *B. G.*, iv, 20, § 4. I agree with Meusel in adopting the reading (ad) *maiorem* (navium multitudinem idonei portus), not *maiorum*. A moment's reflection will show that we ought to read *maiorem*, even though there is no better *MS.* authority for it than the *codex Vratislaviensis I.* Caesar was not anxious to find out what harbours would accommodate a flotilla of large ships, but what harbours would accommodate a large flotilla. The draught of his ships was so small that when they were aground the men could jump overboard and wade ashore. See *Classical Review*, xv, 1901, p. 176.

war and small fast-sailing vessels, called *speculatoria navigia*, or 'scouts'. His intention had been made known in Britain by traders; and while his ships were assembling, envoys sent by several British tribes presented themselves before him, and promised to give him hostages, and to submit to the Roman People. On their return they were accompanied by Commius, a Gallic chieftain, who acted as Caesar's political agent and who took with him about thirty of his own horsemen. Of the merchant vessels eighteen had assembled in a harbour 8 Roman miles 'beyond' that which sheltered the rest of the fleet,[1] and were prevented by contrary winds from joining them. Caesar set sail 'about the third watch' (*tertia fere vigilia*) in favourable weather, having ordered the cavalry to march to the 'further' port, embark there on the eighteen transports, and follow him. Their movements, however, were somewhat dilatory. 'About the fourth hour' (*hora circiter diei quarta*) the leading division of the fleet had approached so close to the British coast that Caesar could see an armed force of the enemy drawn up 'on all the heights' (*in omnibus collibus*). 'The formation of the ground,' he says, 'was peculiar, the sea being so closely walled in by precipitous heights that it was possible to throw a missile from the ground above on to the shore.'[2] Regarding the place as unsuitable for landing, he waited at anchor 'till the ninth hour' (*ad horam nonam*) for the arrival of the rest of the fleet. Meanwhile he assembled his generals and military tribunes, communicated to them the report which he had received from Volusenus, and gave them all necessary instructions. They returned to their respective vessels; and, 'getting wind and tide simultaneously in his favour,' Caesar weighed anchor, sailed on (*progressus*) about 7 Roman miles, and ran the ships aground 'on an open and evenly shelving shore' (*aperto ac plano litore*).[3]

The natives, divining his intention, had sent on ahead their cavalry and charioteers, who were followed by the rest of the forces. The important points in Caesar's description of the disembarkation are as follows:—some of the enemy, in opposing it, threw missiles from the shore; others advanced a little way into the water, riding or driving their horses. The transports, on account of their relatively considerable draught, had necessarily grounded in deep water; and on this account the Roman soldiers hesitated before jumping into the sea to wade ashore. During the conflict Caesar made some of his war-galleys sheer off a little from the transports,

[1] See p. 554, *supra*.

[2] Mr. H. E. Malden (*Journal of Philology*, xxii, 1894, p. 168) remarks that the words *cuius loci haec erat natura atque ita montibus angustis mare continebatur uti ex locis superioribus in litus telum adigi posset* have been 'taken as applying to old Dover harbour'. It is true that they have been taken in this sense by commentators who were ignorant of the meaning of *angustis*; but even if they could be interpreted as meaning a creek or inlet hemmed in by precipitous heights, they could not apply to 'old Dover harbour', which occupied part of the Priory Valley, and was never hemmed in by 'precipitous heights'. Hoffmann unnecessarily conjectures that Caesar wrote not *angustis* but *anguste*.

[3] See pp. 653–4, *infra*.

and take up a position on the enemy's exposed flank; and later on, when legionaries who had just dropped into the sea and gathered in small groups were being hard pressed, he manned the small fast-sailing craft and the small boats belonging to the galleys, and sent them to the rescue. The enemy derived an advantage from their knowledge of the places where the water was shallow. Caesar concludes his description of the landing by saying that it was impossible to pursue the enemy far, because 'the cavalry had not been able to keep their course and make the island'.

On the fourth day after the landing (the day of the landing being doubtless reckoned as the first day)[1] the eighteen transports that carried the cavalry again set sail from the 'further' harbour with a light wind. They were approaching the British coast and were visible from Caesar's camp, when a sudden storm came on with the result that some of them were carried back to the harbour whence they had started, while the others 'were driven down in great peril to the lower and more westerly part of the island' (*ad inferiorem partem insulae, quae est propius solis occasum, magno suo cum periculo deicerentur*). They anchored: but the waves broke over them; and they were obliged to stand out to sea in the face of night[2] and make for the Continent.

On the same night there was a full moon; and Caesar remarks that full moon causes extraordinarily high tides in the ocean. Owing to the high tide and the gale, the ships of war, which had been drawn up on the shore, were waterlogged, and many of the transports, which were riding at anchor, were driven ashore and wrecked. A few days later one of the legions was sent out in the ordinary course to cut corn. Presently Caesar was informed by the troops on guard in front of the camp that an unusual quantity of dust was visible in the direction in which the legion had gone. When he had advanced 'some little distance' (*paulo longius*) from the camp, he saw that his troops were in difficulties; and he tells us that the

[1] See p. 602, n. 5, *infra*.

[2] *quae tamen ancoris iactis cum fluctibus complerentur, necessario adversa nocte in altum provectae continentem petierunt* (*B. G.*, iv, 28, § 3). The meaning of *adversa nocte* has been much discussed. According to C. Schneider (*Comm. de bellis C. I. Caesaris*, i, 397), who refers to a passage in the *Civil War*, ii, 31, § 7—*namque huius modi res aut pudore aut metu tenentur, quibus rebus nox maxime adversaria est*—the word *adversa* is equivalent to *obstante*, that is to say, 'being unfavourable to them': but, assuming that this is the meaning, did Caesar intend to convey that the ships stood out to sea *though* night was unfavourable to the voyage, or *because* night was unfavourable to their remaining where they were? I unhesitatingly reject the former alternative, for all Caesar's voyages between Gaul and Britain were made by night, and, moreover, on this particular night there was a full moon: on the other hand, it would not have been more dangerous to remain at anchor in the night than in the daytime. I agree with Kraner-Dittenberger (*C. I. Caesaris comm. de b. G.*, 1890, p. 85), who hold that, just as *adverso colle* (*B. G.*, ii, 19, § 3) means 'up the hill', and *adverso flumine* (*ib.*, vii, 60, § 3) 'up the river', so *adversa nocte* means 'in the face of night' ('der Nacht entgegen', 'in die Nacht hinein'), a translation which reminds one of Browning's famous line 'And into the midnight we galloped abreast'.

place to which they had gone was the only [accessible] spot in which the corn had not been cut, and that there were woods close by. In repelling an attack which was made upon his camp just before his departure, he made use of Commius's small troop of cavalry, and immediately afterwards he 'burned all the buildings far and wide' (*omnibus longe lateque aedificiis incensis*). On his return voyage he set sail soon after midnight.

The flotilla with which he sailed for Britain in the following year consisted of more than 800 vessels. Of these over 540 were transports and 28 war-galleys, while the rest belonged to individuals.[1] Some of the vessels used in the former expedition had been repaired: the rest were built and rigged by Caesar's troops during the winter of 55–54 B.C., and the following spring. The transports drew very little water, and were adapted for rowing as well as sailing; and, to provide room for troop-horses and stores, they were made proportionately broader than the trading vessels used by the Italians in the Mediterranean. Carrying five legions and 2,000 cavalry, they sailed from the Portus Itius 'about sunset' (*ad solis occasum*), with a light south-westerly breeze. About midnight the wind dropped: the fleet was carried far out of its course by the tidal stream; and at daybreak Caesar 'descried Britain lying behind on the left' (*Britanniam sub sinistra relictam conspexit*). He then followed the turn of the tide, and, as he tells us, 'rowed hard to gain the part of the island where, as he had learned in the preceding summer, it was best to land' (*remis contendit ut eam partem insulae caperet qua optimum esse egressum superiore aestate cognoverat*). He remarks that the soldiers who rowed the heavily-laden transports deserved great credit for their unremitting labour, which enabled them to keep up with the war-galleys. The whole fleet had reached the coast by about noon. No enemy was to be seen; and Caesar learned afterwards from prisoners that large forces had collected at the landing-place, but that, panic-stricken by the sight of 800 vessels, they had abandoned the shore and retreated to 'higher ground' (*superiora loca*).

After the disembarkation Caesar selected a suitable spot for his camp. 'About the third watch' (*de tertia vigilia*) he marched to encounter the enemy, whose whereabouts he had ascertained from prisoners. He left the ships riding at anchor in charge of ten cohorts and 300 cavalry; and he describes the anchorage as on 'a nice open shore'[2] (*litore molli atque aperto*). The force which accompanied him consisted of four legions and 1,700 horse. After a march of about 12 Roman miles he descried the enemy. They advanced with their cavalry and chariots 'from the higher ground' (*ex loco superiore*)[3] to the banks of a stream, and attempted to prevent the Romans from crossing. Repulsed by Caesar's cavalry, they took refuge in a stronghold in the neighbouring woods, which is described by

[1] See p. 331, *supra*.
[2] The meaning of *mollis* is discussed on p. 630, *infra*.
[3] See p. 680, *infra*.

Caesar as 'a well-fortified post of great natural strength' (*locum egregie et natura et opere munitum*).

In a storm which occurred on the following night most of the ships were driven ashore, about 40 being totally wrecked; and in order to prevent a repetition of this disaster, the ships were all hauled up on dry land and 'connected with the camp by one entrenchment' (*cum castris una munitione coniungi*).

In his general description of Britain Caesar says that neither the beech nor the fir grow in the island.[1] He describes it as triangular, and says that one of its sides faces Gaul, and that 'one corner of this side, by Kent—the part which almost all ships from Gaul make for—has an easterly, and the lower one a southerly outlook'. Of the other two sides one, he says, 'trends westward towards Spain' (*alterum vergit ad Hispaniam atque occidentem solem*); while the other has a northerly aspect, and 'its corner looks if anything in the direction of Germany' (*eius angulus lateris maxime ad Germaniam spectat*).

The territories of Cassivellaunus were 'separated from those of the maritime tribes by a river called the Thames, about 80 [Roman] miles from the sea'.

On the second of the two voyages by which the troops were transported back to Gaul in 54 B.C., the ships started in a dead calm (*summa tranquillitate*) at the beginning of the second watch, and reached harbour at daybreak.[2]

The only piece of evidence worth quoting which is not in the *Commentaries* is the statement of Dion,[3] that Caesar, in sailing from his anchorage to his landing-place in 55 B.C., rounded a promontory. Some commentators, however, believe that important additional evidence is furnished by Plutarch and Valerius Maximus; and the statements in question will be considered in subsequent sections of this article.

III. THE DAY ON WHICH CAESAR LANDED IN 55 B.C.

It is absolutely certain, and is universally admitted, that the full moon which Caesar mentions occurred on the night of August 30–1, 55 B.C.:[4] to speak more precisely, it occurred at 3 h. 33 m. a.m. on the 31st.[5] Now, with one or two exceptions, which shall be presently considered, the commentators have concluded that Caesar landed on the fourth day before the full moon, that is to say, on the 27th of August. But any one who has read this article with close attention will have seen that their conclusion rests upon

[1] See pp. 661–2, *infra*.

[2] *B. G.*, iv, 20–6, 28–9, 31–6; v, 1, §§ 1–3; 2, §§ 2–3; 5, §§ 1–2; 8–11; 12, § 5; 13; 23.

[3] *Hist. Rom.*, xxxix, 51, § 2.

[4] See Napoléon III, *Hist. de Jules César*, ii, 174.

[5] The time of the full moon was kindly calculated for me by Messrs. John A. Sprigge, William Frazer Doak, M.A., F.R.A.S., and T. Charlton Hudson, B.A., F.R.A.S., all of the Nautical Almanac Office.

a careless interpretation of Caesar's narrative. Caesar says that the eighteen cavalry transports sailed from Gaul on the fourth day after he landed in Britain; that when they were approaching the island they were prevented from keeping their course by a storm; that some of them were driven westward down the coast and anchored, but were obliged to run back, in the face of night, for the Continent; and that on the same night there was a full moon.[1] Whereupon the commentators leap to the conclusion that the day on which Caesar landed was the fourth before the full moon. They forget that what Caesar said was, not that the transports approached the British coast on the fourth day after he landed, but that on that day they started from Gaul. The distinction is important. The transports may have weighed anchor in the night. On all his four voyages Caesar set sail at night; and Strabo says that for vessels sailing from Gaul this was the regular practice.[2] Let us assume that Caesar landed on the 26th of August. Then, if we adopt, as almost all the commentators have done, the inclusive mode of reckoning, the transports may have sailed from Gaul on the night of the 29th.[3] On the 30th, in the morning, they would have been approaching Britain. Then came on the storm. On the following night—the night of August 30–1—occurred the full moon.

It may be objected that if Caesar had landed in Britain on the 26th of August, and if his transports had set sail on the night of the 29th, but after midnight—say between 2 and 3 a.m.—he would have said that they set sail not on the fourth but on the fifth day after his landing. I will take note of this objection, but I doubt whether it is valid. In the thirty-third chapter of the Second Book of the *Commentaries*, after describing the sortie made by the Aduatuci, which took place in the third watch of the night, that is to say after midnight, and their repulse, he goes on to say that 'on the following day the gates were broken open' (*postridie eius diei refractis portis* &c.); in other words, he loosely reckoned the third watch of the night as part of the day that preceded the one which he calls *postridie eius diei*. It is therefore at least possible, I think it probable, that he landed on the fifth day before the moon which he described as full.

But it has been argued that Caesar may have made a mistake in describing the moon as full. He remarks, as we have seen, that the full moon produces very high tides in the Channel; and he states that on the night of this particular full moon many of his ships were waterlogged by an extraordinarily high tide. But the strongest spring tides, in the eastern part of the Channel, between Dungeness and Beachy Head, occur, not at the time of full moon,

[1] *B. G.*, iv, 28; 29, § 1.

[2] *Geogr.*, iv, 3, § 4.—διάρμα δ' ἐστὶν εἰς τὴν Βρεττανικὴν ἀπὸ τῶν ποταμῶν τῆς Κελτικῆς εἴκοσι καὶ τριακόσιοι στάδιοι· ὑπὸ γὰρ τὴν ἄμπωτιν ἀφ' ἑσπέρας ἀναχθέντες τῇ ὑστεραίᾳ περὶ ὀγδόην ὥραν καταίρουσιν εἰς τὴν νῆσον.

[3] See T. Bergk's article in *Jahrbücher für class. Phil.*, 13 Supplementband, 1884, p. 613.

but a day and a half later, and east of Dungeness two days later.[1] Airy,[2] premising that 'it is impossible to judge precisely of the day of full moon, either from the appearance of the moon's diameter . . . or from the time of moon-rising', argues that the moon which Caesar described as full was probably, as a matter of fact, that which produced the spring tide. Accordingly he assumes that Caesar landed on the third day before the full moon.

Airy's argument, however, is unsound. No well-informed man needs to be told that, long before Caesar's time, astronomers were able to predict the phases of the moon with sufficient accuracy for all ordinary purposes. Livy records that in 168 B.C. a tribune in the army of Aemilius Paullus told his men that a lunar eclipse would occur on the following night.[3] It is therefore at least not unlikely that Caesar should have known on what night the full moon which followed his landing occurred. The fact that the extraordinarily high tide occurred on the night which he calls the night of the full moon, presents no difficulty. The tide on the night of the full moon, even though, in normal circumstances, it would not have risen as high as a spring tide, properly so called, would in any case have been unusually high; and under the influence of the gale which Caesar mentions, its height would of course have been increased. Extraordinarily high tides, indeed, have occurred in such circumstances even at neaps;[4] and it would be just as reasonable to argue that Caesar ante-dated the day in question as that he post-dated it. For, as Airy himself assures us, the moon would have appeared full on the night before it really was so, that is to say, on the night of August 29–30; and if, as Airy assumes, full moon had occurred on the night before that to which Caesar assigned it, he would surely have noticed that the moon appeared full on the two successive nights which preceded what, on Airy's hypothesis, he called the night of the full moon.

The conclusion is that it is not possible to say with absolute certainty on what day Caesar landed. It is morally certain that he adopted the inclusive method of reckoning.[5] It is, as we have seen,

[1] S. H. Brown, *Diagrams and Tables of Tidal Streams*, &c., 1895, p. 51.

[2] *Archaeologia*, xxxiv, 1852, p. 240.

[3] xliv, 37, §§ 5–6.—C. Sulpicius Gallus . . . pronuntiavit nocte proxima, ne quis id pro portento acciperet, ab hora secunda usque ad quartam horam noctis lunam defecturam esse.

[4] At Sheerness on December 30, 1904, six days before new moon, 'the tide rose to an extraordinary height [owing to a severe gale], at least 5 ft. above the natural level' (*Times*, Dec. 31, 1904, p. 4, col. 2). 'Them tides,' said an old seaman to me at Dover, 'is the queerest tides in the world; I've seen myself more flow of water at nips than at springs. It all depends on the wind.'

[5] Napoleon III (*Hist. de Jules César*, ii, 175), after citing two irrelevant passages, asserts that 'le *post diem quartum* de César doit se comprendre dans le sens de quatre jours révolus, sans compter le jour du débarquement'; and then, remarking that the storm broke out on the 30th of August, he concludes that 'quatre jours pleins s'étaient écoulés depuis le débarquement; cela nous conduit au 26. César prit donc terre le 25 août.' To make things perfectly clear, let us put the matter in this way:—the orthodox view is that, according to the *common* Roman method of reckoning, the fourth day after Monday

probable that he landed on the fifth, but he may have landed on the fourth day before the moon which he described as full. It is highly probable that he fixed the day of the full moon correctly; but we cannot be perfectly sure. Accordingly it seems to me probable that he landed on the 26th of August: but he may have landed on the 27th, or possibly even on the 25th.

Nevertheless, I assure the reader that this uncertainty matters nothing. If he will bear with me to the end, he will see that we shall be able, notwithstanding, to determine the place of landing.

IV. DID CAESAR LAND AT THE SAME PLACE IN BOTH HIS EXPEDITIONS?

It remains to inquire whether, in both his expeditions, Caesar landed at the same place. The commentators are virtually unanimous in holding that he did; and Napoleon, whose view is an exception to the rule, believes that the landing-place of 54 B.C. was only a few kilometres north of that of the preceding year.[1] He and von Göler[2] both rely on the express statement of Dion Cassius[3]; but Dr. F. Vogel,[4] who attaches no importance to Dion's testimony on matters of this kind, reminds us that Caesar 'speaks only of the place which he had ascertained in the preceding year to be the best for landing',[5] and does not say that he had actually landed there. I agree with Vogel that Dion's statement proves nothing; for there is no reason to suppose that it represents anything but his own interpretation of Caesar's words. Nevertheless, it is certain

would be Thursday; Napoleon's view is that it would be Saturday! It is neither profitable nor exciting to slay the slain. I will therefore only remark that Napoleon's interpretation of the words *post diem quartum* is peculiar to himself, and that it has been demolished by Merivale (*Contemporary Review*, iii, 1866, pp. 125–6) and, still more effectively, by Heller (*Philologus*, xxvi, 1867, pp. 674–6).

Long (*Decline of the Roman Republic*, iv, 169), remarking that 'the Romans sometimes reckoned inclusively and sometimes not', concludes that 'the expression "on the fourth day" is ambiguous'. The famous jurist, F. C. von Savigny (*System des heutigen römischen, Rechts*, iv, 1841, pp. 602–16), collected a large number of examples of both methods, which both Merivale and Heller have overlooked; and L. Holzapfel (see p. 719, n. 1, *infra*) shows that Cicero often used the exclusive method, which, for numbers from ten upwards, appears to have been invariable (Th. Mommsen, *Die röm. Chron. bis auf Caesar*, 2nd ed., 1859, p. 163, n. 17, and L. Holzapfel, *Röm. Chron.*, 1885, p. 353). Those, however, who are familiar with the language of the *Commentaries* will have no difficulty in concluding that Caesar himself, in that work, used the inclusive method. In *B. G.*, vi, 33, § 4, he writes, *discedens post diem VII sese reversurum confirmat*; and in vi, 35, § 1, *diesque adpetebat VII quem ad diem Caesar... reverti constituerat*. Therefore, as Merivale observes, '*dies VII = post diem VII*.' See also Th. Mommsen, *Die röm. Chron. bis auf Caesar*, 2nd ed., 1859, p. 163, n. 317; L. Holzapfel, *Röm. Chron.*, pp. 353–6; *Philologus*, xlix, 1890, p. 74; and Rice Holmes, *Caesar's Conquest of Gaul*, 1899, pp. 723–5.

[1] *Hist. de Jules César*, ii, 186, n. 2.

[2] *Gall. Krieg*, 1880, i, 147, n. 8.

[3] xl, 1, § 3.

[4] *Neue Jahrbücher für Philologie*, &c., cliii, 1896, p. 270.

[5] *B. G.*, v, 8, § 3.

that Caesar did land in the same 'part of the island'[1] in 54 B.C. and in 55. For, as we shall subsequently see, if he landed on both occasions in East Kent, the coast which answers to the requirements of his narrative lies within the extreme limits of Walmer and Sandwich: if in 55 B.C. he landed at any point west of the South Foreland, it is not possible to suggest any reason why he should have chosen in the following year a new landing-place also on the west of that promontory but in a different 'part of the island'; and not only has it never been suggested, but it is incredible that he should have landed in 55 B.C. on one side, and in 54 on the other side of the South Foreland.

It is hardly necessary to add that before his fleet hove in sight in 54 B.C. the Britons assembled in great force to oppose his landing: in other words, they felt sure that he would attempt to land at or near the place where he had landed the year before.

V. THE VARIOUS THEORIES ABOUT CAESAR'S PLACE OF LANDING

Not less than a dozen different theories have been formed regarding the place of Caesar's landing. It has been identified with Weybourne on the coast of Norfolk; with Richborough; with the neighbourhood of Sandwich; with Deal, or, to speak more correctly, the coast between Deal Castle and Walmer Castle; with Dover, Folkestone, Hythe, Lympne, Hurst on the northern fringe of Romney Marsh, Bonnington near Appledore, Rye, Bulverhythe, and Pevensey. Most of these theories, however, obviously fall into groups. Richborough, Sandwich, and Deal; Hythe, Lympne, Hurst, and Bonnington; Bulverhythe and Pevensey,—these three groups represent three main theories, each of which has undergone modification in detail. The rest may be summarily dismissed. The absurd suggestion that Caesar landed in Norfolk was elaborated in two successive pamphlets;[2] and, what is still more amazing, a zealous antiquary thought it necessary to devote a third[3] to its refutation. Neither Folkestone nor Rye has now any advocates; and the absurdity of their pretensions must be self-evident to every intelligent reader of the *Commentaries*. The theory that Caesar landed at Dover is only worth mentioning because it was seriously maintained by the eminent geographer, Konrad Mannert;[4] and perhaps Heller underestimated the acumen of his readers when he took the trouble to confute it.[5] The claim of Bonnington was maintained with considerable ingenuity by a professional advocate in a book[6] which

[1] See p. 599, *supra*.

[2] S. F. Surtees, *Julius Caesar: did he cross the Channel?* 1866; *Julius Caesar: showing beyond reasonable doubt, that he never crossed the Channel, but sailed from Zeeland, and landed in Norfolk*, 1868.

[3] J. Wainwright, *Julius Caesar; did he cross the Channel. Reviewed*, 1869.

[4] *Geogr. der Griechen und Römer*, Zweyter Theil, Zweyter Heft, 1795, p. 29.

[5] *Zeitschrift für allgemeine Erdkunde*, 1865, p. 100.

[6] *C. J. Caesar's Brit. Expeditions*, &c., 1868. By F. H. Appach.

George Long,[1] who dissented from its conclusions, commended as 'a work of real value'; and it would not be safe to ignore it. Even the view that Caesar landed at Pevensey demands consideration. It was first put forward in 1852 by the late Astronomer Royal, who defended it against a series of attacks with equal ability and vivacity: a few years ago it was resuscitated by Professor Ridgeway: Mr. Warde Fowler[2] observes that 'much can be said in favour of this opinion'; and the late Camden Professor of Ancient History in the University of Oxford[3] was inclined to accept it. But the controversies which have attracted most attention have been centred between the advocates of Lympne or Hythe on the one side and of Deal or Sandwich on the other. And, although there are many collateral questions, the chief point at issue is this,—when Caesar sailed with wind and tide in his favour from the place where he anchored on the morning of his first voyage, and steered for the place where he landed, was the tidal stream running up or down the Channel? Among those who have recently approached the subject the prevailing belief would appear to be that it has been proved that he sailed down. 'The old belief,' writes Mr. Warde Fowler,[4] 'that he turned eastwards and landed at Deal cannot, in the present state of our knowledge of the tides, be any longer maintained.' I engage to convince every reader who will give me his attention that the so-called proof is no proof at all.

VI. THE QUESTION OF THE TIDES

Before we attempt to construct a tide-table for the 26th and 27th of August, 55 B.C., we must first satisfy ourselves whether in that year, at any given period of the moon's age, the tidal stream in the eastern part of the English Channel began to flow and to ebb at precisely the same time as it does in similar circumstances now. On this point there has been much divergence of opinion. Dr. Guest, the late geologist George Dowker, and Professor Montagu Burrows have all argued that the changes which have taken place in the configuration of the coast[5] must have produced changes in the tidal currents. The points on which Dr. Guest laid special stress were, that in Caesar's time Thanet was an island; that Dungeness did not then exist; that Romney Marsh was covered at high tide by an estuary 50,000 acres in extent; and that the estuary of the Thames was far wider than it is now.[6] Dowker called attention to the great changes which, since Caesar's time, must have taken place in the Goodwin Sands: 'would no effect,' he asked,

[1] *Decline of the Roman Republic*, iv, 441.
[2] *Julius Caesar*, 1892, p. 196, note.
[3] H. F. Pelham, *Outlines of Roman History*, 1895, p. 257.
[4] *Julius Caesar*, p. 196.
[5] See pp. 518–52, *supra*.
[6] *Archaeol. Journal*, xxi, 1864, pp. 235, 237; *Origines Celticae*, ii, 366. Dr. Guest's notions about the Thames were perhaps incorrect. See p. 696, *infra*.

'be felt by the tides if the Goodwins were now an island?' Again, observing that Drew 'points out how the beach formerly near Rye had been swept away, and re-deposited in a different direction', he concluded that 'geological changes of outline have altered the direction and velocity of the currents'.[1] Some years later he returned to the subject. 'If,' he wrote,[2] 'we assume the Straits are now one mile wider than when Caesar visited our shores,[3] the tide which runs with a velocity of about three miles an hour up Channel, would carry more water into the German Ocean than a river a mile wide and 15 feet deep. . . . There are other changes also that have taken place in the German Ocean, which must have exerted immense influence on the tides when we remember that a north-east wind will materially heighten the tidal lever by forcing up the water of the North Sea. The travelling of beach in an eastward direction shows that the set of the tide is more strong in that direction now on some part of the coast than formerly.' Finally, Professor Montagu Burrows remarks[4] that 'Not only may the depth of the Channel have largely varied, but the space over which the tides travel must be at least two miles wider than it was some 2,000 years ago,[5] and therefore the point of meeting of the North and South tide-streams cannot possibly be exactly the same'.

On the other hand, Airy says, 'I express my opinion without hesitation that no conceivable changes in the coast within historical times can have produced any sensible change in the relation of the tidal currents to the moon's age.'[6]

I have submitted these remarks to Sir George Darwin, the author of the articles on the tides in the Ninth Edition of the *Encyclopaedia Britannica* and the Supplement to that edition. 'In my opinion,' he replies, 'Airy is absolutely right and Burrows and the others wrong. A channel from Sandwich to Reculver could not have made any sensible change, and so also it would be impossible to detect the difference if Goodwin Sands were an island. All the phenomena now observed must have occurred at the same times within, say, a minute, and with an intensity measurably identical in the days of Caesar. Even if you were in a position to indicate exactly the nature of the changes in the channel since that time, it would be impossible to compute the nature of the *excessively minute* changes in the currents.'

This decisive answer will not be seriously gainsaid. Evidently the divergence of opinion is between those who are not and those who are qualified to judge.

Airy says that 'on the day of Caesar's landing the tide off Dover turned to the west about 1 h. in the afternoon, and at 3 h. it would

[1] *Archaeol. Journal*, xxxiii, 1876, pp. 61, 63–4.
[2] *Twenty-third Report East Kent Nat. Hist. Soc.*, 1881, p. 57.
[3] This, as I have shown, is an assumption which we have no right to make.
[4] *The Cinque Ports*, p. 8.
[5] I have already shown (pp. 528–30, *supra*) that this estimate is enormously exaggerated.
[6] *Athenæum*, Sept. 5, 1863, p. 303.

be running with a strong stream to the west '.[1] Airy, as we have seen,[2] supposes that the day of Caesar's landing was the 28th of August; but if the statement which I have just quoted is accurate, it follows that even on the 26th the tide at 3 p.m. was running westward. Again, in 1866, the Admiralty Hydrographer affirmed that on the 27th of August, 55 B.C., the current ran westward until 6.30 p.m.; [3] and if he is right, it must on the previous day, unless the circumstances were abnormal, have continued flowing in the same direction until 5 p.m. or later. Lewin,[4] relying upon a table compiled by a Mr. Barton of Dover, and based upon observations made by 'some experienced pilot or fisherman', and upon another table filled up from actual observation 'on every day of July, 1862', states that 'with high water . . . at 7.31 a.m., the tide could not turn eastward at the earliest until 4.26 p.m., and at the latest not until 5.21 p.m.' Nevertheless, he admits [5] that, with high tide at 7.31 a.m., the tide might possibly have turned eastward at 4 p.m. Finally, Mr. H. E. Malden tells us that 'at any time that afternoon between two o'clock and seven, in any part of the Channel between Dunge Ness and Dover, the tidal current was running westward . . . when Sir George Airy, the greatest authority of the century upon the tides, says that they were the same then as now, *cadit quaestio*. We are lifted out of the uncertainties of historical topography into the certainties of scientific knowledge.' [6]

This pronouncement is certainly calculated to overawe the timid inquirer. Nevertheless, I venture to suggest that a man who knows little or nothing about the tides should refrain from patting on the back one who knew a great deal, but who did not think it necessary for his purpose to tell all that he knew.

Airy may have been the greatest authority of the century upon the tides: but, apart from his asseveration that they were the same then as now, in the contribution which he made to the solution of the problem which we are investigating he relied upon authorities which are accessible to everybody. Those authorities (I will mention a few besides those which Airy used) are the *Nautical Almanac*; *Tide Tables for the British and Irish Ports* (published by order of the Lords Commissioners of the Admiralty); the *Channel Pilot*; *Tidal Streams, English and Irish Channels* (an Atlas of 12 charts published at the Admiralty in 1899); Captain Usborne Moore's *Report on Observations . . . in the Straits of Dover* (also published at the Admiralty in 1899); an article by Admiral Sir F. W. Beechey published in *Philosophical Transactions*, volume cxli, 1851, pages 703–18; and an article published in *Archaeologia*, volume xxxix, 1863, pages 277–302. The last-named article includes a report of observations on the tidal streams in the Straits of Dover, made in 1862 under the superintendence of E. K. Calver, R.N. These

[1] *Archaeologia*, xxxiv, 1852, p. 241.
[2] See pp. 601–2, *supra*.
[3] *Archaeologia*, xli, 1867, p. 272.
[4] *The Invasion of Britain*, &c., 1862, pp. lxxxi–lxxxiii.
[5] *Ib.*, p. lxxxvi.
[6] *Journal of Philology*, xvii, 1888, p. 172.

observations were made for the purpose of settling the question whether the stream with which Caesar sailed from his anchorage ran up or down the Channel. They were made 'in comparatively still weather' on the 21st of August, the 4th and 5th of September, and the 4th of October, 1862, at eleven distinct stations within a space extending to one mile and a half from the shore, and from the South Foreland to Shakespeare's Cliff. The days on which they were made were, reckoning inclusively, respectively the fifth before the new moon, and the fifth, the fourth, and the fourth before the full moon. 'From the average of these observations,' says Calver, 'it appears that, when high water at Dover occurs about 7 h. 30 m. a.m., the inshore flood or easterly-going stream . . . turns 4 h. 48 m. after it is high water upon the shore. Taking then, for example, a 7 h. 31 m. a.m. high water, and assuming that the ebb or westerly-going stream runs on the average for 6¼ hours, it follows that the flood or easterly-going stream on that day would turn off Dover at 12 h. 19 m., and the succeeding ebb . . . would run to the westward until 6 h. 34 m. p.m.[1]' Vice-Admiral W. H. Smyth, summing up the results of the observations, remarked that the tide turned westward 'soonest near the beach and latest in the offing', and that 'the turn is sooner to the east of Dover than to the west, still not differing more than one hour'.[2]

So far as we have yet examined them, the results of these observations fully bear out what Airy and Lewin maintain,—if it is assumed that high tide at Dover on the day of Caesar's landing must have occurred about 7.30 a.m. But if we scrutinize the tables more closely, and give due weight to certain other facts emphasized by our authorities, we shall see that the dogmatism of Airy and Lewin is unjustifiable. They and their followers make no allowance for the great influence which winds exert upon the tidal streams in the Channel.[3] Moreover, it is useless to base conclusions upon the average of the results obtained in Surveyor Calver's observations. If we scan the tables which give those results in detail, we shall find that although, on the average, the stream turned westward 4 h. 48 m. after high water at Dover, yet on the 21st of August it turned only 3 h. 40 m. after high water.[4] We shall also find that the duration of the flood, as observed by Calver, varied from 4 to 7 hours; and although the duration of the ebb was only observed twice,[5] it may actually have varied as much. On the first of the two occasions on which it was observed it was 6 hours, 10 minutes; on the second,

[1] *Archaeologia*, xxxix, 1863, p. 289.

[2] *Ib.*, pp. 300-2.

[3] 'Winds,' says Beechey (*ib.*, xxxiv, 1852, p. 239), 'greatly affect the time of turn of the stream.' 'Strong winds,' says Mr. S. H. Brown, Trinity House Pilot (*Diagrams and Tables of Tidal Streams*, &c., 1895, p. 4), accelerate and prolong the stream running in the same direction, retard the opposing stream,' &c. See also *The Channel Pilot*, part i, 1900, p. 541, from which we learn that 'on some occasions . . . 8 hours north-eastern and only 4 hours south-western streams have been found'.

[4] *Archaeologia*, xxxix, 1863, p. 290.

[5] *Ib.*, pp. 290, 294, 301.

only 5 hours, 53 minutes;[1] and, according to the *Channel Pilot*,[2] it is occasionally only 4 hours. Therefore it is possible that if on the day of Caesar's landing high water occurred at Dover at 7.31 a.m., the tide may have turned westward as early as 11.11 a.m., and may have continued to run westward for a period not longer than 5 hours, 53 minutes, that is to say, until 5.4 p.m.

But this is not all. It is desirable that the reader should become acquainted with Lewin's methods of reasoning. Lewin himself, after studying the *Admiralty Tide Tables* for the year 1859, admits that 'on January 14th, being the fourth day before the full moon, high water at Dover is at 5.31 a.m.', and that it may possibly have occurred as early on the day of Caesar's landing. This admission throws a breaking strain upon his theory; but by dint of dexterous manipulation of the facts he is just able to make a show of saving it from ignominious collapse. 'As,' he says, 'the stream turns at four hours after high water, and continues for seven hours, it turns at the earliest at 9.31 a.m. and runs till 4.31 p.m. . . . In no case, therefore, would the tide be running east at 3 p.m.' But Lewin is here compelled to ignore observations of which, in another part of his book, he makes free use. As we have already seen, he admits[3] that on a day when high tide occurred at 7.31 a.m. the stream might turn eastward at 4.26 or even at 4 p.m. If he is right, it follows that on a day when high tide occurred at 5.31 a.m. the stream might turn eastward at 2.26 or even at 2 p.m. To make this admission, from which, if he had been confronted with his own words, he could by no subtlety have escaped, would have been to throw up his case; and such candour would have been too much to expect from a professional advocate. Let him, however, shift his ground, if he pleases, and rely upon Calver's observations. They will not avail him. We have just seen that, according to Calver, the stream turned westward on the 21st of August, 1862, 3 h. 40 m. after high water; and the duration of the westward stream, on one of the two occasions on which he observed it, was only 5 h. 53 m.[4] According to these data, if, on the day of Caesar's landing, high tide had occurred at 5.31 a.m., the westward stream might have ceased at 3.4 p.m. It would have been impossible for Lewin, if he had been required to take account of this statement, to deny it. But to admit it would have been to sign the death-warrant of his own theory.

So far I have only been concerned to show that Lewin's whole train of reasoning, examined in the light of the evidence which he himself adduces, is radically unsound. I have argued on his hypothesis—that high tide at Dover, on the day of Caesar's landing, *may* have occurred at the earliest time at which it can possibly occur on the fourth day before the full moon. Neither Lewin nor Airy nor any other commentator has attempted to determine, by the aid of lunar tables, the hour at which, on the day in question,

[1] According to Admiral Smyth (*ib.*, p. 301), 6 hours and 5¾ hours respectively.
[2] Part i, 1900, p. 354.
[3] *The Invasion of Britain*, &c., 1862, pp. lxxxiv, lxxxvi.
[4] *Archaeologia*, xxxix, 1863, pp. 290, 294.

high tide did actually occur.[1] Nevertheless, if the problem which we intend to solve is to be attacked in a scientific spirit, the hour ought to be determined. Messrs. John A. Sprigge, William Fraser Doak, M.A., F.R.A.S., and T. Charlton Hudson, B.A., F.R.A.S., all of the Nautical Almanac Office, have been so kind as to determine it for me. Their calculations are preserved; and at the end of this article, on page 665, will be found a memorandum, in which they have described the method on which they worked. It will be sufficient here to state the results, the error in which, as they point out, is probably insignificant. On the 26th of August, 55 B.C., the Greenwich mean time of high water at Dover was 6.21 a.m.; on the 27th, 7.42 a.m.; and on the 28th, 8.44 a.m.[2] Thus it turns out that on the day of Caesar's landing high water did not occur at all early. This fact, however, will not sustain the theory of Airy and Lewin. Since high water at Dover on the 26th of August occurred at 6.21 a.m., and since the tidal stream, according to one of Calver's observations, turned 3 hours, 40 minutes after high water, the stream may have turned westward at 10.1 a.m. Assuming that the duration of the westward stream was 5 hours, 53 minutes—the same as that recorded on one occasion by Calver—the stream would have ceased at 3.54 p.m. But Calver's observations were made 'in comparatively still weather';[3] and, to quote Admiral Beechey, 'winds greatly affect the time of turn of the stream.'[4] Now Caesar sailed from his anchorage with wind as well as tide in his favour; and the wind which carried him to his landing-place may have accelerated the turn of the stream. It is clear, therefore, that on the day of Caesar's landing the stream may have turned eastward earlier than 3.54 p.m.; and if it turned twenty-five minutes earlier, it turned in 'the ninth hour'.

[1] It is remarkable that most of the writers who have dealt with the question of Caesar's landing-place should have taken so little pains to inform themselves about the tides. Thus Cardwell, who was in 1860 Camden Professor of Ancient History at Oxford, says that 'If you know what was the time of high-water at Folkestone at any full moon during the present year, you know the time of high-water at the same place whenever the moon was full a hundred or a thousand years ago. It is also a fact that each successive tide is later by twenty-five minutes than the one which had preceded it' (*Archaeol. Cant.*, iii, 1860, p. 7). Both these statements are grossly inaccurate, as the professor might have seen if he had taken the trouble to devote half an hour to the study of the *Admiralty Tide Tables*. Thus, taking the August full moon of the years 1883–1900, the time of high tide at Folkestone varied between 11.5 a.m. in 1896 and 10.17 a.m. in 1900; while the time of high tide of the fifth day before the full moon varied between 6.21 a.m. in 1893 and 4.46 a.m. in 1898; and high tide on the morning of August 19, 1896, was 90 minutes later than high tide on the morning of August 18, not 50 minutes, as it should have been according to Cardwell. If he had said that '*on the average* each successive tide is later by twenty-five minutes than the one which had preceded it', he would have told the truth.

[2] See p. 666, *infra*.

[3] On the day when the stream turned westward soonest—only 3 hours 40 minutes after high water—the force of the wind was all but imperceptible (*Archaeologia*, xxxix, 1863, p. 290).

[4] See p. 608, n. 3, *supra*.

But, it will be objected, Caesar may have landed on the 27th of August; and in that case the stream could not have turned eastward before the close of the ninth hour. Certainly it could not have done so unless it had turned westward unusually early, or unless its westward duration had but little exceeded four hours; and although this has been shown to be within the bounds of possibility,[1] it is to the last degree improbable. But if he landed on the 26th of August, an assumption which has been proved to be not inconsistent with his narrative,[2] it is not improbable that the stream may have turned eastward in the ninth hour; and this is all that I am concerned at present to show.

It may be said that, in order to refute the dogma of Airy and Lewin, I have supposed an extreme case. I can only say that I did not start with the intention either of refuting or defending that dogma: I merely examined it, and found that it would not bear examination. And I am justified in supposing an extreme, or rather an exceptional, case because Airy and Lewin have both affirmed that it is absolutely impossible that in the ninth hour on the day of Caesar's first landing in Britain the tidal stream can have been running towards the east. But, supposing that it did not turn eastward until after the ninth hour, still the theory that Caesar must have sailed in the opposite direction will not stand. For the reader, if he has patience to bear with me to the end, will convince himself, from Caesar's own words, that Caesar did not stir from his anchorage until after the ninth hour had passed.

VII. THE THEORY THAT CAESAR LANDED AT PEVENSEY

The two distinguished advocates of the theory that Caesar landed at Pevensey are not in complete accord. Airy holds that he sailed both in 55 and in 54 B.C. from the mouth of the Somme;[3] Professor Ridgeway from Wissant.[4] It has been proved in my article on the Portus Itius that he started from Boulogne; and whoever accepts that proof will, perhaps, skip this section. I am willing, however, for the sake of argument, to accept in turn both Airy's identification of the Portus Itius and that of Professor Ridgeway: but I may remark that when Airy wrote he had forgotten that in Caesar's time there was a natural harbour at Pevensey;[5] and if Caesar had landed in a harbour he would not have left the fact unnoticed.

If we are to accept the premiss on which Airy himself lays so much stress, namely, that in 55 B.C. the tidal currents in the Channel, at any given period of the moon's age, were the same as they are now,

[1] See p. 608, n. 3, *supra*.
[2] See pp. 600–1, *supra*.
[3] *Archaeologia*, xxxiv, 1852, p. 236.
[4] *Journal of Philology*, xix, 1891, pp. 141–2.
[5] See the note at the end of Airy's article in *Archaeologia*, xxxiv, 1852; Sir C. Lyell, *Principles of Geology*, 1875, p. 534; and *Proc. Inst. Civil Engineers*, clix, 1905, p. 129.

Caesar did not land at Pevensey. Airy, as we have seen, assumes that Caesar landed, in 55 B.C., on the third day before the full moon, and, appealing to the authority of Sir F. W. Beechey, he affirms that, off Hastings, the current turns westward five miles from the coast two hours later than it does close inshore. 'If,' he concludes, 'we suppose Caesar to have first attempted the neighbourhood of St. Leonards, the tide, which a few miles from shore had turned to the west at 11 h., was, at 3 h., running in full stream to the west.'[1] But, in order to prop up his theory, Airy is forced to place Caesar's anchorage at five nautical or nearly six statute miles from the shore. To prove that such an assumption is absurd, it is only necessary to say that, at a distance of five nautical miles, Caesar could not have seen the armed men who, as he tells us,[2] were swarming upon the cliffs, without the assistance of a powerful telescope.[3] Lewin[4] rightly concludes that Caesar must have anchored within a mile from the shore, at the outside, and probably within half a mile. Now high water at Dover on the 27th of August, 55 B.C., occurred at 7.42, and therefore at Hastings at 7.23 a.m.[5] But off Hastings, within a mile from the shore, the current turns westward about the time of high tide,[6] runs westward for about six hours and a half, and then runs eastward for about six hours. At 3 p.m., therefore, on the 27th of August, the tide off Hastings would have been running eastward, and would have continued to do so until about 7.50 p.m. And on the 28th of August, which Airy wrongly assumes to have been the day of Caesar's landing, the tide off Hastings would have turned eastward about 2.53 p.m., and would have continued to run in that direction until about 8.53. Consequently, on the theory of the tides which Airy himself so strenuously maintains, it would have been impossible for Caesar to sail with the tide from Hastings or from St. Leonards to Pevensey.[7] Even on Airy's assumption that Caesar anchored five nautical miles from the shore, his theory

[1] *Archaeologia*, xxxiv, 1852, pp. 239, 242.

[2] *B. G.*, iv, 23, § 2.

[3] Viscount Wolseley, *The Soldier's Pocket-Book*, 1886, p. 491.—'Good eye-sight can distinguish bodies of troops at 2,000 yards; at that distance a man or horse appears like a dot; at 1,200 yards cavalry is distinguished from infantry,' &c. I am aware that in certain primitive districts, for instance the islands of Inishbofin and Inishshank off the coast of Galway, the average range of vision is abnormally great (*Proc. Roy. Irish Acad.*, 3rd ser., iii, 1893–5, p. 324); but we may reasonably assume that Caesar could not see eight or nine times as far as a modern Englishman.

[4] *The Invasion of Britain*, &c., 1862, p. xxxiv.

[5] See p. 610, *supra*, and *Tide Tables for the British and Irish Ports*, p. 225.

[6] *Archaeologia*, xxxiv, 1852, p. 239.—'At full and change of the moon,' says Admiral Beechey, 'close in shore off Hastings the stream turns to the west at 11 ^{h}; but the turn becomes later as the distance off shore increases, and at 5 miles distance the stream turns to the west at 1 ^{h} . . . The stream runs to the west about 6½ hours,' &c.

[7] Airy himself, as we have seen, makes no allowance for any variation which may have been produced by wind or other causes from the normal hour of the turn of the stream. I am willing to make any reasonable allowance; but the intelligent reader will have seen that no such allowance would disturb the conclusion which I have reached in the text.

cannot stand: he can only make a show of propping it up by assuming that Caesar landed on the 28th of August. On the 27th, the stream would have turned westward at about 9.30 a.m., and would have ceased running westward about 4 p.m. Therefore, even supposing that Caesar started on his seven miles' sail in the ninth hour,[1] he would have done so on the very last of the tidal stream, when it was barely moving; and it would have turned against him before he had half finished his voyage.

Very wisely, from his own point of view—for his silence has hitherto passed unnoticed—Airy ignored Caesar's account of the voyage of his cavalry transports. Some of them, as we have seen, were 'swept down in great peril' (*magno suo cum periculo deicerentur*[2]), evidently running before the gale, 'to the lower and more westerly part of the island' (*ad inferiorem partem insulae quae est propius solis occasum*[3]): the others were carried back to the port from which they had started. That port, according to Airy, was the mouth of the Authie. The gale evidently blew from about the north-east; but, in order to give Airy the fullest latitude, I will assume that it was from the north-north-east, although in either case the ships which ran before the wind, once they had got under the lee of Beachy Head, would have been in smooth water! The course which the transports would have had to steer for the Authie, if they had been sighted off Pevensey, would have been SE. by E. 2° S., or within less than nine points of the wind. But in the gale they could hardly have made less than four points of lee-way.[4] Therefore, in order to reach their supposed destination, they would have been obliged to lie within less than five points of the wind, which they could not have done.[5] 'No!' said the harbour-master of Dover to me, after he had studied the chart, 'No! they would have fetched Dieppe.'[6] I have assumed that they could work to windward: if they could not, it is self-evident that they could not have returned to the mouth of the Authie.

But if any one is not convinced, let him hear Airy plead his own cause.

1. Airy argues that Volusenus would never have recommended Caesar to land under the cliffs of Dover, or at any point under the cliffs between Folkestone and Hythe. 'No commander,' he says, 'would steer ships to a mural cliff three hundred feet high, with the intention of landing in order to invade the country; nor would any defenders station themselves there to repel an invasion; nor could a "telum" be thrown with any aim. But a daring officer

[1] See pp. 648–9, *infra*. [2] *B. G.*, iv, 28, § 2. [3] *Ib.*

[4] According to Falconer's *Marine Dictionary*, 1815, p. 220, the lee-way of a ship in a gale varies from 5½ to 6½ points. The amount of course depends upon the force of the gale, the build of the ship, and other circumstances.

[5] See Addenda, p. 740.

[6] I need hardly say that if Caesar's transports had been anchored off Pevensey on the night of the full moon a north-north-easterly gale could not have driven them ashore unless they had been inside the harbour, which Caesar would have mentioned.

might steer to a less perpendicular cliff, ten to thirty feet high, with the intention of forcing a landing . . . Such are the cliffs between Hastings and Pevensey; and I conclude that they answer exactly to Caesar's description.' Assuming that the cliffs off which Caesar anchored, when he first approached the British coast, were in the neighbourhood of St. Leonards, Airy affirms that 'the run of eight[1] miles would bring him to the beach of Pevensey, answering perfectly to his description'.[2]

Now whether Caesar did or did not steer towards 'a mural cliff three hundred feet high', he certainly anchored off cliffs which he calls 'precipitous heights' (*angusti montes*); and Airy makes too great a demand upon our credulity when he requires us to believe that Caesar described by the words *angusti montes* a 'cliff ten to thirty feet high'. So much for the theory that Caesar anchored off the clifflets of St. Leonards:[3] the argument that he could not have anchored off the cliffs of Dover shall be considered in its proper place. The reader has of course already observed that Volusenus, being a sane man, would never have recommended Caesar to 'force a landing' under any cliffs, great or small.

2. Airy argues that the Britons would naturally have assembled at Pevensey in order to oppose Caesar's landing, because 'Pevensey was the weakest point of Britain'.[4] No! replies Lewin, Pevensey was not *then* the weakest point; for it was 'backed by the Andred Forest'.[5] Airy[6] triumphantly observes that William the Conqueror landed there; but Lewin[7] rejoins that when William landed the forest presented less difficulty to an invader than in Caesar's time, as the Romans and the Saxons must have made clearances. Be this as it may, it is certain that William did not attempt to march northward through the forest. He returned, immediately after his victory, to Hastings: from Hastings he marched eastward to Romney, and from Romney to Dover.[8] He had his own reasons for landing at Pevensey: but Caesar, for reasons equally good, chose the shortest passage; and although, as I have shown,[9] these words are not to be taken in an absolutely literal sense, they alone exclude the notion that Caesar landed in Sussex. Obviously it is in the

[1] Instead of 'eight' Airy should of course have written 'seven'.

[2] *Archaeologia*, xxxiv, 1852, pp. 239, 241–2.

[3] Lewin (*The Invasion of Britain*, &c., 1862, pp. xxxii–xxxiii) points out that if Caesar approached the British coast anywhere near Pevensey, 'he must have anchored, in the first instance, somewhere off the high cliffs between Hastings and Cliff's End,' because at no point between Hastings and Pevensey are the 'precipitous heights' off which he anchored to be found. But, continues Lewin, if he anchored at any point between Hastings and Cliff's End, 'eight Roman miles would not carry him so far as Pevensey Marsh.' [For 'eight' read 'seven'.]

[4] *Athenæum*, Sept. 10, 1859, p. 338.

[5] *The Invasion of Britain*, &c., 1862, pp. xxiv–xxv.

[6] *Athenæum*, Sept. 10, 1859, p. 338.

[7] *The Invasion of Britain*, &c., 1862, p. xxvi.

[8] E. A. Freeman's *Norman Conquest*, iii, 532–4; *Journal of Philology*, xx, 1892, pp. 63–4.

[9] See p. 571, *supra*.

last degree improbable that Volusenus would have reconnoitred the coast so far westward as Pevensey; nor could the Britons have expected that Caesar would be so foolish as to double the length of his voyage in order to land there.[1]

3. Airy argued that, except on the hypothesis that Caesar landed at Pevensey, it is impossible to account for the long duration of his first voyage. His rate of sailing, said Airy, if, as Dr. Guest maintained, he had started from Wissant and anchored off the Dover cliffs, would not have exceeded two miles an hour. 'When in Shetland,' he adds, 'I have sailed in one of the ordinary fishing-boats of the country, hoisting a single lug-sail . . . with a pleasant, easy breeze (sometimes dying away), from Lerwick to the head of Balta Sound, in Unst, in about eight hours. The distance, as measured on the Admiralty Chart, exceeds forty nautical miles.'[2]

This was one of Airy's more plausible arguments; and it demands consideration. To begin with, it must be pointed out that Caesar did not, as Dr. Guest believed, sail from Wissant, but from Boulogne, which is more than seven nautical miles further from Dover. Airy assumed[3] that Caesar's first voyage lasted from midnight till 10 a.m. But it is impossible to say how long it lasted. Caesar does not say that he started at midnight: he says that he started 'about the third watch' (*tertia fere vigilia*); and the third watch lasted, on the night of the 25th—26th of August, from midnight till 2.32 a.m., on the night of the 26th—27th till 2.33 a.m.[4] Nor does he say that he reached Britain at 10 a.m.; he says that he reached it 'about the fourth hour of the day', which lasted on the 26th of August from 8.33 to 9.42.[5] As Mr. Peskett says, 'the possible duration of the voyage lies between the extreme limits of 9 h. 40′ and 6 hours.'[6] Split the difference, and you will find that the average rate of sailing would have been about three knots and a half per hour. The answer to Airy's argument is that Caesar's narrative is quite consistent with the view that his ships may have remained for some time anchored off the Gallic coast in the expectation that the cavalry

[1] When Professor Ridgeway resuscitated Airy's theory, he found himself called upon to meet the objection which we have just considered. Mr. Malden (*Journal of Philology*, xix, 1891, pp. 197–8) told him that Caesar would never have landed 'opposite the Great Wealden Forest, where resistance would be easy and supplies scarce'. The professor replied (*ib.*, p. 206) that a passage in Caesar's narrative proves that he *did* land opposite the Wealden Forest. The passage will be found in the ninth chapter of Caesar's Fifth Book, in which he describes the combat which took place on the banks of a stream, about 12 miles from his camp, the day after his second landing. The Britons, on being driven from the banks, withdrew into woods (*repulsi ab equitatu se in silvas abdiderunt*). Mr. Malden (*Journal of Philology*, xx, 1892, p. 63) makes the obvious reply:—'All that Caesar tells us is that there were woods in which the Britons took refuge . . . but Caesar does not lead us to believe that he landed in a place where his march inland was barred by an all but impenetrable forest 30 to 40 miles wide.'

[2] *Athenæum*, Sept. 5, 1863, p. 302.

[3] *Archaeologia*, xxxiv, 1852, p. 240.

[4] Napoleon III, *Hist. de Jules César*, ii, 553.

[5] *Ib.*, p. 553.

[6] *Journal of Philology*, xx, 1892, p. 197.

transports would sail out of Ambleteuse harbour to join them; and, further, that the wind may have shifted to an unfavourable quarter before the voyage was at an end.[1]

4. Airy[2] maintains that a river corresponding with Caesar's description of the one on the banks of which he defeated the Britons on the day after his second landing,[3] is to be found in the neighbourhood of Pevensey, and of Pevensey only. That river, he says, was the Rother, and the scene of the victory was Robertsbridge. He produces evidence to show that if Rye Sluice were broken, 'the whole valley at Robertsbridge would now become a great tidal morass.' This, he continues, 'was its state in the age of Caesar, and it must have been a very formidable defence against an army advancing from the coast.'

Undoubtedly; so formidable that it would have been absolutely impassable. How Caesar's cavalry succeeded in forcing their way over this 'great tidal morass' Airy omits to explain. If he had studied Caesar's description[4] of the much less formidable morass over which his ablest marshal, Labienus, tried in vain to construct a causeway, and from which he was obliged to retreat, he would hardly have made Caesar attempt to cross 'a great tidal morass' in the face of an enemy.

Caesar, as we have seen, descried at daybreak, on his second voyage, the coast of Britain 'lying behind on the left';[5] and if these words mean, as all commentators except Airy and Professor Ridgeway maintain, that he had drifted to some point east or north-east of the South Foreland, they alone dispose of Airy's theory. Airy of course saw this; and accordingly he put his own construction upon the passage. 'I cannot conceive,' he says,[6] 'that the expression refers to any direction but to that of the drift; it asserts that, in reference to the direction of tidal current, the coast was on the left hand. It is therefore indecisive as to place.'

Lewin, in his reply,[7] overlooked one consideration, which by itself overthrows Airy's interpretation. If, as Airy would have us believe, Caesar's vessel had not drifted as far east as Dover, she was, owing to the direction of the current, moving parallel with the British coast.[8] How, then, could Caesar, in the case supposed by Airy, have said that he saw Britain '*lying behind* on the left' (*sub sinistra relictam*)?

The theory that Caesar landed at Pevensey is irreconcilable with the fact that the four chieftains who attacked his naval camp in

[1] The late arrival of some of Caesar's ships (*B. G.*, iv, 23, § 4) can only be accounted for on the assumption that during the voyage the wind shifted to an unfavourable quarter,—an assumption which is verified by Caesar's express statement (iv, 26, § 5) that the cavalry transports were unable 'to make the island', and had to put back.

[2] *Essays on the Invasion of Britain*, &c., pp. 35–6.

[3] *B. G.*, v, 9, §§ 2–4.

[4] *Ib.*, vii, 57, § 4; 58, §§ 1–2.

[5] *Ib.*, v, 8, § 2.

[6] *Essays on the Invasion of Britain*, &c., p. 33.

[7] *The Invasion of Britain*, &c., 1862, pp. xxvii–xxviii.

[8] See *Tidal Streams*, &c., and *Admiralty Tide Tables*, pp. 112–3, 119.

54 B.C. belonged not to Sussex but to Kent.[1] Airy endeavoured to answer this objection by the remark that the men of Kent were more numerous than those of Sussex, and would therefore have gone to the assistance of their countrymen.[2] But, replied Lewin,[3] 'as the men of Kent were distinct from the Regni, or men of Sussex, the natural inference to be drawn from the assault of the camp by the *men of Kent* surely is that the *camp was in Kent*.' I may point out further that, considering the state of internecine war in which the Britons habitually lived, and which was only suspended for the time under the pressure of a common danger,[4] it is not credible that the men of Kent would have consented to make a long march away from their own territory in order to undertake an operation which would have properly devolved upon another tribe, and unlikely that they would have been sufficiently well organized to feed their army during a march of such duration.

The distance between the mouth of the Somme, which Airy identifies with the Portus Itius, and St. Leonards, where he maintains that Caesar first reached Britain, is, as he himself says,[5] 'about 52 nautical miles,' that is to say, rather more than 65 Roman miles: the distance between the Portus Itius and Britain, according to Caesar's estimate,[6] was about 30 Roman miles. To say nothing of this glaring discrepancy, Caesar's account of his return voyage from Britain to Gaul in 54 B.C. presents a difficulty which taxed all Airy's ingenuity to explain away. Caesar[7] tells us that he started in the second watch in a dead calm (*summa tranquillitate*), and reached Gaul at daybreak. Naturally the opponents of Airy's theory insist that to cross from Pevensey to the mouth of the Somme in this time would have been impossible.

But Airy is never so confident as when he has to defend an untenable position. He roundly asserts that his critics do not understand Caesar's language. *Summa tranquillitas*, he tells them, does not mean 'a dead calm': it means 'a stiff north-west wind'. Professor Thompson, he informs us, assured him that a favourable wind 'is compatible with a "tranquillum mare"'; and he refers, in support of this view, to a passage in one of Cicero's letters,[8]—'I am forced to wait for fair weather owing to the open ships . . . of the Rhodians' (*Nos Rhodiorum aphractis ceterisque longis navibus tranquillitates aucupaturi eramus*). He also appeals to two passages in Vergil:—

placidi straverunt aequora venti,
Creber et adspirans rursus vocat Auster in altum,[9]

and

postquam alta quierunt
Aequora, tendit iter velis portumque relinquit.[10]

'It appears to me,' he observes, 'that Virgil's idea of circumstances

[1] *B. G.*, v, 22, §§ 1–2.
[2] *Athenæum*, Sept. 10, 1859, p. 338.
[3] *The Invasion of Britain*, &c., 1862, p. xxvii.
[4] *B. G.*, v, 11, §§ 8–9.
[5] *Archaeologia*, xxxiv, 1852, p. 240.
[6] *B. G.*, v, 2, § 3.
[7] *Ib.*, 23, §§ 5–6.
[8] *Att.*, vi, 8, § 4.
[9] *Aen.*, v, 763–4
[10] *Ib.*, vii, 6–7.

favourable to navigation always implied the co-existence of brisk wind and smooth water.' The idea that Caesar's fleet was rowed across the Channel he scouts as ridiculous. 'If,' he adds, 'with smooth water there had been a brisk breeze, the steerage would have been good . . . the voyage would have been easy . . . we have only to suppose a stiff north-west wind, capable of carrying the ships 7 or 8 miles an hour.' [1]

Now as to the first of these passages, the context shows that Cicero had been weatherbound by the violence of the trade winds; and he uses the word *tranquillitates* in the sense of 'fine weather' in contrast with these.[2] His vessels were undecked; and therefore he could not venture to set sail in a rough sea. It can hardly be inferred from this passage, which Airy does not understand, that *summa tranquillitas* means 'a stiff north-west wind'. The first passage quoted from Vergil simply says that gentle winds (*placidi venti*—an expression by no means identical with *summa tranquillitas*) stilled the sea, and that then a southerly wind invited Aeneas to set sail: the second tells us that Aeneas set sail after the cessation of a storm. If Cicero does not imply that *summa tranquillitas* means 'a stiff north-west wind', neither does Vergil. If Airy had really known his authorities, he would have called to mind the passage in which Cicero [3] relates, in language virtually identical with that of Caesar, that he was prevented from sailing by 'an astonishingly dead calm' (*mirae tranquillitates*). And if he had known his Caesar, he would have thought of the passage [4] which tells how the ships of the Veneti were becalmed in their fight with Decimus Brutus,—'suddenly there was a dead calm, and they could not stir' (*tanta subito malacia ac tranquillitas exstitit ut se ex loco commovere non possent*). If *tanta tranquillitas* means 'such a dead calm', as it assuredly does, it is not easy to see how *summa tranquillitas* can mean 'a stiff north-west wind'. If these passages do not fix the meaning of *summa tranquillitas*, we may dispense with further research.[5]

I confess that I do not know whether more to admire the audacity and resource which Airy displayed in controversy, or the sublime lack of humour which permitted him to translate *summa tranquillitas* by 'a stiff north-west wind'.

So much for the late Astronomer Royal. If I do not ignore the arguments of Professor Ridgeway, it is because I am unwilling to appear wanting in due respect for his reputation. But I would ask him to explain one little difficulty which he has left unnoticed,—namely, how Caesar's cavalry transports could have contrived to

[1] *Essays on the Invasion of Britain*, &c., pp. 31, 34–5.

[2] Cf. R. Y. Tyrrell and L. C. Purser, *Correspondence of Cicero*, iii, 1890, p. 246, note. [3] *Att.*, x, 18, § 1. [4] *B. G.*, iii, 15, § 3.

[5] *Tranquillitas*, in the singular, does of course sometimes mean 'fine weather': but in such cases the context makes the meaning clear; and if Caesar had intended to express this meaning, he would, as his *usus loquendi* shows, have written *nactus idoneam ad navigandum tempestatem*. Cf. H. Meusel, *Lex. Caes.*, ii, 689.

return, as, on his theory, they must have done, from a point near Pevensey to Sangatte, that is to say, to steer E. 9° N. in the teeth of a gale which unquestionably blew from some point east of north? Let the professor consult any seafaring man, and he will learn that such a feat would have been absolutely, absurdly impossible.

1. Professor Ridgeway labours to show that the distance of Pevensey from Wissant corresponds with the distance, as stated by Caesar, of Britain from the Portus Itius. He assures us that, according to certain MSS. (which, however, he does not specify), that distance was not 'about thirty', but 'about forty miles' (*circiter milium passuum XXXX. . . . a continenti*[1]).

It is surprising that so distinguished a scholar should have committed himself to a statement which five minutes' search in any critical edition of the *Commentaries* would have shown to be unfounded. The *MSS.* to which he appeals have no existence; or, if they exist, they have never come to light.[2] But, as I have already shown, on other grounds,[3] Caesar *may* have written *XXXX*; so let Professor Ridgeway have the benefit of the doubt, though I need hardly say that the distance of Pevensey, and even of Bexhill, from Wissant is much more than forty Roman miles.

2. The professor then invokes the authority of Dion Cassius. 'If,' he argues, 'Caesar, on coming into the land of the Morini, found, as Dio says, that all the landing places opposite the continent were held by Britons, by which he evidently means the landing places in the narrow part of the Channel, would Caesar obstinately persist in crossing at the narrowest spot, or like a wise general seek for a more suitable, although more distant landing place?' This view, he pleads, is supported by the fact that Caesar describes the passage from the Portus Itius not as the shortest, but simply as the most convenient (*commodissimus*).[4]

Mr. H. E. Malden makes the obvious reply that Caesar did not, in point of fact, avoid the landing-places in question for the reason suggested by Professor Ridgeway; for 'he landed in the teeth of a British army'.[5] Moreover, as we have already seen, Caesar tells us that he sailed from the coast of the Morini 'because the shortest passage to Britain was from their country'.

3. The professor contends that his theory is supported by Caesar's account of his voyage in 54 B.C. Mr. Malden[6] told him that Caesar could not have sailed from Wissant [or, as he ought to have said, from Boulogne] to Pevensey with a south-west wind;[7] and that,

[1] *Journal of Philology*, xix, 1891, p. 205.

[2] According to Jean Brant of Antwerp, who published an edition of the *Commentaries* in 1606, some scholars affirmed that *XXXX* was to be found in 'good *MSS.*' (*C. I. Caesaris quae exstant*, ed. G. Jungermann, 1606, p. 501 of notes): but this vague statement, which C. Schneider (*Comm. de bellis C. I. Caesaris*, ii, p. 10) naturally discredits, is incapable of confirmation. The reading *XXXX* is not attested in any critical edition.

[3] See p. 558, *supra*.

[4] *Journal of Philology*, xix, 1891, pp. 205–6.

[5] *Ib.*, xx, 1892, p. 63.

[6] *Ib.*, xix, 1891, pp. 197–8.

[7] See *B. G.*, v, 8, § 2.

since the tide must have carried him in 54 B.C. at least as far as the South Foreland,[1] it would have been impossible for his men to row to Pevensey—a distance of fifty-five miles—between dawn and noon, that is to say in less than nine hours. The professor replied to the former objection that Caesar 'evidently sailed, not direct for Pevensey, but rather across Channel'.[2] The reply was as true as it was futile; but it was true only because Caesar was bound, not for Pevensey but for East Kent. Mr. Malden's second objection the professor endeavoured to rebut by the following arguments:— First, that as Caesar's men began to row at 3 a.m.,[3] continued rowing till noon, and had the tide in their favour for the first six hours, they could, if necessary, have rowed fifty-five miles in nine hours. Secondly, that fifty-five miles is an excessive estimate; and that the actual distance was not more than thirty-nine; for *accessum est ad Britanniam*[4] [the words by which Caesar describes the arrival of his fleet] 'seems to denote nothing more than what he expressed by the words *Britanniam attigit*[5] in the story of the former voyage. But,' continues the professor, 'he did not land at all at the place where he *Britanniam attigit*, but dropped down with the tide seven miles further. Moreover, Caesar does not say that he made for the very spot where he had landed before, but simply *remis contendit ut eam partem insulae caperet qua optimum esse egressum superiore aestate cognoverat*[6] ["rowed hard to gain the part of the island where, as he had learned in the preceding summer, it was best to land"]. The high cliffs formed his landmark.' The professor is presumably referring to the cliffs eight miles east of Pevensey, which, as Airy points out, are 'from ten to thirty feet high': these cliffs would evidently have made a most conspicuous 'landmark'. However, the professor contrives to reduce the length of the voyage by eight miles at one end: he curtails it at the other by simply denying, like Airy, that when Caesar 'saw Britain lying behind on the left', he had drifted past the South Foreland. He insists that Caesar 'might use the word *relictam* ['left behind'] when, instead of finding himself close to the shore of Britain, he discovered that, between the course he had sailed and the way he had drifted, he had moved away from Britain'.[7] This remark only shows that the professor did not know what was the direction of the flood tide. Unless Caesar had got past the South Foreland by the time when he 'saw Britain lying behind on the left', the tide had not carried him 'away from Britain'.

The professor's argument comes to this. He says that Caesar's

[1] See pp. 574–7, 616, *supra*. [2] *Journal of Philology*, xix, 1891, pp. 210–1.

[3] Caesar's men certainly did not begin to row at 3 a.m. Daybreak did not occur before 3.15; and, as Mr. Peskett remarks (ib., xx, 1892, p. 198), 'the starting on the right course with the turn of the tide of a large and probably somewhat scattered fleet is not a momentary act which you can assign to a particular minute of the day.'

[4] *B. G.*, v, 8, § 5. [5] *Ib.*, iv, 23, § 2.

[6] *Ib.*, v, 8, § 3.

[7] See *Journal of Philology*, xix, 1891, pp. 206–10.

men rowed as hard as they could; that they could have rowed fifty-five miles in nine hours, but that they only did row thirty-nine! He asks us to believe that they rowed fifty-five miles in nine hours, though, on his own showing, the tide was against them for one-third of that time![1] Finally, when he argues that because Caesar did not land in 55 B.C. at the point where he *Britanniam attigit*, therefore he did not land in 54 B.C. at the point where *accessum est ad Britanniam*, he forgets two things:—first, that Caesar distinctly says that in 55 B.C. he sailed on seven miles from the point where he first *Britanniam attigit*, whereas all commentators except Professor Ridgeway have drawn from Caesar's narrative the inevitable inference that in 54 B.C. he landed at the point where *accessum est ad Britanniam*; secondly, that the Britons expected him to land in 54 B.C. at the point where *accessum est ad Britanniam*, for 'large forces had assembled there' (*magnae manus eo convenissent*). Does the professor seriously mean to argue that if Caesar had landed elsewhere, he would not have said so?[2]

Not a single argument of the least weight has been or can be adduced to show that Caesar landed at Pevensey or anywhere on the coast of Sussex.[3] On the other hand, there is not a single objection which has here been brought against that theory which is not alone sufficient to overthrow it. The truth is that Airy, with all his scientific knowledge and controversial skill, was not adequately equipped to discuss the question: his classical scholarship left much to be desired; and, having once committed himself to the preposterous theory that the Portus Itius was in the estuary of the Somme, he was forced to look for Caesar's landing-place far to the west of that part of Britain in which Caesar's narrative inevitably places it. To that part of Britain our inquiry must henceforth be confined. Whether Caesar landed east or west of the cliffs of Dover, he landed in Kent.

[1] As a matter of fact it would have been against them much longer. See *Admiralty Tide Tables*, pp. 112, 115, 118, and S. H. Brown, *Diagrams and Tables*, &c., 1895.

[2] The professor denies that Caesar's men could have taken all the time from daybreak to noon to row with the tide from a point off the South Foreland to Romney Marsh; and, on the assumption that they landed on the eastern end of the marsh, he is unquestionably right. But there is no evidence that they began to row at daybreak (see p. 620, n. 3, *supra*): we are not obliged to assume that because *all* the ships, including stragglers, had reached Britain by about noon, rowing went on in all till twelve o'clock; and the professor would have done better to conclude, not that they rowed to Pevensey, but that they drifted as far as the latitude of Deal, and rowed to a point on the eastern coast of Kent.

[3] The theory that Caesar landed between St. Leonards and Bulverhythe, which was advocated by R. C. Hussey (*Archaeol. Cant.*, i, 1858, pp. 94–110), requires no comment; for the same arguments that are fatal to Airy's theory are fatal also to it.

VIII. THE THEORY THAT CAESAR LANDED AT LYMPNE OR HYTHE

The most dexterous advocate of the theory that Caesar landed on Romney Marsh was Thomas Lewin; and it says a great deal for his persuasiveness that not one of his critics appears to have detected the inconsistencies with which his work abounds. Those which vitiate his argument, in so far as it relates to the tides, I have mentioned already.[1] The rest all spring from one and the same source. When Lewin wrote his book, he adopted a theory as to the configuration of Romney Marsh which, after obtaining what he considered 'more accurate information',[2] he discarded. This information he embodied in an appendix to his second edition; but at the same time he allowed the statements based upon his former researches to stand. Thus on page 65, note 4, he implies that 'the heart of the marsh' was inhabited; but in his preface (pages v–vi) he affirms that 'the eastern end of the Marsh where Caesar arrived was as much *terra firma* in his day as in our own', virtually admitting, as the context and the map which faces page liii alike show, that the rest was inundated at every high tide. On page 92 he says that 'the sea, as is proved incontestably by the fragments of ships and anchors which have been dug up, flowed to the very base of the hill, and formed there the port of Limne. Stuttfall [castle], therefore, was formed for the protection of the shipping.' On pages lxviii and lxix the incontestable proof is not only contested but flung to the winds: 'the fragments of ships and anchors' are silently annihilated; Stutfall Castle, it now appears, 'was for the protection, not of the port, but of Saxonicum littus'; and the 'Portus Limanis' (*sic*) becomes a 'narrow gut', extending from a point near Shorncliffe to West Hythe behind a bank of shingle broken by a narrow entrance nearly opposite Hythe. In an article which Lewin contributed to the fortieth volume of *Archaeologia* he remarks that if the Portus Lemanis had been at the foot of Lympne Hill, 'we should expect to find some vestiges, however faint, of the port itself': but, he adds, 'I have never heard or read (though I have often inquired) that any remnant of a pier or sunken vessel, or even any anchor or other part of a ship's tackle, was ever discovered in this part.'[3] Are we to infer, then, Mr. Lewin, that when you told us on page 92 of your book that 'fragments of ships and anchors' had been 'dug up' at the foot of the hill, you were romancing? On turning back to page 42, we find that the above-mentioned 'narrow gut' first came into existence in the time of the Saxons; but on page lxix we learn with bewilderment that it 'must have continued such until the abandonment of Britain by the Romans or nearly so'. In the article which he contributed to *Archaeologia* Lewin changed his mind again. In the map which illustrates this article the 'narrow

[1] See p. 609, *supra*.
[2] *The Invasion of Britain*, &c., 1862, p. ciii.
[3] *Archaeologia*, xl, 1866, pp. 364–5.

gut' extends no further westward than Hythe. On page 44 of the book we read that Caesar landed on 'the western side' of 'the creek of Limne' or 'port of Limne', the very existence of which the author's later and 'more accurate information' led him to deny. On pages lxxii and lxxiv Caesar's landing-place is silently transferred from 'the western side' of 'the creek of Limne' to Hythe. On page 44 two islands mentioned (according to Lewin) by Valerius Maximus are identified with 'two islands' depicted on 'old maps' of 'the bay of Limne': on page lxxiii we are asked to identify them with two 'islands' in the 'narrow gut' above mentioned.

The reader now understands that, according to the theory of the ancient configuration of Romney Marsh which Lewin adopted in his Appendix and illustrated in the map facing page liii of his book, it would have been impossible for Caesar to land opposite Lympne, because on that theory the marsh between Lympne Hill and the shingle beach was flooded by the sea at high tide. Nevertheless, I shall consider the arguments by which Lewin defended his original view—that Caesar landed opposite Lympne—because distinguished scholars still hold that there was a harbour there in Roman times.

When we come to examine Lewin's final view—that Caesar landed at Hythe—we shall find some difficulty in doing justice to it; for he carefully avoids committing himself to any clear explanation of his meaning. If we look at his map[1] of the 'narrow gut', which he believed to have extended from West Hythe to a point opposite Shorncliffe, we shall see that, on his theory, Caesar must have done one of two things. Either he must have landed on the shingle west of the mouth of the gut, or he must have landed on the shingle east of that mouth; for Lewin clearly gives us to understand that the Roman ships did not sail into the harbour.[2] He maintains that on the day on which the first landing occurred a fierce struggle took place between the Romans and the Britons in 'the field south of Hythe'.[3] In order to reach this field, the Romans would have had to walk along the shingle either westward or eastward, and then along the northern shore of the 'narrow gut'. But Caesar distinctly states that as soon as the Romans stood on dry land, that is to say, on Lewin's own showing, on the shingle beach, they put the Britons to flight.[4] What becomes, then, of the imaginary combat in 'the field south of Hythe'? Furthermore, Lewin, while he is constrained to admit that this field is 'below high-water mark', assures us that it was 'certainly dry at low water'.[5] But he himself strenuously maintains that the Romans *began* to land three hours after low tide.[6] Perhaps he was uneasily conscious that he had contradicted himself when he suggested that the Britons 'would

[1] I have reproduced the relevant part of Lewin's map on the map facing p. 531.
[2] *The Invasion of Britain*, &c., 1862, p. lxxv.
[3] *Ib.*, pp. lxxiii–lxxiv.
[4] *B. G.*, iv, 26, § 5.
[5] *The Invasion of Britain*, &c., 1862, p. lxxiv.
[6] *Ib.*, p. xciii.

unquestionably have possessed the skill to embank the port and drain the land in the immediate neighbourhood '.[1]

The theory that Caesar landed at or near Hythe involves another mystery, which Lewin does not attempt to clear up. Where was the camp which, in 54 B.C., Caesar linked by 'a single defensive work' (*una munitione*[2]) with the ships which he found it necessary to haul up on dry land, and how was the defensive work constructed? Lewin tells us that the ships could not possibly have been drawn up opposite Shorncliffe, because the shore there 'is rocky and precipitous '.[3] Therefore, if the landing took place near Hythe, they must have been drawn up on the beach west of Shorncliffe, and on the seaward side of the 'narrow gut',—as he suggests, opposite Hythe. If so, of what was the 'defensive work' composed? Surely not of shingle? The entrenchments which a child constructs with his toy spade at Margate would have been just as effective. But if not of shingle, what other material was available on a shingle beach? And what was the direction of the 'defensive work' which connected the ships with the camp? Presumably the camp was on dry land behind the 'narrow gut', and constructed not of shingle but of earth. Now the entrenchment which protected the ships and connected them with this camp could hardly have been carried across the 'narrow gut', which was deeply submerged at every high tide! I can only suppose, then, that the connecting work was really constructed, by some occult process, of shingle; traced out along the shingle beach either eastward towards Shorncliffe, or westward to West Hythe Oaks, and then along the northern shore of the 'narrow gut' until it joined the camp. Or if, as Lewin suggests, the camp with which the ships were connected was distinct from the camp which Caesar marked out on an 'advantageous position' (*loco idoneo*[4]) immediately after his second landing; and if, as he affirms,[5] it 'must have stood upon the seashore', and its site 'must long since have disappeared'; then it can only be concluded that camp and connecting work were both constructed of shingle!

Again, it is incredible that a gale which drove some of Caesar's eighteen cavalry transports past Hythe or Lympne to a more westerly part of the island should have carried the rest back to the port from which they had started, which Lewin rightly identified with Ambleteuse. If the ships had been approaching Hythe when the storm arose, they would have been obliged, in order to return to Ambleteuse, to steer SE. by E. 9° S.[6] Now Lewin himself maintains that

[1] *The Invasion of Britain*, &c., 1862, p. lxxiv.
[2] *B. G.*, v, 11, § 5.
[3] *The Invasion of Britain*, &c., 1862, p. 87.
[4] *B. G.*, v, 9, § 1.
[5] *The Invasion of Britain*, &c., 1862, p. cxxiii. If the reader will consult Lewin's map in *Archaeologia*, xl, 1866, p. 369, or my reproduction of it, he will see that even if Lewin's final view of the topography of Hythe harbour could be accepted (see pp. 547–8, *supra*), the absurdities involved in his theory of Caesar's disembarkation would still remain.
[6] I mean of course that this would have been their true course.

the gale blew from the north-east.[1] As a matter of fact, if it had blown from this quarter at Lympne or Hythe, it would have been practically innocuous; for off either of those places vessels would have been sheltered from it by the hills, and the transports which were driven westward would not have been, as they were, swept by the waves: if the gale had blown from the north-east off Walmer, its direction off Hythe would have been about east-north-east.[2] Still, for the sake of argument, I will accept Lewin's view. According to it, the ships would have been forced to sail within less than eight points of the wind. But in a heavy gale a ship will make as much as six points and a half of lee-way.[3] Let us reduce this estimate to four, which is certainly a liberal reduction.[4] It follows that the hapless transports would have been required, in order to reach their destination, to lie within less than four points of the wind, a feat which, I need hardly say, would have been utterly impossible.[5] Furthermore, Lewin affirms, in an unguarded moment, that the gale, when a few hours later it wrecked Caesar's ships as they rode at anchor, was still blowing from the north-east: but he does not explain how a north-easterly gale could have driven ashore, that is to say, driven in a northerly direction, ships anchored off Lympne or Hythe!

Such are a few of the absurdities in which Lewin's theory plunges him. However, he shall be heard in his own defence.

The argument upon which Lewin lays the most stress has been already refuted.[6] He maintains that at the time when Caesar, on the day of his first voyage, quitted his anchorage and sailed with the stream to the spot where he was to land, the stream must have been running down the Channel.[7] Let us assume, in order to allow more than its due weight to his argument, that Caesar must have weighed anchor as early as 3.30 p.m.,[8] though I shall afterwards prove that the assumption is both unnecessary and false; and let us also suppose that the tide did not actually turn eastward until after 5 p.m. Even then, unless Caesar could have calculated the exact time which it had still to run with more than the certainty of the experts who prepare the *Admiralty Tide Tables*, he would have had to face the risk that, before he could reach his landing-place, it might turn against him. Even if Heller[9] is not right in interpreting the words 'getting wind and tide simultaneously in his favour' (*et ventum et aestum uno tempore nactus secundum*) as meaning that Caesar weighed anchor just after the stream had turned, there can be no doubt that this would have been the wisest course to pursue.

[1] *The Invasion of Britain*, &c., 1862, pp. 59–60.
[2] Information supplied by Commander Boxer, R.N., Harbour-Master at Folkestone.
[3] See Falconer's *Marine Dictionary*, 1815, p. 220.
[4] The harbour-master of Dover, who fully endorses my argument, thinks that four points would be a fair estimate.
[5] See p. 613, *supra*.
[6] See pp. 606–11, *supra*.
[7] *Archaeologia*, xxxix, 1863, pp. 301–1.
[8] See p. 611, *supra*.
[9] *Philologus*, xxii, 1865, p. 307.

1. Having disposed, to his own satisfaction, of the question of the tides, Lewin remarks that 'Hythe would be much nearer than Deal, and was the natural port for vessels from Boulogne'.[1]

Yes, nearer by a bare two miles, and a few yards nearer than Walmer! But, as Lewin himself insists,[2] Caesar, when he sailed from Boulogne, did not intend to land either at Hythe or at Deal; and unless he intended to sail direct to one of those places, Lewin's argument collapses. Moreover, since the greater part of the voyage was made upon the easterly-going stream, it would have taken longer to sail to Hythe than to the coast between Walmer and Deal. And when Lewin affirms that Hythe 'was the natural port for vessels from Boulogne', he apparently forgets that he has already told us that 'Folkestone ... would be the natural port for Boulogne',[3] that Caesar's fleet never entered Hythe harbour, and that 'the mooring of the Roman vessels within it would be certain destruction'.[4]

2. Lewin attempts to show that the wind which carried Caesar from Boulogne to his anchorage on his first voyage, and which would, if it had continued, have been in his favour if he had intended to sail on towards Deal, veered round before he quitted his anchorage.[5] His argument runs as follows:—'Caesar says that he started from his anchorage ... having *got* the wind in his favour, and the Latin word *nactus* implies that the wind had undergone a change ... When he embarked at Boulogne he despatched the cavalry to Ambleteuse ... with orders to follow him with all haste; but ... they did not leave that haven for Britain until the fourth day after,[6] and no plausible reason can be given for this except that, for the whole of this interval, the wind was contrary; that is ... had shifted.' In a later passage[7] he maintains that if the wind had not shifted during the voyage, the length of the passage, and especially the tardy arrival of the transports, would be inexplicable. Moreover, he says, the word *nactus* 'implies a change either in the *wind* or the tide: the tide had not changed, and therefore the change alluded to must have been in the wind'.[8] While, however, he argues that the wind must have shifted, he endeavours to secure his retreat by affirming that, if it had not shifted, Caesar could nevertheless have sailed to Hythe as easily as to Deal. 'Supposing,' he says,[9] 'the

[1] *The Invasion of Britain*, &c., 1862, p. lxxxix.
[2] *Ib.*, p. 31
[3] *Ib.*, p. xlviii.
[4] See p. 632, *infra*.
[5] *The Invasion of Britain*, &c., 1862, pp. 39–40.
[6] This statement, as Caesar's narrative (*B. G.*, iv, 26, § 5) shows, is incorrect; and Lewin himself corrects it when he says (p. xlvii) that on the day of Caesar's first voyage 'the eighteen transports ... set sail, according to orders, but had been forced to put back by stress of weather'.
[7] *The Invasion of Britain*, &c., 1862, pp. xlvi–xlvii.
[8] In regard to Caesar's use of the word *nanciscor*, see H. Meusel, *Lex. Caes.*, ii, 688–9. Long (*Decline of the Roman Republic*, iv, 434), commenting on the inference which Lewin draws from the word *nactus*, says that 'this is not a certain conclusion', and quotes *B. G.*, v, 9, § 4 (*repulsi ab equitatu se in silvas abdiderunt, locum nacti egregie et natura et opere munitum, quem domestici belli, ut videbatur, causa iam ante praeparaverant*). I doubt whether this passage is relevant.
[9] *The Invasion of Britain*, &c., 1862, pp. 39–40.

wind to have blown from the south, it would have been favourable to a movement, from a point opposite Dover, either to the east or west.'

I freely admit—indeed I have myself maintained—that the wind had shifted during the voyage. It had shifted to a point unfavourable, or comparatively unfavourable, to ships sailing from North-Eastern Gaul to Britain. I also, like Lewin, maintain that it shifted again before Caesar quitted his anchorage. The fact is obvious. But what then? How can Lewin prove that before Caesar quitted his anchorage the wind did not shift to a quarter which would have been favourable to a run from a point off the cliffs of Dover towards Deal? Moreover, it is not true that if the wind had 'blown from the south, it would have been favourable to a movement, from a point opposite Dover, either to the east or west'. If the wind had blown exactly from the south, it would certainly not have been called favourable to a movement from a point opposite Dover to Hythe, that is to say, within less than seven points of the wind; and if the wind had blown from any point west of south, the word 'favourable' would have been still less appropriate.[1]

3. Lewin maintains that the wisest course which Caesar could have pursued when he sailed on from his anchorage in 55 B.C. would have been to steer westward. 'Let us first consider,' he says,[2] '*a priori*, what a prudent commander might be expected to do under similar circumstances . . . To the right he would see the precipitous chalk cliffs stretching away . . . till they terminated at the South Foreland . . . The lowlands about Walmer and Deal would not be visible; and it is at least doubtful whether Volusenus had included them in his survey . . . tracing the line of cliffs westward, he could not fail to see that they terminated at Sandgate, and that a broad level plain there succeeded.' At Hythe, he adds, there was a landing-place 'distinctly visible from his moorings'.[3]

This argument rests upon the absolutely groundless assumption that Volusenus had not reconnoitred the coast on the north of the South Foreland;[4] in other words, that Volusenus, whom Caesar specially selected as a thoroughly competent man, had grossly neglected his duty and disobeyed his instructions. Besides, even if the landing-place at Hythe had been 'distinctly visible' from Caesar's moorings,[5] nothing would have been gained; for Caesar acted, not upon what he could see, but, as he tells us himself,[6] upon what he had learned from the report of Volusenus. Whether he could or could not see 'from his moorings' the place where he was to land, he knew the direction in which he intended to sail. Finally,

[1] Lewin afterwards saw that if Caesar landed at Hythe, he could not have anchored off Dover, and accordingly transferred his anchorage to a point off Folkestone. See p. 635, *infra*.

[2] *The Invasion of Britain*, &c., 1862, pp. 33–4. [3] *Ib.*, p. xc.

[4] I am glad to find that Heller (*Zeitschrift für allgemeine Erdkunde*, xviii, 1865, p. 111) has anticipated my argument.

[5] Obviously it would have been invisible if Caesar had anchored off Dover. See n. 1, *supra*. [6] *B. G.*, iv, 23, § 5.

when Lewin considers '*a priori*' that 'a prudent commander might be expected under similar circumstances' to land at Hythe or Lympne, he only advertises his own ignorance of a commander's business. As I shall show presently,[1] no commander who was not hopelessly incompetent would have dreamed, in the circumstances of ancient warfare, of attempting to land either at Hythe or at Lympne.

4. Lewin contends in his text that at Lympne and in his appendix that at Hythe there was a landing-place which exactly corresponded with Caesar's description; and he denies that any such landing-place exists at Deal. He affirms[2] that the shore on the western side of 'the creek of Lympne' was '*apertum* or open, for the heights to the north were at least a mile distant. The sea-beach was also *molle* or soft . . . in a sailor's sense, *i.e.* it consisted of shingle, than which nothing can be more favourable to the security of vessels . . . Sand, on the contrary, is, in naval phraseology, of the hardest kind, as it has no "give", and a ship beating against it would soon be dashed to pieces.' He says that, according to Lucan,[3] Plutarch,[4] and Dion Cassius,[5] there was 'marshy ground' at the place where Caesar landed, and he assures us that marshes were formed by the streams which entered the port of Hythe.[6] He remarks that, according to Valerius Maximus,[7] there were two small islands near the landing-place, which were the scene of an exploit performed by one of Caesar's centurions: 'on looking,' he says, 'at the old maps of this part of the coast, I find . . . that the bay of Limne contained . . . two islands.' Like other people who had learned to measure the trustworthiness of Valerius Maximus, Lewin had himself been sceptical about the anecdote of the centurion; but how could he resist the testimony of the 'old maps'? He admits that he was converted:—'the circumstance, so apocryphal before, becomes thus no inconsiderable argument for placing the descent in this locality.'[8] Afterwards he changes his mind, and transfers the islands some distance to the east of Lympne. They actually existed, he tells us, a few years before the publication of his book, 'either near to or in the ancient port of Hythe'; they also are depicted in various old maps; and they were 'carted away' by the late James Elliott, the engineer of Romney Marsh.[9] Furthermore, he assures us[10] that the incident described by Valerius Maximus is also noticed by Plutarch,[11] who 'lays the scene in sight of Caesar himself, and therefore close to the camp; and in a marsh or swamp, which, with the light afforded by the account of Valerius, must be taken to mean a lagoon subject to the alternations of the high and low tides'.

Now as to the exact meaning of the word *apertum* in the passage

[1] p. 629, *infra*.
[2] *The Invasion of Britain*, &c., 1862, p. 44.
[3] *Pharsalia*, ii, 571–2.
[4] *Caesar*, 16.
[5] *Hist. Rom.*, xxxix, 51, § 2.
[6] *The Invasion of Britain*, &c., 1862, p. cxxi.
[7] iii, 2, § 23.
[8] *The Invasion of Britain*, &c., 1862, pp. 43–4.
[9] *Ib.*, Preface (p. vi), p. lxxiii; *Archaeologia*, xxxix, 1863, p. 313.
[10] *The Invasion of Britain*, &c., 1862, pp. 64–5.
[11] *Caesar*, 16.

to which Lewin refers, the commentators are not agreed. While he insists that the beach on which he believes Caesar to have landed was 'open', because 'the heights to the north were at least a mile distant', Dr. Guest[1] denies that they were 'open', because 'there is a range of heights at no great distance'. According to Long,[2] '"open" means that from the beach he could see into the country.' Now any one who has read the *Commentaries* attentively will see that all these explanations are wrong. For in his narrative of the second expedition[3] Caesar tells us that he 'felt little anxiety for the ships, as he was leaving them at anchor on a nice open shore'[4] (*eo minus veritus navibus, quod in litore molli atque aperto deligatas ad ancoras relinquebat*). If the explanations which I have quoted were correct, the word *apertum* in this passage would be irrelevant. Whether the 'heights' were 'at no great distance', or 'from the beach Caesar could see into the country', the security of the ships would not have been affected in the slightest degree. The word *apertum* does not describe the country near the shore: it describes the shore itself; and, as C. Schneider[5] says, *apertum litus* means a shore free from such obstacles or dangers as rocks, boulders, and the like.[6] But, even assuming that Lewin was right in his interpretation of the word *apertum*, this much is certain:—the existence of the heights to which he refers is alone sufficient to prove that Caesar did not land either at Lympne or at Hythe or at any point between those two places. This is a matter on which I confidently appeal to any military expert who has studied the records of ancient warfare. Caesar might no doubt have *landed* at Hythe without any extraordinary difficulty; but if he had been so foolish as to land there, he would have found that his difficulties were only beginning. Never, even when fighting against an uncivilized enemy, did he attempt to force his way up a hill if it was possible to avoid doing so.[7] If he had landed either at Lympne or at Hythe, he could not have turned the line of heights which extends behind those two places: he could not have penetrated into the interior of the country unless he had passed them; and he could not have passed them except at a cost of life which the least experienced of his officers would have been too prudent to incur. Furthermore, however 'open' the country may have been on the western side of 'the [imaginary] creek of Lympne', he could not have landed there if, as Lewin admits,[8] the country was under water at high tide.

[1] *Archaeol. Journal*, xxi, 1864, p. 239.
[2] *Decline of the Roman Republic*, iv, 166.
[3] *B. G.*, v, 9, § 1.
[4] For the meaning of *molli* see p. 630, *infra*.
[5] *Comm. de bellis C. I. Caesaris*, ii, 45–6.
[6] On the meaning of *apertus*, as used by Caesar, cf. H. Meusel, *Lex. Caes.*, i, 283–4. In *B. G.*, i, 41, § 4, *loca aperta* means a country free from woods and other features which would have made marching difficult: in *B. G.*, ii, 18, § 2, and vii, 18, § 3, *collis apertus* means a hill free from woods.
[7] See *B. G.*, ii, 10, § 4; 23, § 2; 27, § 5; v, 32, § 2; 49, § 6; 51, § 1; vi, 8, §§ 1, 3; vii, 45, § 9; 49, § 1; 52, § 2; 53, § 1; 83, § 2; 85, § 4; and numerous passages in the *Civil War* (cf. H. Meusel, *Lex. Caes.*, ii, 170–2, s. v. *iniquitas*, *iniquus*).
[8] See pp. 546, 622, *supra*.

The argument which Lewin bases upon the word *mollis* has no value; for he does not fully understand the meaning of the word. *Mollis*, in the passage which we are considering, simply means that the shore was one where the anchorage was good, and where the ships, if they were driven aground, would suffer comparatively little:[1] it probably also implies that the shore was gently sloping.[2] Moreover, even if the word *mollis* implied that the shore on which Caesar landed was composed of shingle, Lewin would not be justified in concluding that Caesar landed at Hythe or Lympne unless he could prove that no other shingle beach in Kent satisfied the requirements of Caesar's narrative.

The statements of Lucan, Valerius Maximus, Plutarch, and Dion Cassius, upon which Lewin lays so much stress, are not really evidence at all; and if he had been a classical scholar, he would never have quoted them. Nor, indeed, was his scholarship sufficient to enable him to understand what they meant. The passage quoted from Lucan occurs in a rhetorical speech which he puts into the mouth of Pompey:—

Rheni gelidis quod fugit ab undis
Oceanumque vocans incerti stagna profundi
Territa quaesitis ostendit terga Britannis.

Stagna of course simply denotes the English Channel, about which Lucan's ideas were vague; and the point of the line is the suggestion that Caesar, in order to magnify the difficulties of his expedition, mendaciously described 'the pools of a shifting sea' as an ocean.[3] As to Valerius Maximus, the idea of going to his collection of anecdotes for the details of Caesar's military operations is really funny;[4] but if he is to be counted an authority, he does not support Lewin's theory, but overthrows it.[5]

[1] See p. 655, n. 3, *infra*.

[2] Schneider maintains (*Comm. de bellis C. I. Caesaris*, ii, 45–6) that 'molle idem esse quod leniter acclive, imprimis apto exemplo demonstravit Heldius, 7, 46, *ad molliendum clivum* non aliter dictum docens'. I do not think that Schneider is right in arguing that *mollis* should be *translated* by 'gently sloping', though that meaning is doubtless implied. My friend, Professor Postgate, who agrees with me, has kindly referred me to a passage in Ovid (*Ep. ex Ponto*, i, 2, 61–2)—

Cum subit Augusti quae sit clementia, credo
Mollia naufragiis litora posse dari—

which seems to justify my explanation. Professor Postgate has also written me a most interesting letter, in which he remarks that while *aperto* describes the approach to the shore, which was not blocked by rocks, *molli* connotes both a gentle slope and a soft surface: he refers to a passage in Pomponius Mela (i, 19, § 102), where the Black Sea is described as *non molli neque harenoso circumdatus litore*.

[3] See *Pharsalia*, ed. C. E. Haskins, 1887, p. 67, note to line 571, and cf. Ovid, *Fasti*, iv, 278. The word *incerti* apparently refers to the tides.

[4] See C. Kempf's edition of Valerius Maximus, 1854, pp. 26–33, and *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, p. 96.

[5] The statements of Plutarch, Dion Cassius, and Valerius Maximus, to which Lewin refers, are as follows:—Ἐν δὲ Βρεττανίᾳ τῶν πολεμίων εἰς τύπον ἐλώδη καὶ μεστὸν ὑδάτων ἐμπεσοῦσι τοῖς πρώτοις ταξιάρχοις ἐπιθεμένων στρατιώτης, Καίσαρος αὐτοῦ τὴν μάχην ἐφορῶντος, ὡσάμενος εἰς μέσους καὶ πολλὰ καὶ περίοπτα τόλμης ἀποδειξάμενος ἔργα, τοὺς μὲν ταξιάρχους ἔσωσε, τῶν βαρβάρων φυγόντων, αὐτὸς δὲ

Lewin[1] insists that 'the *vada* described by Valerius Maximus as caused by the flux and reflux of the tide are evidently the *vada* referred to by Caesar (iv, 26)'. Now 'the *vada* referred to by Caesar' were the shallow places of the shore on which he landed; and 'the *vada* described by Valerius Maximus' were, according to Lewin's final view, in Hythe harbour. Yet Lewin emphatically denies that Caesar landed in Hythe harbour! Furthermore, Lewin assures us that the 'islands' of which Valerius Maximus speaks were composed of earth, and have been 'carted away'. But Valerius Maximus says nothing about islands: the only island which he mentions is Britain. As to the marshes, Heller[2] points out that Lewin is wrong in concluding from the narratives of Plutarch and Dion that the coast on which Caesar landed was marshy. Dion's word τενάγη is simply his translation of Caesar's *vada*, and means not 'marshes' but 'shallows'.[3] Plutarch, as the words εἰς τόπον ἑλώδη καὶ μεστὸν ὑδάτων ('into a marshy place full of water'), which are explained by the following words ῥεύματα τελματώδη ('marshy [or muddy] streams'), clearly show, was not speaking of a combat on the seashore, but of one which took place inland; and when Lewin identifies Plutarch's ῥεύματα τελματώδη with Dion's τενάγη, he simply exposes his own ignorance of Greek. Moreover, Plutarch is so deficient in accuracy and precision that his description of an incident of this kind is useless for the purpose of topographical identification.

There is, therefore, no evidence that there was any marshy ground

χαλεπῶς ἐπὶ πᾶσι διαβαίνων ἔρριψεν ἑαυτὸν εἰς ῥεύματα τελματώδη καὶ μύλις ἄνευ τοῦ θυρεοῦ, τὰ μὲν νηχόμενος, τὰ δὲ βαδίζων, διεπέρασε. Plutarch, *Caesar*, 16. κάνταῦθα [*i.e.* at the landing-place in 55 B.C.] τοὺς προσμίξαντάς οἱ ἐς τὰ τενάγη ἀποβαίνοντι νικήσας ἔφθη τῆς γῆς κρατήσας, &c. Dion Cassius, xxxix, 51, § 2. Bello quo C. Caesar . . . Britannicae insulae caelestis iniecit manus, cum quattuor commilitonibus rate transvectus in scopulum vicinae insulae, quam hostium ingentes copiae obtinebant, postquam aestus regressu suo spatium, quo scopulus et insula dividebantur, in vadum transitu facile redegit, ingenti multitudine barbarorum affluente, ceteris rate ad litus regressis solus immobilem stationis gradum retinens, undique ruentibus telis et ab omni parte acri studio ad te invadendum nitentibus, quinque militum diurno proelio suffectura pila, una dextera hostium corporibus adegisti. Ad ultimum destricto gladio audacissimum quemque modo umbonis impulsu, modo mucronis ictu depellens hinc Romanis, illinc Britannicis oculis incredibili, nisi cernereris, spectaculo fuisti. Postquam deinde ira ac pudor cuncta conari fessos coegit, tragula femur traiectus saxique pondere ora contusus, galea iam ictibus discussa et scuto crebris foraminibus absumpto, profundo te credidisti ac duabus loricis onustus inter undas, quas hostili cruore infeceras, enatasti, visoque imperatore armis non amissis, sed bene impensis, cum laudem merereris veniam petiisti quod sine scuto rediisses &c. *Factorum et dictorum memorabilium*, iii, 2, § 23.

[1] *The Invasion of Britain*, &c., 1862, p. 65, n. 1.

[2] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, pp. 96–7.

[3] Cf. Herodotus, viii, 129, § 1,—Ἀρταβάζῳ δὲ ἐπειδὴ πολιορκέοντι ἐγεγόνεσαν τρεῖς μῆνες, γίνεται ἄμπωτις τῆς θαλάσσης μεγάλη . . . ἰδόντες δὲ οἱ βάρβαροι τέναγος γενόμενοι &c. ('while Artabazos was besieging the town, there came to be a great ebb of the sea backwards . . . and the barbarians, seeing that shallow water had been produced', &c. [G. C. Macaulay, *The Hist. of Herodotus*, ii, 1890, p. 285]). See also Strabo, iii, 5, § 11,—ὁ ναύκληρος ἑκὼν εἰς τέναγος ἐξέβαλε τὴν ναῦν.

at or near Caesar's landing-place; and if there was, the fact does not prove that he landed at Hythe.[1]

5. Lewin[2] next proceeds to examine Caesar's narrative of the events which followed his first disembarkation,—the hauling up of the galleys on dry land, the wreck caused by the storm which occurred on the night of the full moon, and the subsequent attack by the British charioteers. The whole account, he tells us, corresponds with the topography of Lympne and Hythe, but of no other place. The shingle field between Dymchurch Wall and Shorncliffe, being 'sound and dry, without any mixture of ooze', deserves Caesar's epithet, *aridum*:[3] the 'steep place' (*declivis ac praeceps locus*[4]), down which the British chariots charged, is 'in this part of the marsh on the north'; the spring tide, driven by 'a strong south-east wind', would have poured over the shingle, and waterlogged Caesar's galleys. 'It is a singular confirmation,' says Lewin, 'of our hypothesis of the debarcation at Romney Marsh that the range of high water is greater here than at any other point of the southern coast. At Dungeness, for example, the mean range is twenty-one feet three quarters, while at Deal it is only sixteen feet.' Finally, he anticipates the objection that if Caesar had landed near Hythe, he would have moored his ships within Hythe harbour, and thereby avoided the destruction which overtook so many of them. He explains that 'the narrow and winding gut which constituted the port' was 'little capable of receiving a fleet', and that, 'as it ... could only be entered and quitted at high water, and as its banks were lined by a hostile population, the mooring of the Roman vessels within it would be certain destruction,' but 'it would be highly useful for keeping up his communication with the Continent'.

That Caesar's ships could have been hauled up on the shingle between Dymchurch Wall and Shorncliffe; that the said shingle might have been described as '*aridum*'; that a 'steep place' existed 'in this part of the marsh on the north';[5] and that the tide, driven by 'a strong south-easterly wind', might have waterlogged Caesar's ships if they had been drawn up on the beach at Hythe,—all these things may be admitted: but they avail nothing to establish Lewin's theory unless he can prove that on the east coast of Kent there was no place which answered equally well to Caesar's narrative; and it is amusing to find that in another passage[6] he rightly insists that the wind which he here calls 'south-easterly' was 'from the north-east'. The comparison which he makes between 'the range of high water' at Romney Marsh and at Deal is irrelevant; for I doubt whether he would have seriously maintained that a spring

[1] See p. 654, *infra*.
[2] *The Invasion of Britain*, &c., 1862, pp. 60, lxx-lxxi, lxxiii, lxxv, xc, cxxi.
[3] *B. G.*, iv, 24, § 3.
[4] *Ib.*, 33, § 3.
[5] It should be noted by the way that Caesar's remark about 'steep places' (*B. G.*, iv, 33, § 3) is purely general, and does not necessarily refer to any combat which took place between his troops and the Britons.
[6] *The Invasion of Britain*, &c., 1862, pp. 59–60.

tide between Walmer and Deal, heightened by a storm, would have been insufficient to cause the damage which Caesar described.

The objection which Lewin anticipates, and waves aside, is not thereby disposed of. If the reader will examine Lewin's map, he will see that Hythe harbour would have been amply large enough to accommodate Caesar's fleet;[1] and if it could only be entered at high water, the Portus Itius, according to Lewin himself,[2] was in the same case. That Caesar would have landed close to a harbour which he afterwards found 'highly useful', without mentioning its existence, is, to say the least, unlikely. Moreover, Lewin omits to tell us in what respect Caesar could have found a harbour 'highly useful' even for 'keeping up his communication with the Continent', if the dispatch vessels which kept up the communication never entered the harbour; nor does he explain what would have been the use of their entering the harbour when, on his own showing, they could not have remained there without 'certain destruction', but after they had entered it, must have forthwith gone out again, and have been hauled up on the beach outside along with the others.

6. It remained for Lewin to point out the spot on the banks of a river about twelve miles from the Roman camp, where the Britons disputed Caesar's advance on the day after his second landing in Britain. He maintained that Wye on the Stour, between Ashford and Canterbury, answered in every detail to Caesar's account, and denied that any such place could be found on the theory that Caesar landed near Deal.[3]

Airy[4] objects that the Stour at Wye is 'little wider than a wide ditch', and that, as it flows 'between sound meadows, where there is not, and never has been, any marsh or broad stream', it never could have been wider. Lewin[5] replies that he has himself seen the Stour at Wye when it was so full of water that 'the mill had constantly at work four pairs of stones from 5.30 a.m. till 8 p.m., except for a short time at noon'; and I may add that it is more than ten yards wide.[6] Caesar does not describe the stream at all: he merely calls it a *flumen*; and he calls the Oze and the Ozerain, the two streams which encompass Alesia[7] (the modern Mont Auxois), and which are narrower than the Stour at Wye, by the same name. Moreover, as Lewin[8] justly remarks, Caesar, in describing the combat which took place on the banks of the stream in question, and the subsequent attack upon the British stronghold,[9] 'does *not* make the river the important part of the defence.' The Stour at Wye and the features of the surrounding country correspond sufficiently well with Caesar's account; but this does not constitute a positive

[1] According to the map facing p. liii of Lewin's book, Hythe haven was about 3 miles long, and in many places more than a quarter of a mile broad.

[2] *The Invasion of Britain*, &c., 1862, p. 27. [3] *Ib.*, pp. 87–8, 90.

[4] *Essays on the Invasion of Britain*, &c., p. 36.

[5] *The Invasion of Britain*, &c., 1862, p. xxix.

[6] *Ordnance Survey* (25 inches to one mile), Sheet LV, 11; personal observation.

[7] *B. G.*, vii, 69, § 1.

[8] *The Invasion of Britain*, &c., 1862, p. xxx. [9] *B. G.*, v, 9, §§ 2–6.

argument in favour of Lewin's theory unless it can be proved that, on the theory that Caesar landed in East Kent, it is impossible to discover a stream which satisfies the requirements of his narrative.

D'Anville, the famous French geographer, added little or nothing to the case for Hythe. He maintained that the 'higher ground' to which the Britons withdrew on descrying the Roman flotilla in 54 B.C. must have been the line of heights which extends just behind Hythe.[1] But the passage in which Caesar describes the retirement of the Britons, so far from implying that the 'higher ground' was close to the landing-place, implies the very reverse; for Caesar, immediately after telling us that the Britons had retired to higher ground, expressly states that the position on which he found them posted was about twelve miles from his camp near the sea.[2]

Lewin has now had a fair hearing; and those who are interested in the question will decide whether he has made out his case. Over and above what has been said in refutation of his arguments there remain other facts which make it absolutely certain that Caesar did not land either at Hythe or at Lympne.

1. Caesar, as we have seen, tells us that when he sailed from the Portus Itius for Britain in 54 B.C. the wind was from the south-west.[3] Now a south-west wind would not have been favourable to a voyage from Boulogne to Hythe, much less to Lympne. The wind would have been abeam even if it had been possible to keep the ships heading in the direction of Hythe, that is to say, even if they had made no lee-way: the ships, being shallow and flat-bottomed,[4] would have made considerable lee-way; and two or three hours after the voyage began the current turned east-north-east, and continued to run in that direction until daybreak[5]. It is curious that so adroit a controversialist as Lewin should have inadvertently quoted an opinion which damaged his own case. He tells us[6] that a Mr. John Dougall, in an unpublished tract, remarked that Caesar would have called the south-west wind favourable if he had sailed from Boulogne for the South Foreland. But, on Lewin's theory, Caesar sailed, not for the South Foreland but for Hythe; and Lewin naïvely tells us that, in Dougall's opinion, Caesar would never have called the wind favourable if it had been on the beam. Certainly he would not have done so when the tide was setting in the wrong direction.

2. Another objection to the theory that Caesar landed at Hythe or Lympne is that it involves the assumption that when he started on his first voyage, he steered for the unsuitable port of Folkestone, or else that, although he was in possession of Volusenus's report, he did not know where he intended to land, and steered at haphazard. According to Lewin's original idea,[7] he first anchored off the cliffs of

[1] *Mém. de litt. tirés des registres de l'Acad. Roy. des Inscr. et Belles-Lettres*, xxviii, 1761, p. 408.
[2] *B. G.*, v, 9, §§ 1–2.
[3] *Ib.*, v, 8, § 2.
[4] *Ib.*, v, 1, § 2.
[5] See p. 576, n. 1, *supra*.
[6] *The Invasion of Britain*, &c., 1862, p. xcviii. Cf. *Philologus*, xxii, 1865, pp. 305–6.
[7] *The Invasion of Britain*, &c., 1862, pp. 33, 39–40, xlviii.

Dover. This is the view which naturally commends itself to every unbiassed reader of the *Commentaries*; but, as Lewin afterwards saw, it is irreconcilable with the theory that Caesar sailed from his anchorage either to Hythe or to Lympne; for Caesar tells us that he sailed 7 Roman miles, and Hythe is almost 11, Lympne about 14, Roman miles from the nearest point of the Dover cliffs.[1] Lewin thus found himself obliged to relinquish his original view, and to maintain that Caesar anchored off Folkestone,[2] or, as he suggests in another passage, and implies in his map,[3] off East Wear Bay. But second thoughts are not always best. Lewin insists that Folkestone 'would be the natural port for Boulogne'.[4] Why? Surely not because the steamboats of the South Eastern and Chatham Railway Company discharge their passengers there! 'The natural port for Boulogne' would be the port to which, having regard to the prevailing winds and the currents, it would be easiest to sail, and which would best accommodate Caesar's fleet. That port was Dover. Caesar's narrative shows that he required a capacious harbour;[5] and the port of Folkestone was too small.

3. Another consideration—one of several which, strange to say, have hitherto been overlooked—is alone fatal to the theory that Caesar landed either at Hythe or at Lympne. He tells us, in regard to his first landing, that his troops were not able to pursue the enemy far 'because the cavalry had not been able to keep their course and make the island'.[6] These words unmistakably imply that if he had had his cavalry with him, he would have been able to make good use of them; and in fact he says expressly that, in repelling the attack which the Britons made upon his camp just before his departure from Britain in 55 B.C., he actually did employ Commius's small troop of cavalry.[7] The camp, so Lewin assures us when he is advocating the cause of Lympne, was 'in the marsh'.[8] How, then, would Caesar have been able to utilize his cavalry? Would he have sent them up the steep slopes on which stand the ruins of Stutfall Castle? Even on the incredible hypothesis that Romney Marsh had been embanked by the Britons[9] the cavalry would have been useless; for how could they have acted in a country intersected by sluices?[10] It is possible, indeed, that if Caesar had landed at Hythe, his cavalry might have acted in 'the field to the south and east of Hythe', in which Lewin finally assumes that the combat took place on the day of the landing,—if the Britons had patiently waited to receive their charge, and if the field was not under water.[11] But before the cavalry could have come into action the Britons would have been on the hills behind.

[1] *Ordnance Survey* (one inch), Sheets 289, 306.
[2] *The Invasion of Britain*, &c., 1862, p. xlviii. [3] *Ib.*, p. 52.
[4] *Ib.*, p. xlviii. [5] *B. G.*, iv, 20, § 4; 21, § 1. [6] *Ib.*, 26, § 5.
[7] *Ib.*, 35, § 1. [8] *The Invasion of Britain*, &c., 1862, p. 62.
[9] See pp. 549–52, *supra*.
[10] Lewin himself remarks (*The Invasion of Britain*, &c., 1862, pp. lxiv–lxv) that when the marsh was enclosed, it 'was at the same time intersected by sluices'. [11] See p. 623, *supra*.

I do not deny that the cavalry could have managed to trot up Lympne Hill or the hills behind Hythe, if Caesar had been so foolish as to give the order.[1] But the point is that the absence of the cavalry prevented Caesar from pursuing the fugitives *far*; and that the hills would have effectually concealed from him the nature of the country that lay beyond them, its woods, defiles, and other obstacles. Does he not tell us himself that when he had gained a victory on the day after his second landing in Britain, 'he forbade [his troops] to pursue the fugitives far, partly *because he had no knowledge of the ground*' (*eos fugientes longius Caesar prosequi vetuit, et quod loci naturam ignorabat*,[2] &c.)? Worse ground for the manœuvring of cavalry than the wooded heights which extended behind Hythe and West Hythe it would have been hard for Volusenus to find.

4. Heller[3] has acutely seen that Caesar's account of the movements of one division of the ships which carried his cavalry is irreconcilable with the theory that he landed at any point on the south coast of Britain. Caesar[4] tells us that the storm which arose when these vessels were sighted from his camp drove some of them 'to the lower and more westerly part of the island' (*aliae ad inferiorem partem insulae, quae est propius solis occasum, . . . deicerentur*). 'This,' says Heller, 'can only mean a different side of the island from that on which Caesar was: if he had meant to designate a point on the same side, he would have said, *paulo infra ac propius solis occasum*.[5] If one compares the expression which he actually uses in the thirteenth chapter of his Fifth Book—*unum latus est contra Galliam. Huius lateris alter angulus, qui est ad Cantium, quo fere omnes ex Gallia naves appelluntur, ad orientem solem, inferior ad meridiem spectat*[6]—it becomes clear that in the passage in which the fate of the eighteen ships is described *inferior pars insulae* means the southern side of the island. Consequently Caesar implies that his camp was on the eastern angle.'

5. One of the episodes which Caesar describes in his narrative of the first expedition is, in spite of the ingenuity with which Lewin has tried to make it fit in with his theory, irreconcilable with the view that he landed either at Hythe or Lympne. One day, when the 7th legion had gone out to cut corn, Caesar learned from the troops on guard in front of the camp that an extraordinary quantity of dust was visible in the direction in which the legion had gone. He marched to the rescue, and, after he had advanced some little

[1] Lympne Hill rises 288 feet in 1823, and between Hythe and Seabrook the hill rises 282·2 feet in 1723. See *Ordnance Survey of England* (6 inches to 1 mile), Sheet LXXIV, SW. and SE. The angles are 9° 5′ 25″ and 9° 25′ 7″ respectively.

[2] *B. G.*, v, 9, § 8. Cf. ii, 11, §§ 2–3.

[3] *Philologus*, xxii, 1865, pp. 305–6.

[4] *B. G.*, iv, 28, § 2.

[5] 'A little lower down and more towards the west.'

[6] . . . 'one side is opposite Gaul. One corner of this side, by Kent—the point which almost all ships from Gaul make for—has an easterly, and the lower one a southerly outlook.'

distance (*paulo longius*), he found that the legion was hard pressed by the enemy. The place where the legionaries had been reaping was the only one in which the corn had not yet been cut; and the enemy, anticipating that they would come there, posted themselves in ambush in a wood close by.[1] Lewin[2] argues that the camp must have been in Romney Marsh, 'probably on the seaside'; but he stultifies his own argument by admitting, in his appendix, that the marsh was inundated at every high tide.[3] In his text he admits that neither the cornfield nor the wood could have been in the marsh; and he could not at first conceive how, if they had been anywhere else, the dust raised by the combatants could have been seen from the ground in front of the camp. But, he continues, 'when I visited Hythe by land, and walked from it to the old port of Limne, and mounted the hill, I discovered the explanation. On reaching the top I stepped at once into a cornfield . . . and on my right was Park Wood . . . What I had taken from the sea for a hill . . . had no descent on the north side . . . and corn growing so near to the edge that even the reapers, if labouring in that part of the field, might have been seen from the camp. The whole narrative was now realized to the mind's eye . . . The legion had marched up to the standing fields of corn on the high ground, and the Britons, starting from their lurking-place at the side, had intercepted their retreat, and surrounded them at just such an interval from the edge that the combatants were out of sight and hearing, but the dust flying in the air had attracted the attention of the guard . . . at a mile's distance below.'

Now observe what becomes of Lewin's explanation. First, 'the old port of Limne,' to which he walked, became, before the publication of his second edition, a figment of the imagination! Having obtained, as he tells us, 'more accurate information,' he strenuously denied its existence, and accordingly transferred Caesar's landing-place to Hythe.[4] Yet in this same second edition the 'explanation' is offered as confidently as ever! Secondly, he asks us to believe that the only cornfield which the Romans had left unreaped was the one nearest them! Thus the 'explanation' which Lewin discovered with such pride collapses; and his theory, which cannot stand without it, falls like a house of cards. What explanation he would have discovered on the theory that Caesar landed at Hythe, he wisely omits to say.

[1] *B. G.*, iv, 32.
[2] *The Invasion of Britain*, &c., 1862, pp. 56, 62–4.
[3] See pp. 546, 622, *supra*.
[4] See pp. 622–3, *supra*.

IX. THE THEORY THAT CAESAR LANDED AT HURST

The latest supporters of the theory that Caesar landed on Romney Marsh are Mr. H. E. Malden and, tentatively, Mr. Warde Fowler[1] and Professor Pelham.[2] Mr. Malden relies upon the argument, drawn from the study of the tides, which I have already refuted.[3] But on certain points of detail he differs from Lewin. He maintains that Lewin's description of Romney Marsh, as it existed in the time of Caesar, is incorrect, and that 'the coast-line then ran nearly east and west from Sandgate towards Appledore'. In other words, he maintains that the hills which bound Romney Marsh on the north were accessible 'by ships sailing over what is now embanked land'. Accordingly he believes that Caesar landed neither at Hythe nor at Lympne, but 'on a broad flat muddy shore' near Hurst, that is to say, about three miles west of Lympne. This, he assures us, 'was a landing-place second to none'; so presumably the mud was a recommendation. Mr. Malden notes that there was 'good camping-ground, wood and water on the slope above'; but a few lines lower down he observes that 'there would be good camping-ground on the slope where Stutfall castle now stands'; that is to say, three miles off on the east! He assures us, further, that 'the place agrees singularly with the account of the battle on the shore'; that 'the passage into the inner country would be easy by the break in the hill above West Hythe'; and that 'the hill is anywhere accessible'. Finally, he remarks that 'in A.D. 893 Hastings the pirate came here with his fleet'.[4]

Now Lewin, as I have already shown,[5] gives three successive and different descriptions of Romney Marsh as it existed in the time of Caesar; and Mr. Malden does not say to which of the three he refers. It is evident, however, that he is thinking of the first; and I am afraid that he did not take the trouble to read the second edition of Lewin's book. In a previous article[6] I have examined all Lewin's theories. I will here assume that Mr. Malden is right; that the coast-line 'ran nearly east and west from Sandgate towards Appledore'; and that the northern fringe of Romney Marsh was accessible 'by ships sailing over what is now embanked land'. On this hypothesis and on any other it is absolutely certain that Caesar could not have landed at the point which Mr. Malden indicates. He does not know what 'good camping-ground', in the circumstances of ancient warfare, was. If he knew the *Commentaries* as intimately as a man who professes to explain them should do, he would see that neither at Hurst, nor 'on the slope where Stutfall castle now stands', nor at any intermediate point did good camping-ground exist; for everywhere along this line a camp would have

[1] *Julius Caesar*, 1892, p. 196.
[2] *Outlines of Roman History*, 3rd ed., 1900, p. 257, n. 2.
[3] See pp. 605–11, *supra*.
[4] *Journal of Philology*, xvii, 1888, pp. 167–78.
[5] See pp. 622–4, *supra*.
[6] See pp. 535–7, 545–9, *supra*.

been dominated by higher ground above. Does Mr. Malden not remember the words in which Caesar describes the camping-ground which Reginus and Rebilus were compelled by adverse circumstances to occupy at Alesia? They 'were obliged', he says, 'to make the camp on a gentle slope, which gave an assailant a slight advantage' (*necessario paene iniquo loco et leniter declivi castra fecerunt*[1]). The slope between Hurst and Stutfall Castle would have given an assailant not a slight but a considerable advantage. Where, then, could Caesar have found the 'advantageous position' (*loco idoneo*[2]) which he selected for his camp in 54 B.C.? 'Hastings the pirate,' as I have already proved,[3] did *not* 'come here with his fleet'; and what good Caesar's cavalry, the absence of which he deplored, could have done by pounding up the hill, which was overgrown by woods,[4] Mr. Malden does not explain. The hill, as he says, is 'everywhere accessible',—to a pedestrian: but, as I have already shown,[5] 'the passage into the inner country,' *in the face of armed resistance*, could only have been effected with heavy loss; and it would be interesting to hear Mr. Malden account for the fact that the Britons retired in 54 B.C. to 'higher ground' twelve miles from the landing-place, when, just above the landing-place which he calls 'second to none', there was ground 312 feet above the level of the sea, and 300 feet above the present elevated level of the marsh.[6] I should also like to hear him explain why Volusenus, a trained soldier who thoroughly understood his business, should have advised Caesar to land at Hurst rather than between Walmer and Deal; what motive could have induced Caesar to attempt to sail from Wissant (which he identifies with the Portus Itius) to Hurst with a south-west wind, that is to say, to attempt to sail west-north-west, on an easterly-going stream, within six points of the wind; how his cavalry transports contrived to sail back from a point near Hurst to Sangatte, or even to Ambleteuse, against a gale which drove their sister ships towards the west; or by what miracle this gale dashed his ships ashore in a northerly direction!

X. THE THEORY THAT CAESAR LANDED BETWEEN HURST AND KENNARDINGTON

The late Francis Hobson Appach published in 1868 a book called *Caius Julius Caesar's British Expeditions*, the principal object of which was to prove that Caesar's troops disembarked in 55 B.C. between 'the foot of the spur from Aldington Knoll' (which is about four miles west of Lympne) on the right, and a point 'about half way between Bonnington and Bilsington' on the left;[7] and

[1] *B. G.*, vii, 83, § 2. Cf. 85, § 4,—*exiguum* [v.l. *iniquum*] *loci ad declivitatem fastigium magnum habet momentum* ('a slight downward inclination of the ground has a great effect'). [2] *Ib.*, v, 9, § 1.

[3] See p. 539, n. 7, *supra*. [4] See p. 640, n. 4, *infra*. [5] See p. 629, *supra*.

[6] *Six-inch Ordnance Survey*,—Kent, Sheet LXXIII, SE.

[7] *C. J. Caesar's Brit. Expeditions*, pp. 72–3, § 6.

that in the following year the left of the line extended as far westward as Kennardington. This theory differs but slightly in locality from Mr. Malden's, but is defended by different arguments; and, moreover, from Bonnington westward the slope of the hills which bound Romney Marsh on the north becomes much gentler. Appach maintains that at least the northern portion, if not the whole, of the marsh was, in Caesar's time, submerged, and that its northern fringe was accessible to ships sailing from the east. His arguments have already been refuted in an earlier section of this book;[1] but for the purpose of the present inquiry I will assume that he made out his case, and that, in Caesar's time, 'the Bay of Apuldore,' to which he constantly alludes, was, as he assures us, deep enough to float 'the heaviest of Caesar's ships at the lowest spring tides'.[2]

Appach, of course, like Lewin and Airy, bases his theory upon the hypothesis that the tidal stream which Caesar had in his favour when he sailed from his anchorage to his landing-place must have set towards the west. Although I have proved that this hypothesis is untenable, I will assume, in order to do justice to Appach's arguments, that Caesar may have sailed with the westward stream.

1. Appach begins by arguing that Volusenus, when he was reconnoitring the British coast, found on the west of Aldington Knoll 'the very ground of which he was in search', namely, 'a low level coast', which 'extended as far as Apuldore . . . forming the head of a bay protected from the waves in the Channel by the cliff at Fairlight . . . and the cliff at Folkestone'.[3]

It may be freely admitted that this coast, if it was accessible by sea, would have been a convenient landing-place, if Caesar had only desired to land, and then to sail away again. But his object was to invade Britain; and the landing-place which Appach believes Volusenus to have selected would have led Caesar into the inhospitable forest of Anderida,[4] where no corn was to be procured. This objection, indeed, is equally fatal to Mr. Malden's theory; and neither he nor Appach attempts to explain how there could have been 'buildings far and wide' (*omnibus longe lateque aedificiis incensis*) on those lonely slopes, on which, even now, woods are abundant, and buildings very few and very far between.[5]

2. Appach tells us that Caesar's original intention was to disembark between Sandgate and Hythe; and it must be borne in

[1] See pp. 543–5, 551–2, *supra*.

[2] *C. J. Caesar's Brit. Expeditions*, p. 138, § 1

[3] *Ib.*, p. 49, §§ 4–5.

[4] See W. Topley, *Geology of the Weald*, pp. 402–3, and R. Furley, *Hist. of the Weald of Kent*, i, 12, and map facing p. 26. The strip of country extending two or three miles northward from Hurst to Kennardington is still thickly covered by woods: no less than eleven are named on the One-Inch Ordnance Map (Sheet 305).

[5] Furley (*ib.*, p. 13, n. *) has noted this objection. Lewin would perhaps have argued that the buildings were in Romney Marsh, as he finally concluded that the marsh had perhaps been enclosed by the Britons in pre-Roman times; but the absurdity of this theory has been already demonstrated. See pp. 549–52, *supra*.

mind that, according to him, Hythe harbour, as described by Lewin, did not then exist. 'This landing place,' he says, 'though open to the south and east, was sheltered on the north and west. It was also sheltered from the force of the sea and stream in the Channel, and must therefore have been the principal British port on this part of the coast in the time of Caesar.'[1]

Perhaps (if it had existed),—'on this part of the coast'. But then Caesar[2] says that 'almost all ships from Gaul' used to make for what he calls the eastern 'corner' of Britain, 'by Kent.' He may have been thinking of Dover, or of Richborough, perhaps of both; but I doubt whether it is possible to force into his words the meaning that the harbour where 'almost all ships from Gaul' discharged their freight was between Sandgate and Hythe.

3. But let us assume that Caesar intended, for some inscrutable reason, to land between Sandgate and Hythe. What, then, were the 'precipitous heights' off which he anchored,—the heights from which it was possible to throw a missile right on to the shore? Can Appach find them? Yes! He tells us that they are there, and that if we look for them anywhere else, we shall look in vain.[3] He bids us note carefully the passage in which Caesar describes them:—'there, standing in full view on all the heights, he saw an armed force of the enemy. The formation of the ground was peculiar, the sea being so closely walled in by abrupt heights that it was possible to throw a missile from the ground above on to the shore' (*in omnibus collibus expositas hostium copias armatas conspexit. Cuius loci haec erat natura, atque ita montibus angustis mare continebatur, uti ex locis superioribus in litus telum adigi posset*[4]). You are to observe, says Appach, that 'Caesar uses the word "all" . . . thus pointedly implying, if not . . . asserting that there were more than two hills'. We are then told that, 'it being about half an hour after high water, the sea reached quite up to the mouths of the three valleys between Hythe and Sandgate, so that the bay of Hythe appeared to Caesar to be shut in by the hills exactly as he describes it.'

Now these words alone are evidence that Appach did not know enough Latin to qualify him for the task which he had undertaken. That he should have misconstrued the words *montibus angustis mare continebatur* is not so surprising; but to a man who is familiar

[1] *C. J. Caesar's Brit. Expeditions*, pp. 48, 65, § 14. Appach (*op. cit.*, pp. 56, § 5, 71, §§ 3–4) assumed that Caesar in 55 B.C. steered for Hythe, intending to land there if the Britons were friendly, and otherwise to sail either to Deal or Bonnington; that he was 'of course completely ignorant of the turn of the stream in the Channel'; that while he was at anchor he gave orders for a landing at Deal; but that when the stream turned westward he changed his mind and issued new orders for a landing at Bonnington! Appach failed to see that since Caesar, when he was at anchor, saw how the stream was running, Volusenus could have done the same. To say that Caesar was 'completely ignorant' is to assume that Volusenus was a fool. Besides, did not Caesar's Gallic seamen know the Channel by heart?

[2] *B. G.*, v, 13, § 1.

[3] *C. J. Caesar's Brit. Expeditions*, pp. 69–70, § 1.

[4] *B. G.*, iv, 23, § 3.

with Caesar's *usus loquendi*, the conclusion which Appach draws from the words *omnibus collibus*—that Caesar must have seen at least three *separate hills*—will appear somewhat forced.[1] If he had engaged a boat at Dover, and looked at the cliffs stretching away eastward towards the South Foreland, he would have seen that they answered exactly to Caesar's description. The upper edge of the chalk is not a straight line : it rises and falls in a succession of deep curves, corresponding with the rolling downs above, which are, so to speak, divided into a series of heights by well-defined depressions. Here are the *omnes colles*, as plain as can be, not three of them but six,[2] between the Priory Valley and the South Foreland. And Appach wants us to believe that the hills between Sandgate and Hythe are 'precipitous heights' (*angusti montes*), from the summits of which it would have been possible to throw a missile right on to the shore. Precipitous heights! Go, reader, and look at them.[3]

4. Appach candidly admits that Caesar, on his imaginary voyage from Hythe to Bonnington, lost off Lympne the benefit of the westward current, and 'met the eddy in the bay of Apuldore running from west to east along its northern shore' :[4] but by this candour he gives away his whole case ; for who will believe that Caesar would have told us that he 'got the stream in his favour' if the stream had turned against him before he had completed half his journey?

5. Again, Appach observes that Caesar's account of his first landing implies that the Britons who opposed him used their chariots as well as their cavalry ; and he scoffs at the notion that the chariot-horses or even the cavalry could have acted on shingle. Therefore, he concludes, 'there could have been no shingle at the place where Caesar landed. Bonnington fulfils this condition.'[5]

Quite so ; and the coast north of Sandown Castle also practically fulfils, or once fulfilled, this condition.[6] If Appach's argument shows that Caesar did not land on shingle, it does not show that he landed at Bonnington. Moreover, granting that Caesar's narrative implies that chariots were used by the Britons in opposing the landing, Appach fails to realize the situation. The horses were not required to gallop into the sea ; nor could they have galloped through the waves at Bonnington any more than at Lympne, or Hythe, or Deal : but at any of those places they could have seriously obstructed

[1] In *B. G.*, vii, 36, § 2, Caesar describes the various elevated points of the mountain mass of Gergovia by the words *omnibus eius iugi collibus*, though they could not be called separate hills ; and Long (*Decline of the Roman Republic*, iv, 438, note), referring to *B. G.*, iii, 18, § 8, remarks that '"fossae" often signifies every part of a "fossa" which surrounds a camp'.

[2] Viewed from the sea about half a mile off the Foreland, there are eight. See p. 736, *infra*.

[3] The steepest rises 282·2 feet in 1723, forming an angle of 9° 25′ 7″ (p. 636, n. 1, *supra*).

[4] *C. J. Caesar's Brit. Expeditions*, pp. 77–8, § 6.

[5] *Ib.*, pp. 75, note a, 78–9, § 7.

[6] See G. Dowker in *Archaeol. Journal*, xxxiii, 1876, p. 58, and his *Coast Erosion*, p. 3.

men who could hardly keep their footing, and who were encumbered by their armour in the way which Caesar[1] so graphically describes. All that the horses had to do was to convey their masters 'a little way into the water' (*paulum in aquam* [progressi]), so as to enable them to throw their missiles with effect; and this they could easily have done if they had drawn their light cars no faster than the ancient steeds which drag, or used to drag, bathing-machines over the shingle at Eastbourne.

6. Finally, Appach cites the well-known passage in which Caesar tells us that at daybreak on his second voyage he 'descried Britain lying behind on the port quarter' (*sub sinistra Britanniam relictam conspexit*). He argues that Caesar's words would be meaningless 'if the configuration of the coast had been the same in his day as it is at the present time, for Britain could not . . . have been in any other position. The expression, however, is peculiarly appropriate if the sea then filled the Bay of Apuldore; for Caesar, sailing, as he thought, from Boulogne to Kennardington, of course expected to see Britain on his right.'[2]

Yes, *we* of course see that, as Caesar was drifting up channel, 'Britain could not have been in any other position' than on his left. But Caesar was not writing for us: he was writing for his countrymen, who did not know anything about the configuration of Britain until they had read as far as the thirteenth chapter of his Fifth Book.[3] Moreover, if Appach had known his Polybius, he would have remembered the passage in which readers who had passed their lives in the Mediterranean basin were informed of the self-evident fact that Hannibal, marching eastward through Southern Gaul, had the Mediterranean on his right.[4] Furthermore, if Caesar had steered for Kennardington, he would have had to drift more than fifteen nautical miles in order to reach a point opposite the South Foreland: it would have been impossible to drift nearly so far between 'about midnight' and daybreak;[5] and unless he had drifted further, he could not have seen Britain 'lying behind' on his left.[6]

I need hardly add that Appach does not explain how those cavalry transports got back, in spite of the north-easterly or east-north-easterly gale, from off Bonnington to Ambleteuse.

Enough has been said to show that, even if 'the Bay of Apuldore',

[1] *B. G.*, iv, 24, § 2. [2] *C. J. Caesar's Brit. Expeditions*, p. 102, § 5.

[3] I am glad to find that Heller (*Philologus*, xxii, 1865, pp. 309–10) has anticipated my argument. If, he remarks, the Romans had already known where Caesar's landing-place was, the expression *sub sinistra*, coupled with *Britanniam relictam*, might have been superfluous; but it was precisely from these words that they first learned whereabouts to look for it.

[4] Ἀννίβας δὲ παραδόξως, τοὺς μὲν χρήμασι πείσας τῶν Κελτῶν, τοὺς δὲ βιασάμενος, ἦκε μετὰ τῶν δυνάμεων, δεξιὸν ἔχων τὸ Σαρδόνιον πέλαγος, ἐπὶ τὴν τοῦ Ῥοδανοῦ διάβασιν. Polybius, iii, 41, § 7. Even the best modern historians, in trying to bring a scene vividly before the imagination, sometimes mention geographical facts which are known to everybody.

[5] See p. 656, *infra*. [6] See p. 616, *supra*.

as Appach describes it, existed in Caesar's time, he never sailed its waters. We now know where to look for his landing-place: he must have first set foot in Britain on the eastern coast of Kent.

XI. THE THEORY THAT CAESAR LANDED BETWEEN WALMER AND DEAL

The oldest English writers whose works have come down to us believed that Caesar had landed on the north of the South Foreland. This was certainly the view of Nennius, or of an author whose work Nennius edited;[1] and Dr. Guest attached great weight to his testimony.[2] I think that, in doing so, he showed less than his usual judgement; and perhaps he was not aware that Maistre Wace, who lived in the twelfth century, had anticipated the modern theory that Caesar landed on Romney Marsh.[3] For some centuries, however, the prevailing view, first definitely stated, if I am not mistaken, by Leland,[4] who lived in the reign of Henry the Eighth, was that the disembarkation took place near Deal.[5]

1. We know that Caesar, before he set sail from Gaul, intended to land, if possible, in one of the harbours of Britain; for he in-

[1] See Th. Mommsen, *Chronica minora*, iii, 1898, p. 114, and *La Grande Encyclopédie*, xxiv, 927.

[2] Iulius Caesar . . . venit ad Brittanniam cum sexaginta ciulis, et tenuit in ostium Tamesis, in quo naufragium perpessae sunt naves illius dum ipse pugnabat apud Dolobellum, qui erat proconsul regi Brittannico &c. (*Chronica minora*, ed. Th. Mommsen, iii, 1898, p. 162). Besides *apud* there is another reading, *contra*; and one *MS.* has *Dorobellum* instead of *Dolobellum*. Geoffrey of Monmouth, in his fabulous account of Caesar's first invasion (*Hist. Brittonum*, iv, 3), says that 'Cassibellaunus' came *ad Dorobellum oppidum*. It is not surprising that an uneducated writer like S. Pritchard (*Hist. of Deal*, pp. 1, 10, 39) should assure his readers that Caesar called his landing-place *Dola*; but when a scholar like Dr. Guest (*Archaeol. Journal*, xxi, 1864, p. 242) gravely points to 'the use of the phrase "apud dolo"' in the Vatican *MS.* of Nennius, and argues that *dolo* means Deal, I am amazed. The reading *dolo* is not so much as mentioned in Mommsen's *apparatus criticus*; but let us provisionally accept it, and then consider how Dr. Guest would have construed the passage:—*Iulius Caesar . . . pugnabat apud Dolo, qui erat proconsul regi Brittannico* &c. *Dolo*, says Dr. Guest, means Deal; *Dolo*, says Nennius, was *proconsul*, or commander-in-chief, under the British king. Whatever Nennius may have written, it is clear that he believed Caesar to have landed somewhere on the north of the South Foreland, and probably on the coast of East Kent; for, as Battely pointed out (*Antiquitates Rutupinae*, 1711, p. 46), the mouth of the Thames, in which Nennius places the landing, had a wider signification in the Middle Ages than it has now; and William of Malmesbury (*De gestis Pontificum Anglorum*, ed. N. E. S. A. Hamilton, 1870, lib. ii, § 73 [p. 140]) actually made it extend as far as Dover.

[3] *Le Roman de Brut*, ed. Le Roux de Lincy, 1836, vv. 4651–3.

[4] *Itin.*, vii, 1744, p. 127 (116–7). See also Camden's *Britannia*, ed. R. Gough, i, 218.

[5] According to Geoffrey of Monmouth (*Hist. Brittonum*, iv, 3, 9) and Matthew Paris (*Chronica Majora*, ed. H. R. Luard, pp. 72–4), who seems to have copied Geoffrey's amusing fable, Caesar landed *in ostium Tamensis fluminis* on the occasion of his first expedition, and, when he invaded Britain for the third time (!), in the harbour of Richborough (*in Rutupi portu*).

structed Volusenus to ascertain what harbours were capable of accommodating a numerous fleet, and, before he set sail, a deputation came to him from numerous British tribes to promise submission.[1] The choice, as we have seen, lies between the harbours of Dover and Folkestone; and it is not credible that Caesar should have deliberately preferred the small harbour of Folkestone to the more spacious and more easily accessible harbour of Dover.[2] And if he steered for Dover harbour, the 'precipitous heights' off which he anchored in 55 B.C. must have been, not the cliffs of East Wear Bay, but the cliffs between Dover and the South Foreland.[3]

Airy,[4] indeed, denies that Caesar ever intended to land in a harbour. 'This,' he says, 'is not the manner of attempting debarkation in a country possessed by an enemy. . . . Sir Arthur Wellesley made no attempt at Lisbon, but put his troops on shore at the Mondego Beach.' I wonder whether Airy knew the circumstances and the motives which determined Wellesley's action: at all events his argument will not mislead any reader who has studied the history of the Peninsular War. Why did Wellesley make 'no attempt at Lisbon'? Simply because, as Napier[5] says, 'the strength of the French, the bar of the river, the disposition of the forts, the difficulty of landing in the immediate neighbourhood, where a heavy surf broke in all the undefended creeks and bays, convinced him such an enterprise was unadvisable if not impracticable. . . . It was difficult to find a place to land. The coast, from the Minho to the Tagus, save at a few points, is rugged and dangerous; all the river harbours have bars and are difficult of access even for boats. . . . Seventy miles northward of the Lisbon Rock, the small peninsula of Peniché offered the only safe and accessible bay adapted for a disembarkation; but the anchorage was within range of the fort, which contained a hundred guns and a garrison of a thousand men. The next best place was the Mondego river; there the little fort of Figueras, now occupied by English marines, secured a free entrance, and Sir Arthur adopted it.' The reader will have noticed that Airy not only ignores the circumstances which forbade Wellesley to attempt a landing at Lisbon, but also ignores the existence of the Mondego river.[6] Caesar put his troops

[1] *B. G.*, iv, 20, § 4; 21, §§ 1–2, 5–8.

[2] See p. 635, *supra*, and F. H. Appach, *C. J. Caesar's Brit. Expeditions*, p. 47, §§ 4, 6. Appach observes that at Dover 'there was a very fine harbour', but that it was 'probably dangerous to enter in bad weather'. But the fact remains that it was continually entered; and when Caesar sailed to Britain the weather was good.

[3] It is possible that the reason why Caesar approached Britain a little eastward of Dover harbour was that he intended to run into the harbour on the ebb, or westerly-going stream. Long (*Decline of the Roman Republic*, iv, 438), like Appach, fatuously remarks that Caesar 'was ignorant of the turn of the stream in the Channel'. See p. 641, n. 1, *supra*.

[4] *Essays on the Invasion of Britain*, p. 29.

[5] *Hist. of the War in the Peninsula*, i, 1851, pp. 120–1.

[6] See E. B. Hamley, *Operations of War*, 1878, p. 221, and C. Oman, *Hist. of the Peninsular War*, i, 1902, pp. 228–9.

ashore on a beach; but he had intended to put them on shore in the best available harbour, because, unlike Wellesley, he had had reason to hope that his landing would not be opposed. If he did not intend to land in a harbour, why did he instruct Volusenus to report upon the Kentish harbours? And why did he steer towards 'precipitous heights', at the foot of which the dullest soldier in his army knew that it would be madness to land, if he did not intend to enter the harbour formed by the gap in those cliffs? Why did the enemy occupy those heights unless they believed that this was his intention? It might, indeed, be argued that after he received Volusenus's report, he decided that Dover harbour was unsuitable, and therefore abandoned all idea of landing in a harbour. If so, he would not have steered towards the Dover cliffs; but neither would he have steered towards those of Folkestone or East Wear Bay. The only alternative would be to assume that he anchored off the lower cliffs north of St. Margaret's Bay in order to wait for the overdue ships, having intended from the outset of his voyage to sail on and land north of Walmer.[1] But when he says that 'he thought the place [off which he anchored] most unsuitable for landing' (*hunc ad egrediendum nequaquam idoneum locum arbitratus*),[2] he unmistakably implies that he had contemplated the possibility of landing there; and, as he could have decided with his eyes shut that it would be absurd to land at the foot of 'precipitous heights', he must have concluded that it would be unwise to attempt to land, in the face of an enemy, in the harbour which was formed by the gap in the cliffs. Probably, before he knew that his landing would be opposed, he intended to observe the harbour with his own eyes and decide upon its merits himself.

Mr. H. E. Malden, however, remarks that, according to Dion Cassius, 'Caesar made the land on the first occasion where he ought not, *οὐ μέντοι καὶ ᾗ ἔδει προσέσχεν*.' 'Surely,' Mr. Malden continues, 'this implies not that Caesar aimed at a certain point for a landing place and then abandoned it upon a nearer view, but that something like what befell him on the second voyage happened on the first also, and that he drifted out of his course to a point which he did not intend to reach. If so, this disposes of all idea of his aiming at Dover as the usual port of landing.'[3]

Yes,—'if so'. According to Mr. Malden,[4] Caesar landed near Hurst in Romney Marsh. On this theory, he would have anchored off the coast near Sandgate, which was very little out of his course if he intended to land near Hurst, and not at all out of his course if, as Mr. Malden wrongly supposes, he sailed from Wissant. It is universally admitted, and it is certain that when Caesar was approaching Britain, and for some hours previously, the tidal stream was

[1] This appears to have been Kiepert's view (see his large wall-map of Gaul). But even if Caesar had anchored off Kingsdown, he would have first reached Britain (*attigit Britanniam*) off the South Foreland.

[2] *B. G.*, iv, 23, § 4.

[3] *Journal of Philology*, xvii, 1888, p. 174.

[4] See pp. 638–9, *supra*.

running up the Channel. Will Mr. Malden explain what that current could have been which would have caused Caesar's ships, while steering either from Wissant or from Boulogne for Hurst, to 'drift' towards Sandgate *on the eastward stream*?

2. The reader is already familiar with the subject of the tidal streams, in so far as it relates to the present discussion.[1] As we have seen,[2] Caesar may have landed in Britain on the 26th of August, 55 B.C.; and on that day high water should have occurred at Dover at 6.21 a.m. I say 'should have occurred', because observations have shown that high tide sometimes occurs a few minutes earlier or later than the time predicted in the Admiralty *Tide Tables*.[3] Therefore, *accepting the general rule laid down by Admiral Beechey and Surveyor Calver*,[4] the stream off the Dover cliffs would have turned to the west some time between 10.21 and 11.9 a.m., and again to the east at 5.24 p.m.

But Caesar, we are assured by Airy and Lewin, weighed anchor at 3 p.m.; and at 3 p.m. the stream would still have been running towards the west. The argument has imposed upon weak minds: the reader will see that it has not the slightest force. There is no evidence that Caesar weighed anchor at 3 p.m. What he says is simply that he awaited at anchor the arrival of the rest of the ships —the ships which had failed to keep pace with the leading division of the fleet—until the ninth hour. The ninth hour in the latitude of Dover lasted on the 26th of August from 2.20 till 3.30 p.m.[5] Therefore Caesar, assuming that his statement was literally correct, may have waited at anchor for the arrival of the laggard ships until 3.30 p.m. Now, as the reader will remember, I have demonstrated, from the evidence to which Airy appeals—the evidence supplied by Admiral Beechey and Surveyor Calver—that on the day of Caesar's landing the tidal stream may have turned eastward earlier than 3.54 p.m.;[6] and if it turned twenty-five minutes earlier, it turned in the ninth hour.

[1] See pp. 605–11, *supra*. Heller (*Zeitschrift für allgemeine Erdkunde*, xviii, 1865, pp. 116–25), after an elaborate argument, which, if his premisses are correct, is unanswerable, arrives at the conclusion that, assuming high water to have occurred at Dover on the 27th of August, 55 B.C., at 7.31 a.m., the stream off Dover turned between 4.26 and 5.21 p.m.; and that, as the turn must have been accelerated by the favourable wind which Caesar mentions (*B. G.*, iv, 23, § 6), 'one may say, without fear of error, that the stream turned at 4.26 p.m.': but it is unnecessary to examine his argument, because he was not acquainted with the results of the observations which, as we have seen, were made in 1862 by Surveyor Calver.

Neither have I taken any notice of the argument by which the late Professor Cardwell (*Archaeol. Cant.*, iii, 1860, pp. 14–7) endeavoured to prove that if high tide had occurred at Dover on the day of Caesar's landing at 7.31 a.m., he must at 3 p.m. 'have gone up Channel on the first of the flood and proceeded to the eastward'; for the evidence upon which the professor relied has been shown by Airy (*Archaeologia*, xxxix, 1863, pp. 304–6) to have been misleading.

[2] See pp. 600–3, *supra*.

[3] *Archaeologia*, xxxix, 1863, p. 290.

[4] See p. 608, *supra*.

[5] Napoleon III, *Hist. de Jules César*, ii, 553. In the latitude of Paris the ninth hour lasted on the 26th of August till 3.27 p.m.

[6] See pp. 610–1, *supra*.

Hitherto we have assumed that Caesar's statements of the hour up to which he awaited the arrival of his overdue ships was literally correct. But now let me beg any one who still feels a doubt whether his narrative agrees with the hypothesis that he sailed eastward from his anchorage, to use his common sense and to remember that he has a sense of humour. Airy, Lewin, Mr. Malden, and the rest argue in this strain :—Caesar waited at anchor till 3 [or 3.30] p.m., and not a minute later: the stream was then running westward; therefore Caesar sailed towards the west.—*Q.E.D.* This is the sort of argument that might have been expected, not from an Astronomer Royal, or from a barrister like Lewin who knew the world, but from a clever schoolboy. Yet not a single commentator has ever pointed out its absurdity. Had Airy forgotten the discrepant statements that were made by officers who had watches in their pockets as to the hour at which this or that episode occurred in the campaign of Waterloo ?[1] Did Airy or Lewin imagine that Caesar had a Dent's chronometer on board his galley, and, the moment after he weighed anchor, noted down in his diary the words, *Hora nona ancorae sublatae sunt ?* It is possible that he may have had a water-clock (*clepsydra*)[2] on board, which would have enabled him, if it had been duly corrected for the latitude of Dover, and if the sea had been so smooth that the ship was motionless, to tell the time approximately; but surely it is probable that he roughly estimated the time from his observation of the altitude of the sun ?[3] And is it not equally probable that he trusted not to a diary but to his memory ? His estimate may have been right: but also it may have been wrong; and anyhow it is folly to stake the whole argument upon its accuracy.

Nor, again, is it even certain that Caesar did weigh anchor at the time which he called *horam nonam*. As he says that he waited for the arrival of his overdue ships till the ninth hour, it may be presumed that they did not arrive earlier. When they arrived, their captains had, I suppose, to receive instructions, as the generals and military tribunes had done already. As Heller[4] and de Saulcy

[1] See H. Houssaye, *Waterloo*, 38th ed., 1902, pp. 195, n. 4, 275, n. 2, 277. n. 2, 313, n. 1, 366, n. 1, 413, n. 1. These discrepancies arose, I presume, from lapses of memory.

[2] Cf. *B. G.*, v, 13, § 4.

[3] Cf. Varro, *De lingua Latina*, vi, 89.

[4] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, p. 127. Heller's argument may be summarized as follows :—Caesar does not say that he weighed anchor in the ninth hour; had he done so, he would not have used the words 'weighing anchor' (*sublatis ancoris*) in the later passage in which he describes how he quitted his anchorage: he only says that he waited at anchor till the ninth hour for the overdue ships. Meanwhile, as he tells us, he issued his orders to the officers of the vessels which had already arrived; and, as orders had also to be given to the captains who were late in arriving, and they were obliged, after receiving their orders, to get back to their ships, delay was inevitable. That the turn of the stream did not take place until after the ninth hour is to be inferred from Caesar's having used the words (His dimissis et ventum et aestum) *uno tempore* (nactus secundum), which refer only to *et ventum et aestum* (*Philologus*, xxii, 1865, p. 308).

Dr. Guest (*Origines Celticae*, ii, 347, note) puts the matter well. '*In anchoris*

have argued, we have no right to infer from the words *in ancoris exspectavit* that when the period of waiting was over, Caesar ceased to remain at anchor.[1] And, even if he gave no instructions to the captains of the laggard ships, but left them to their own devices, it remains certain that to get the ships into order, to give the signal for starting, and to weigh anchor, consumed an appreciable time.

But we need not insist upon this argument. We may rest satisfied with the knowledge that the stream may have turned earlier than 3.54 p.m.[2] We may be assured that Caesar had no means of knowing exactly at what time he weighed anchor; and therefore, even if he intended to convey that he weighed anchor in the ninth hour, we are not compelled to assume that he did so before 3.30 p.m. We know that *Caesar was too wise to start for a seven miles' sail on the last of the ebb tide, when the current was slowest, and when he would have had no certainty that it might not turn against him before he had completed his voyage*; and we are confirmed in this conviction by his own words, which tell us that he started when he had got wind and tide in his favour. Finally, *we have proved that it is not improbable but impossible that he landed either at Pevensey or on any part of Romney Marsh.*[3] Thus the question of the direction in which the tide was running when Caesar sailed from his anchorage, which in itself would be doubtful, is settled by other considerations. There is but one conclusion to which we can come, and that conclusion is absolutely certain: when Caesar weighed anchor off the Kentish cliffs, he sailed towards the north-east.[4]

exspectare dum can only,' he observes, 'mean, to wait at anchor for the happening of the event. If we add the words *ad horam nonam*, surely we make the ninth hour the limit, not of lying at anchor, but of waiting for the event . . . Caesar probably steered for Dover with the view of landing his men as the vessels came in, but finding his landing opposed, he awaited the arrival of his other vessels *in anchoris*, *i.e.* in the roadstead. The emphasis [laid on *in anchoris*] marks the change of plan occasioned by the unexpected opposition he met with.'

[1] Lewin, indeed, objects (*The Invasion of Britain*, &c., 1862, p. xci) that 'had another interval of two hours occurred [after the ninth hour] Caesar could not fail to have mentioned it'. But, in the first place, as the reader will have seen, it is unnecessary to assume that the interval lasted as long as two hours, or even one; and, in the second place, there was no reason why Caesar should mention it, except for the benefit of stupid readers, whom he invariably left to their own devices. Some interval there must have been unless the captains of the laggard ships were left without the instructions which had been given to the rest.

[2] See pp. 610-1, *supra*.

[3] If, then, he really did weigh anchor in the ninth hour, and if in the ninth hour the stream was running down the Channel on the 26th of August, he must have landed on the 25th; and it has been shown (p. 600-3, *supra*) that he may have done so.

[4] George Long, who was a very able man, was nevertheless capable, if hard pressed in controversy, of writing sheer nonsense. Having only the most superficial knowledge of the tides, he submissively accepted the assertion that, at the time when Caesar weighed anchor off the Kentish cliffs, the stream was running westward; yet he insisted that Caesar landed at Deal! Let him speak for himself. 'When Caesar says that the tide (*aestus*) was favourable, he means that he had water sufficient to keep near the shore. There is only

3. It will be remembered that Caesar, in describing the final stage of his first voyage, says that he 'moved on' (*progressus*[1]), seven miles from his anchorage. Both Long[2] and Heller[3] regard the word *progressus* as a proof that Caesar must have landed on the coast of East Kent. Had he anchored off East Wear Bay, and sailed thence to Hythe or to Lympne, he would not, says Heller, have continued *in a straight line* the direction of his voyage from Gaul to Britain; but he would have done so if he had anchored off the South Foreland and sailed thence to Deal. I do not commit myself to absolute agreement with this argument, as it stands, although every one who is familiar with Caesar's use of the word *progredi*[4] will admit that it would apply much better to a run from the Dover cliffs to Walmer, made in continuation of a voyage from Boulogne towards the South Foreland, than to a run from East Wear Bay or from a point off Folkestone to Hythe or Lympne.

4. Dion Cassius says that Caesar, in sailing from his anchorage to the place where he landed, rounded a promontory.[5] I attach some importance to these words for the following reasons. Although Dion is no authority in regard to such matters as the details of a battle, and although, from carelessness or love of meretricious ornament, he constantly misunderstood or misused his authorities, it is not conceivable that he should have made so definite and simple a statement as this without authority, unless he had invented it. I am sure that every one who is capable of weighing the credibility of an historical writer will agree with me that this is not such a statement as Dion would have invented, and that it is such a statement as the most careless and rhetorical of writers, if he had found it in an authority, would have followed correctly. I therefore believe that Dion took it from some authority which is now lost, and that it is true. If so, the promontory which Caesar rounded can only have been the South Foreland. Lewin[6] admits that the statement, 'if taken literally, looks as if he went round the South Foreland; but,' he adds, 'I am satisfied that if he had done so, Caesar would have

one meaning of *aestus* in Caesar . . . Caesar says that he went with wind and tide favourable. If "tide" means stream, his statement is not true. If he means by "tide" what I have said—and there is not the least doubt of that—I should like some sufficient reason to be given why he could not go to Deal, though the stream was against him' (*The Reader*, Sept. 5, 1863, pp. 254–5). This singular argument was demolished with somewhat needless vigour by Dr. Guest (*Origines Celticae*, ii, 1883, pp. 364–5). If in the often quoted passage, *longius delatus aestu orta luce sub sinistra Britanniam relictam conspexit*, the word *aestus* does not mean 'the tidal *stream*', it means nothing. That it does mean what I have said Long virtually admits when, in his edition of the *Commentaries* (p. 225), commenting on this very passage, he observes that Caesar 'was carried out of his course by the flood tide'.

[1] *B. G.*, iv, 23, § 6.
[2] *Decline of the Roman Republic*, iv, 434.
[3] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, p. 129. See also *Archaeol. Journal*, xxi, 1864, p. 238.
[4] See H. Meusel, *Lex. Caes.*, ii, 1245–7.
[5] *Hist. Rom.*, xxxix, 51, § 2.—ἄκραν οὖν τινα προέχουσαν περιπλεύσας, &c.
[6] *The Invasion of Britain*, &c., 1862, p. 52.

mentioned so remarkable a promontory'. This observation only shows that Lewin was not familiar with Caesar's style. Caesar did not trouble himself about picturesque details, however remarkable they might be, the mention of which was not essential to the clearness of his narrative. 'If,' continues Lewin, 'the descriptive words of so late a writer as Dion are to have any weight, I should interpret them as meaning only that Caesar sailed round the bend of the precipitous shore between Folkestone and Sandgate . . . or else that Caesar arrived at first off Eastweir Bay . . . and then sailed round the cliff which shuts in the bay on the west.' I doubt, however, whether either 'the bend' or 'the cliff' could fairly have been described as 'a projecting headland' (*ἄκραν προέχουσαν*); and Lewin virtually admits that neither would have been worth mentioning. But if any one gainsays this argument, we can well afford to dispense with it.

5. If Volusenus deserved the confidence which Caesar reposed in him, there cannot be the faintest doubt in the mind of any unprejudiced man who has studied military history or even Caesar's *Commentaries*, and has taken the trouble to observe the features of the Kentish coast, as to the landing-place which he would have selected. He would not have wasted his time by cruising down the coast of Sussex: the first sight of the lofty hills which hem in East Wear Bay, of the cliffs which extend from Folkestone to Sandgate, and of the heights which back the coast from Shorncliffe to Lympne and beyond, would have warned him not to advise the great captain whom he served to land beneath them; but, as his galley glided past Kingsdown and neared Walmer, he saw, stretching away towards the neighbourhood of Sandwich, the open coast, offering easy access into the interior, of which he was in search. Here and here only, if Dover Harbour were held by an enemy, it would be safe to land. Here and here only it would be possible to follow up a victory by an effective cavalry charge.

6. Assuming that Caesar landed in East Kent, his narrative of the adventures of his cavalry transports and of the shipwrecks in 55 B.C., which, as we have seen,[1] is inexplicable on any other theory, presents no difficulty. The transports which returned to the port whence they started would have been laid to on the port tack, and would have been carried back to Ambleteuse by the same wind which drove their sister ships westward down the Channel.[2] This wind, which could not have driven ashore, in the immediate neighbourhood of their anchorage, vessels anchored off Lympne or Hythe, would inevitably have driven ashore, once they had parted their anchors, vessels lying off the coast between Walmer and Deal.[3]

[1] See pp. 558, 582–3, 613, 618–9, 624–5, 639, 643, *supra*.

[2] This statement has been approved by Commander Boxer, R.N., Harbour-Master of Folkestone, to whom I submitted it. See Addenda, p. 740.

[3] In March, 1898, a north-easterly gale sent the waves rushing over the sea-wall at Deal and across the road. W. H. Wheeler, *The Sea-Coast*, 1902, p. 301. Cf. C. Seymour, *New Topographical . . . Survey of Kent*, 1776, p. 410.

7. Finally, Caesar sailed on his second voyage to Britain with a south-west wind; and a south-west wind would obviously have been much more favourable to a voyage either from Boulogne or from Wissant to Deal or to any point on the coast of East Kent than to a voyage from Boulogne to Lympne or even Hythe.

It remains to consider objections.

1. I have already refuted Airy's criticism of the view that Caesar anchored off the cliffs of Dover; and we have seen that Airy is compelled by his own argument to identify the cliffs which Caesar describes as 'precipitous heights', and Cicero as 'astonishing masses of cliff'[1] with cliffs 'ten to thirty feet high'.[2] He maintains that our interpretation of the words *angustis montibus* 'must be guided by consideration of the character of place under which an officer would think of attempting to land', and 'must also depend upon the possibility of aiming a javelin from the heights'. 'Both considerations,' he insists, 'exclude such lofty cliffs as those of Dover and Folkestone.'[3] One might have expected that a man so clever as Airy would have perceived that Caesar never intended to land under *angusti montes* at all, whether they were 300 or, as Airy will have it, 10 feet high. He intended to land in the gap between the *angusti montes*, that is to say, in Dover Harbour.[4] Airy, indeed, contends that 'neither Caesar nor Volusenus would think for a moment of pushing his boats into a creek whose defenders could attack them on both sides';[5] but he forgets that Caesar had reason to expect that his landing would not be opposed.[6] As a matter of fact, Caesar says nothing about javelins; but if Airy had stood on the beach at the foot of Dover cliffs and allowed an army standing above to pelt him with missiles, he would have speedily realized that it was possible to take aim from those 'lofty cliffs'; and, if he had survived the experiment, he would have been a wiser and a sadder man.

2. Lewin[7] says that 'if we assume that Caesar was wholly ignorant of the British coast . . . he could not have discovered the level at Deal'.

But what is the use of making absurd assumptions? As we have already seen, it is certain that before Caesar set foot in Britain he knew everything about the coast of East Kent that Volusenus could tell him.[8]

3. It has been objected that the distance from Dover to Walmer exceeds the seven miles which separated Caesar's anchorage from his landing-place. But no sensible man maintains that Caesar anchored exactly opposite Dover Harbour. He anchored off the

[1] See p. 329, *supra*.
[2] See p. 614, *supra*.
[3] *Essays on the Invasion of Britain*, &c., p. 29.
[4] See pp. 644–7, *supra*.
[5] *Archaeologia*, xxxiv, 1852, pp. 240–1.
[6] See pp. 309, 645, *supra*.
[7] *The Invasion of Britain*, &c., 1862, p. xc.
[8] *B. G.*, iv, 23, § 5. On p. lxxv Lewin himself maintains that Caesar, before he sailed from Gaul, 'was well enough informed of the smaller havens on the British coast,' &c.

cliffs east of Dover. He says that he sailed '*about* seven miles' (*circiter milia passuum VII*[1]) to his landing-place. The distance by sea from off Dover to a point just north of Walmer Castle is $7\frac{1}{2}$ English miles, or a little less than $8\frac{1}{6}$ Roman miles; from off the South Foreland to the same point $4\frac{7}{8}$ English miles, or a little more than $5\frac{1}{4}$ Roman miles. Accordingly, if we assume that Caesar's landing-place extended for about one mile north of Walmer Castle, and that his anchorage extended eastward for about one mile towards the South Foreland from a point about one mile east of Dover, the requirements of the *Commentaries* are satisfied absolutely; and even if the ship on the left of the line anchored just east of the harbour, off the Castle cliff, Caesar's rough estimate, which he qualified by the word 'about' (*circiter*), is hardly violated. Surely the difference between 'about seven' and $8\frac{1}{6}$ is not worth cavilling over.[2]

4. Lewin [3] maintains that the beach at Deal does not correspond with Caesar's account of his disembarkation. The Romans, he argues, evidently landed on a gradually shelving shore; whereas 'at Deal the beach . . . descends so steeply that with a three hours' flood transports can come up to the water's edge'.[4] Again, remarking that Caesar describes the shore on which he landed as 'open' (*apertum*) and 'level' (*planum*), he says that 'between Walmer and Deal . . . the ground is uneven, and cannot be called flat'. Lastly, he recalls the witnesses whose evidence he had cited in support of his own theory. 'Caesar, Dion Cassius, Plutarch, and Valerius Maximus, all . . . refer to the *marshes* at the place of landing . . . who has ever heard of a marsh at Deal.' [5] And again, 'Valerius Maximus speaks of *two small islands* at the landing-place. . . . It was never suggested that islands did exist or could have existed at Deal.' [6]

Now, to begin with, Lewin assumes that the beach between Walmer and Deal has undergone no change, and was as steep 2,000 years ago as it is to-day. In a previous article [7] I have shown that this assumption is untenable. Secondly, Lewin misunderstands Caesar's narrative. As some of the Britons threw their missiles from the shore,[8] it is evident that the deep water [9] in which the Roman ships grounded was within the range of a sling or of an arrow, that is to say, quite close to the shore. Thirdly, no shore is or could conceivably be 'level': the shore between Walmer and Deal is, as anybody may satisfy himself by the evidence of his own eyes, free from obstructions [10] (*apertum*) and, speaking generally,

[1] *B. G.*, iv, 23, § 6.
[2] See *One-Inch Ordnance Survey*, Sheet 290.
[3] *The Invasion of Britain*, &c., 1862, p. xciii; *Archaeologia*, xxxix, 1863, pp. 312-3.
[4] *Ib.*, p. 313.
[5] *Ib.*; *The Invasion of Britain*, &c., 1862, p. 50. Dr. Guest, on the other hand, maintains (*Archaeol. Journal*, xxi, 1864, p. 239) that 'the marshy lands off Deal' correspond exactly with Caesar's description. Caesar does not describe any marshy lands.
[6] *Archaeologia*, xxxix, 1863, p. 313.
[7] See pp. 523–5, *supra*.
[8] *B. G.*, iv, 24, § 3.
[9] *Ib.*, § 2.
[10] See p. 629, *supra*.

evenly shelving (*planum*)[1]. Caesar applied these epithets to the shore on which he ran his ships aground, not to 'the ground' behind it. As to the arguments which Lewin bases upon the statements of Plutarch, Dion Cassius, and Valerius Maximus, I have already shown[2] that they are worthless; but if the story told by Valerius Maximus were worth anything, it would not support, but destroy Lewin's theory. For, as we have seen, Valerius Maximus[3] speaks not of 'two small islands' but of one large island (*Britannica insula*), and a rock (*scopulus*); and there are no rocks off Hythe or Lympne.[4] Moreover, although the fact is of no consequence, there *are* rocks, called the 'Malms', which are visible at low water, during spring tides, opposite Deal;[5] and in the time of Caesar there were marshes behind the sand-hills north of Deal.

5. General Creuly,[6] referring to Caesar's description of the storm by which some of his cavalry transports, after they were descried from his camp in 55 B.C., were driven further down the coast,[7] maintains that the only point to which they could have been driven was Dungeness. It follows, he says, that the camp could not have been at Deal; for its distance from Dungeness is far too great.

The answer to this curious argument is, first, that it is plainly impossible to indicate the exact point to which the ships were driven; secondly, that it was certainly not Dungeness, for Dungeness did not exist in Caesar's time;[8] thirdly, that there is nothing in Caesar's narrative to show how far the point to which the ships were driven was from his camp; and, fourthly, that if the camp was at or near Deal, there is nothing in his narrative to show that the ships could not have run before the gale as far as the longitude of Dungeness, or even a good deal further. From Deal to Dungeness is only about 28 nautical miles; and if the ships approached the British coast in the morning,[9] they would have had the greater part of the day in which to make the run.

6. General Creuly[10] calls attention to the passage, which I have already more than once quoted, in which Caesar tells us that at daybreak, on his second voyage, he 'saw Britain lying behind on the port-quarter'. If, says the general, Caesar had steered for Deal, he would have had the coast of Britain on the port side throughout the voyage, and there would have been no point in the words *sub sinistra*. He insists that these words are simply the correlative of *longius*: Caesar saw Britain on the left because the flood had carried him *too far*.

[1] 'PLANUS proprie est aequus . . . in quo nihil eminet,' &c. (Forcellini, *Totius latinitatis lexicon*, iv, 1868, p. 695). That the shore where Caesar landed was only relatively *planum* is proved by the existence of the 'shallow places' (*vada*), the situation of which was known to the Britons but not to the Romans (*B. G.*, iv, 26, § 2).

[2] See pp. 630–1, *supra*.

[3] iii, 2, § 23.

[4] T. Lewin, *The Invasion of Britain*, &c., 1862, pp. li, 87.

[5] G. B. Gattie, *Memorials of the Goodwin Sands*, 1890, p. 297, note.

[6] *Rev. arch.*, nouv. sér., viii, 1863, p. 302.

[7] *B. G.*, iv, 28.

[8] See p. 536, n. 1, *supra*.

[9] See pp. 600–1, *supra*.

[10] *Rev. arch.*, nouv. sér., viii, 1863, pp. 302–3.

It will be remembered that Appach used this passage to prove that Caesar landed in the (assumed) Bay of Appledore.[1] Creuly uses it to prove that Caesar landed near Hythe. I have shown the futility of Appach's argument; and to answer Appach is to answer Creuly. I may remark, however, that Caesar does not merely say that he saw Britain 'on the left' (*sub sinistra*): he says that he saw it '*lying behind* on the left' (*sub sinistra relictam*). The coast could not have been described as 'lying behind on the left' until after Caesar had passed the South Foreland;[2] and, although he was steering not for Hythe, but for some point on the coast of East Kent, the flood tide did carry him 'too far'.

7. Many commentators have argued that Caesar's account of the last stage of his second voyage is inconsistent with the theory that he landed near Deal. It will be remembered that, after telling us that 'he saw Britain lying behind on the port quarter', he goes on to say that he then followed the turn of the tide, and that all his ships reached the landing-place by rowing towards midday. The tide which he followed was of course the westward stream: strictly speaking, its direction, if he had drifted to some point east of the Goodwin Sands, was south-west[3] (magnetic), or between south-west by south and south-south-west (true). It is very doubtful, Lewin thinks, whether, with a current running at the rate of $3\frac{3}{4}$ miles an hour, the fleet could, by the mere use of oars, have reached Deal at all; and it is certain that, in order to do so, it would have been necessary to steer 'across, if not actually against the current'. Caesar 'could not', Lewin concludes, 'be said to follow the tide when he was steering athwart it. Besides, as it must necessarily have been almost low water when the tide turned, had he held on for Deal he would infallibly have struck on the Goodwin Sands.'[4] Airy, on the other hand, asserts that Caesar 'must have been cast upon the Goodwin Sands' during the drift.[5]

Now the reader will have already perceived that if Lewin's argument tells against the view that Caesar landed near Deal, it is fatal to the view which Lewin himself defends. For Lewin's theory compels him to assume that Caesar had drifted no further than a point off the South Foreland:[6] in order to reach Hythe he would have had to row less than thirteen nautical miles, with the stream throughout; and it is therefore not easy to understand why his rowers should have been called upon to make any extraordinary

[1] See p. 643, *supra*.

[2] See p. 616, *supra*.

[3] *Admiralty Tide Tables*, p. 119.

[4] *The Invasion of Britain*, &c., 1862, pp. 82, xcii. Lewin also remarks that 'had he been making for Deal, he would in drifting up channel have been advancing in the right direction'. This remark is only worth noticing as an instance of Lewin's ignorance. Any one who has the most rudimentary knowledge of the tidal streams will see that once Caesar had drifted past the Foreland, the stream would have carried him further and further away from Deal.

[5] *Archaeologia*, xxxiii, 1851, p. 242.

[6] *The Invasion of Britain*, &c., 1862, p. xcii.

efforts. This point he ignores. Again, he admits, or rather insists, that Caesar must have drifted as far as the South Foreland; but it is easy to demonstrate that if Caesar had been steering for Hythe, he could not have drifted so far. For, as Lewin himself says, the length of the drift must, in that case, have been not less than twelve nautical miles.[1] Now the drift only lasted from about midnight till daybreak; and, assuming that it lasted four hours—a liberal estimate—the stream actually travelled, at the most, nine miles:[2] of course the ships would not have travelled so far if they had merely drifted,[3] but the faint breeze, without which they would not have had steerage-way,[4] may have made up the deficit. But, in order to give Lewin every chance, let us accept the most favourable of three estimates with which he himself supplies us. He says[5] that 'the greatest velocity of the tide is, according to the Tidal Tables, 3·3 knots an hour. . . . The drift would, of course, be less than the velocity. . . . From midnight till daybreak at 4 a.m., would, therefore, give a drift of twelve miles'. But Caesar's voyage took place on or about the 7th of July;[6] and daybreak was about 3.15 a.m. This consideration alone compels us to reduce Lewin's estimate to ten miles and a half; and, moreover, he forgets that the tide never runs for four consecutive hours, much less for the last four hours, at its greatest velocity.[7] Thus the argument upon which he relies

[1] *The Invasion of Britain*, &c., 1862, p. xcii.

[2] *Tidal Streams, English and Irish Channels.*

[3] Not more than about three-quarters of the whole drift, according to the harbour-master of Dover.

[4] See p. 576, n. 1, *supra.*

[5] *The Invasion of Britain*, &c., 1862, p. xcii.

[6] See pp. 728–30, *infra.*

[7] *Admiralty Tide Tables*, p. 119; *Tidal Streams*, &c. Lewin tells us, in another passage (*The Invasion of Britain*, &c., 1862, pp. 82–3) that, according to the captain of one of the steamers running between Folkestone and Boulogne, 'the maximum drift for a single tide, *i.e.* for the six hours that the stream runs in the same direction, is eighteen miles, and the minimum nine miles.' 'The fleet,' he adds, 'was heavily freighted, and therefore, sinking deep into the water, would receive the full shock of the tide . . . the expedition was on the very day of the full moon [which he wrongly assigns to the 18th instead of the 21st of July], when, of course, it was a spring tide. The drift, therefore, would be the maximum or near it. Now, if we draw a straight line from Boulogne to Limne, and then a line of sixteen miles, or thereabouts, at right angles to it up the channel, it will take us to a point off the South Foreland.'

It will be observed that Lewin here assumes that Caesar was steering not for Hythe but for Lympne, and accordingly he is forced to make the length of the drift sixteen instead of twelve miles! Facts, from his point of view, were rather elastic than stubborn things. The expedition did not take place 'on the very day of the full moon', but about the time of new moon (see p. 729, *infra*). This mistake, indeed, is immaterial; but the estimate of eighteen miles is, as we have seen, greatly exaggerated; and, moreover, Lewin forgets that Caesar's ships did not drift for the whole of one tide, but only from 'about midnight' till 'daybreak'.

In a footnote to p. 82 he says that, according to 'an experienced pilot', a loaded vessel 'would drift about 12 or 14 miles in the six hours, when the tide is at its greatest velocity'. Yes,—'in the six hours'; but not in four hours. And even 12 miles is an excessive estimate. 'As a rough general rule,' says Admiral Sir Frederick Bedford (*The Sailor's Pocket-Book*, 8th ed., 1893, pp. 232–3), 'in the fair way of both the Irish and English Channels a vessel will be carried nine miles by the stream in a whole tide at Springs.'

to prove that Caesar must have landed at Hythe turns and pulverizes his already shattered theory.

On the hypothesis that Caesar landed anywhere between Walmer and Sandwich, the statement that he 'followed the turn of the stream' presents no difficulty unless he meant that from the time when the tide turned until the time when he reached Britain his men were rowing hard exactly in the direction of the stream. This is what Lewin assumes. But Caesar says no such thing. What he says is that, having followed the turn of the tide, he rowed hard in order to gain the desired landing place. So long as the tide served, hard rowing was obviously unnecessary,—if the Goodwin Sands existed. In that case the true explanation is a modification of that offered by C. Schneider,[1] who says, 'As long as it was possible to follow the turn of the tide, rowing was unnecessary. But after they had reached a point where they could do so no longer without being carried past their destination, they took to rowing.' Of course they would not have trusted to the current alone at any time; but, supposing that the Goodwin Sands then existed or that an island occupied their site, they travelled south-westward with the current until they had turned the obstacle, and then rowed hard in a north-westerly direction, across the current,[2] till they reached the landing place.[3]

The question of the Goodwin Sands has been discussed in an earlier part of this book.[4] Either they did not exist in the time of Caesar, the substratum on which they rest being covered by the sea; or they did exist, but had not accumulated to their present height, or perhaps to their present extent; or they were virtually identical with the present sands, though their limits, which are not constant, may not have been the same as they are now; or, finally, as Sir Charles Lyell suggested, their place may have been occupied by an island. The question cannot be positively settled; but, for reasons which I have already given, I am rather inclined to believe that either Sir Charles Lyell's suggestion was right, or the sands had accumulated sufficiently to be visible, or at all events dangerous, at certain points at low water. I shall, however, presently take account of the possibility that neither of these alternatives is true.

Much depends upon the answer which is to be given to the question, To what point had Caesar drifted when he saw the coast of

[1] *Comm. de C. I. Caesaris bellis*, vol. ii, p. 41; *C. I. Caesaris b. G., libri VII*, ed. A. Doberenz and B. Dinter, vol. ii, p. 40.

[2] The direction of the ebb stream between the Goodwins and the shore varies between SW. and SW. ½ W. magnetic, or, approximately, between SW. by S. ½ S. and SSW. true; and its rate at springs varies from 1½ to 3 knots. *Admiralty Tide Tables*, p. 113.

[3] The late George Dowker (*Archaeol. Journal*, xxxiii, 1876, pp. 67–8, 70) maintained that Caesar drifted 'at the back of the Goodwin beyond the North Foreland', and that he 'returned on the other side of the Goodwin', and anchored off Stonar. On this theory it is impossible to account for the efforts which the rowers were obliged to make; and, as I have shown (pp. 575–6, *supra*), it is impossible that Caesar should have drifted beyond the North Foreland.

[4] See pp. 525–8, *supra*.

Britain ? Heller argues that this point must have been about nine miles due east of Ramsgate ; but, as I have shown in the previous article, his reasoning is unsound.[1] If Caesar had drifted to the point which Heller indicates, his course to Deal, where, according to Heller, he disembarked, would have been nearly south-west, that is to say, nearly identical with the direction of the Gull Stream, which is described in the Admiralty Tide Tables as SW. ½ W. (magnetic), or, approximately, SW. by S. true.[2] But, on this hypothesis, it would be inexplicable that his soldiers were obliged to row hard. Heller, indeed, conjectures that Caesar steered for the nearest point of the coast, that is to say, nearly due west, intending to keep close inshore until he found the landing-place ; and he remarks that this would explain why his men were obliged to use their oars instead of committing themselves to the stream alone.[3] But Caesar must have known the whereabouts of his landing-place ; and Heller's explanation seems to be far-fetched. Besides, as I shall presently show, Caesar, on his second expedition, did not land between Walmer and Deal, but in the neighbourhood of Sandwich.

For reasons which I have given in the article on the Portus Itius[4] I think we must conclude with Napoleon the Third,[5] that Caesar could hardly have drifted much further than a point on the latitude of Deal and east of the Goodwin Sands. When the tide turned soon after daybreak—about 4.30 a.m., if, as is probable, the day was the 7th of July[6]—he would have dropped down with the ebb as I have already explained. If, after he had passed the sands or the island, he had waited till about 9.30 a.m., the stream would have turned, and have begun to flow NE. ½ N.[7] magnetic, or, approximately, NNE. true : if he had not waited, he would have had to row hard, as I have shown above,[8] athwart a stream which was flowing at a rate varying from three to two knots, until it turned about 9.30.

The objection that, during the drift, Caesar 'must have been cast upon the Goodwin Sands' is as groundless as the objection, which has just been met, that if he had attempted to row to Deal [or to the coast between Sandown Castle and Sandwich] 'he would infallibly have struck upon the Goodwin Sands'.[9] Captain Iron, the harbour-master of Dover, traced out upon the chart in my presence the course which the Roman flotilla would naturally have steered from Boulogne, and showed that, after the south-west wind

[1] See pp. 574–7, *supra*.
[2] *Admiralty Tide Tables*, p. 119.
[3] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, p. 124 ; *Philologus*, xxii, 1865, pp. 309–10.
[5] *Ib.*
[4] See p. 576, n. 1, *supra*.
[6] I have not thought it necessary to have a calculation made of the hour of high tide at Dover on the 7th of July. Whether the stream turned a little earlier or later than 4.30 a.m. is unimportant. Every one admits that it turned not very long after daybreak.
[7] *Admiralty Tide Tables*, p. 119.
[8] See p. 657, *supra*.
[9] General von Göler apparently thought that Caesar had gone through the channel, between the North and the South Goodwins, which is known as 'the Swash' (see his map,—*Gall. Krieg*, 1880, Taf. 1). It is extremely doubtful whether this channel existed in 54 B.C.

dropped, it would have drifted east of the Sands. If, as is probable, the flat-bottomed vessels had made so much lee-way that, even before the wind dropped, they had got a little out of their course, they would have drifted still further eastward. It may also be objected that if Caesar, after he had followed the tide south-westward, had turned the sands or the island which may have occupied their place, he would have mentioned the fact. The objection would be quite natural if it came from a writer who had merely 'got up' so much of Caesar's narrative as he thought would be necessary to enable him to study the question; but every one who is really familiar with the *Commentaries* knows that Caesar often omitted to mention matters, especially geographical, which a modern historian would feel bound to record. There remains the possibility —perhaps the probability—that neither the Goodwin Sands nor the hypothetical island then existed. In this case Caesar would have had to row across the current: still he might fairly have said that he followed the turn of the tide. He was bound for Britain, and could not begin to row until the tide began to set towards Britain, though in a different direction from his: what other expression could he have used than *aestus commutationem secutus*? Lewin doubted whether he could have reached Britain by rowing at all; but Lewin did not understand what he was writing about, and ought to have consulted a treatise on practical navigation.[1] On the other hand, it might possibly be objected that he would not have taken till noon to reach his destination: that depends upon the exact direction of the current, which often varies from the direction indicated in the *Admiralty Tide Tables*,[2] and upon the rate at which the vessels could have been rowed in still water. What he says is that *all* his ships reached Britain by about noon; and doubtless there were stragglers. Of course it may be argued that there were not, and that all the ships were actually rowed for seven hours. But if any one thinks that the possible objection which I have anticipated is valid he will find himself confronted by another which is absolutely insuperable. For he must needs accept Lewin's alternative theory, to which the objection would apply with redoubled force,—that Caesar took seven hours to row from a point off the South Foreland (though he could not have drifted so far) to Hythe; in other words, that *by rowing hard Caesar could only manage to travel two knots an hour with the stream*; and that *he took twice as long to row less than thirteen knots with the stream as he had taken to drift twelve knots without rowing!*

8. Airy,[3] remarking that it is evident from the *Commentaries* that 'there were forests and cornfields near' the Roman camp, maintains, first, that if Caesar had landed near Deal 'he would have had for seven miles all round his camp bare chalk-downs, on which in those days there probably was neither a tree nor a ploughed

[1] See S. T. S. Lecky, *Wrinkles in Practical Navigation*, 1884, p. 414.
[2] See the caution in the Atlas entitled *Tidal Streams*.
[3] *Archaeologia*, xxxiv, 1852, pp. 243–4, 246.

field'; secondly, that a night march, such as that which Caesar made after he landed in 54 B.C., can only be made upon good roads; that 'the roads in a woodland and clay-ground country are almost invariable'; that accordingly the roads of East Kent 'are in the very same tracks as in the days of Julius Caesar'; and that Caesar's night march, the length of which was 12 Roman miles, would have brought him to the marshes of the Stour, whereas, if the Britons had been posted on that river, he would have crossed it 'at the sound ground of Canterbury or above it, and would have attacked their flank'; and thirdly, that if Caesar's march to the point where he crossed the Thames had begun near Deal, 'his course would have been all the way parallel to the Thames, and the expression "ad Tamesin"[1] could scarcely have been used.'

The first of these objections is summarily disposed of by Long,[2] who points out that at Worth, between Deal and Sandwich, there is 'some of the best wheat land in England'; and by Dr. Guest,[3] who remarks that 'the uplands round Deal are every autumn white with corn';[4] and that, as many of the great forests which once existed in England have disappeared, the absence of woods in the neighbourhood of Deal is no proof that there were none there in Caesar's time. But, as a matter of fact, there are no less than five woods at distances varying from about a mile to three miles and a quarter from Upper Deal;[5] and Caesar only speaks of one wood as having existed in the neighbourhood of his camp, and implies that it was a considerable distance off.[6] The second objection is valid against the theory that Caesar landed between Walmer and Deal *on his second expedition*, unless he encountered the Britons on the Little Stour; but I shall presently show that on that occasion he landed near Sandwich, and that his march of 'about twelve miles' did bring him either to 'the sound ground of Canterbury or above it', or to Fordwich or Sturry below Canterbury, but of course not to 'the marshes of the Stour'. The third objection, from the point of view of Airy, according to whom Caesar landed at Pevensey, may to some minds appear plausible; but the view that Caesar landed at Pevensey is out of the question. But the same objection has been urged by the advocates of Hythe; and they hardly deserve an answer. Supposing Caesar's march had been nearly parallel with the Thames, what then? If he had landed at or near Hythe, he must have first encountered the Britons at Wye on the Stour;

[1] *B. G.*, v, 18, § 1.
[2] *The Reader*, Sept. 5, 1863, p. 255.
[3] *Archaeol. Journal*, xxi, 1864, p. 241.
[4] In 1905 in the parish of Walmer alone three fields were planted with wheat, one of which, as I was informed by Mr. J. W. Minter of the Railway Hotel, covered eighteen acres. Moreover, as Mr. H. E. Malden observes (*Journal of Philology*, xvii, 1888, pp. 170–1), 'marks of ancient cultivation on the sides of downs, where no farmer would think of ploughing now, are common enough everywhere.' See also A. Pitt-Rivers, *Excavations in Cranborne Chase*, ii, 235, and p. 90, *supra*.
[5] *Ordnance Survey of England*, 6 inches to 1 mile, Sheet LVIII.
[6] *B. G.*, iv, 32, §§ 3–4.

and from Wye to Brentford[1] his march would have been hardly less parallel (if the expression may be pardoned) to the Thames than from the neighbourhood of Sandwich. In writing the words *ad Tamesim* he simply intended to indicate approximately the distance from his naval camp, either to the point where he crossed the Thames or to the nearest frontier of the territory of Cassivellaunus; and the direction of his march is nothing to the purpose. Whether that direction was nearly parallel to the Thames or at right angles to it, the distance was about 80 Roman miles.

9. It will be remembered that at the close of Caesar's second campaign his camp was attacked by the four chieftains of Kent.[2] General Creuly argues that if the camp had been at Deal, Caesar, when he was marching to the place where he crossed the Thames, must have traversed the country of the four chieftains without having first subdued them. If, on the other hand, says the general, he had started from Hythe, he would have marched not through the heart of their territory, but close to their frontier; and for this very reason they would not have thought it necessary to submit.[3]

Heller[4] has taken the trouble to answer this nebulous argument. He points out that Caesar, having defeated the chieftains in the engagements which immediately followed his arrival, evidently did not think them sufficiently dangerous to wait until he had secured their complete submission. Indeed, if their territory comprised the whole of Kent, or even that part of it which lies east of Maidstone, it is evident that Caesar, marching northward from Hythe to the Stour, and then turning westward or north-westward, would have traversed the heart of their country. Moreover, as Heller might have added, it would have been just as hazardous for Caesar to leave the chiefs unsubdued if he had marched from Hythe as if he had marched from the neighbourhood of Deal or of Sandwich. Furthermore, he had no time to spare; and unless he had completely laid waste their country, and treated their people with the ruthless severity with which he afterwards treated the Eburones[5]—and to do this would have required the greater part of the time which he had to spend in Britain—it would have been utterly impossible for him to subdue them so thoroughly as to prevent them from attacking his camp in his absence. Think of the Boers!

10. Lastly, it has been objected[6] that Caesar could not have landed near Deal, or indeed at any point on the coast of East Kent, because, if he had marched against Cassivellaunus from that neighbourhood, he must have passed through tracts abounding in beech-woods, whereas he says expressly that there were no beech-trees in Britain.[7] But Dr. Guest[8] disposed of this objection by pointing out that 'at whatever point on the south coast Caesar landed . . .

[1] See pp. 697–8, *infra*.
[2] *B. G.*, v, 22, §§ 1–2.
[3] *Rev. arch.*, nouv. sér., viii, 1863, p. 303.
[4] *Philologus*, xxii, 1865, pp. 309–10.
[5] *B. G.*, vi, 34, 43.
[6] *Athenæum*, Feb. 27, 1869, p. 317.
[7] *B. G.*, v, 12, § 5.
[8] *Origines Celticae*, ii, 370–2.

he must have crossed the North Downs on his way to the Thames, and so have passed through " tracts abounding in beech-woods " '. Mr. Mackinder,[1] indeed, asserts, without giving any authority, that the beech was introduced into this country by the Romans; but it has been found in submerged forests and in deposits of the Bronze Age.[2] If by the word *fagum* Caesar meant the beech, his statement was incorrect.[3]

XII. THE THEORY THAT CAESAR LANDED AT RICHBOROUGH OR SANDWICH

The commentators who believe that Caesar landed in the neighbourhood of Sandwich are not agreed among themselves. Napoleon III, who holds that he landed between Walmer and Deal in 55, and at or near Sandwich in 54 B.C., argues that the disaster which befell his ships on the night of the full moon in August, 55 B.C., must have taught him the danger to which they would be exposed on the beach near Deal, and that accordingly he must have selected a better landing-place 'some kilometres further north'.[4] Others, like the late George Dowker, maintain that he landed near Sandwich on both occasions.

Dowker assumes that Caesar's own ship, if not the rest of his fleet, anchored in 55 B.C. off the South Foreland; and he goes on to say that 'from the South Foreland . . . seven miles would bring him near the mouth of Sandwich Haven'. He decides for Sandwich instead of Deal because, in his opinion, the *Commentaries* show that Caesar landed 'at or near a point whence he could get his long vessels on the flank of an enemy', and at Deal 'no such bay existed', whereas at Sandwich the very bay which he wanted was formed by the mouth of the Stour.[5]

That Caesar did 'get his long vessels on the flank of an enemy'[6] is unquestionable: but he does not say that he placed them in a bay or in the mouth of the Stour or any other river; nor is it easy to understand why a bay or the mouth of a river should have been necessary for his purpose. The object of placing the 'long vessels' on the enemy's right flank was that the artillerymen, slingers, and archers who manned them might drive away the enemy who were

[1] *Britain and the British Seas*, 1902, p. 315.

[2] J. Prestwich, *Geology*, ii, 1888, p. 502; Clement Reid, *The Origin of the Brit. Flora*, 1899, pp. 69, 146. Cf. J. Evans, *Anc. Bronze Implements*, p. 339, and *Reliquary*, N.S., vii, 1901, p. 92.

[3] The late Professor Rolleston (*Sc. Papers*, ii, 1884, p. 780) argued that by *praeter* Caesar meant 'besides'. It is true that he used the word several times in this sense (H. Meusel, *Lex. Caes.*, ii, 1186–7): but when he did so the meaning was always unmistakable; and, as Mr. Colbeck remarks, in his school edition (p. 49), 'to say " there is timber of all sorts *besides* the beech and the fir " is hardly a natural expression, unless these two trees were the commonest form of timber [or were non-existent in Gaul], which they were not.'

[4] *Hist. de Jules César*, ii, 186, n. 2. Napoleon's map (pl. 16) contradicts his text.

[5] *Archaeol. Journal*, xxxiii, 1876, pp. 65–6, 71.

[6] *B. G.*, iv, 25, § 1.

trying to stop the disembarkation; and some of the enemy were standing on the shore, while the rest were wading, or mounted on horseback, or perhaps standing in their cars in the sea close to the water's edge.[1] Why should not the 'long vessels' have been in the sea too? What was to be gained by sending them into the mouth of a river? When Dowker said that a run of 7 miles would have brought Caesar from the South Foreland nearly to 'the mouth of Sandwich Haven', his eagerness to prove his point prevented him from making an accurate measurement. From the South Foreland to the place which was once the mouth of Sandwich Haven, as measured within half a mile from the shore on Sheet 290 of the One Inch Ordnance Map, is just over 11 statute, or 12 Roman miles. Nor does Battely contribute much to the argument when he pleads that *VIII milia passuum* 'does not occur invariably in all the editions', and that Caesar may have made a mistake. We are not concerned with 'the editions', but with the *MSS.*; and *VIII milia passuum* does not occur in any of them, but *VII* (or *septem*) *milia passuum* in all.[2] It would be strange if Caesar had not made a slight mistake; but it would be stranger still if he had mistaken twelve miles for seven. Battely[3] argues that he must have landed at Richborough, (*a*) because he says that Cantium, where the Gauls generally landed, has an easterly aspect, whereas Dover looks south; (*b*) because Dion's description of the landing-place, 'which, as to the nature of the shore, directly contradicts Caesar's narrative,' is applicable to Richborough, where there 'was a marshy and muddy shore, on which Caesar's soldiers . . . could not keep their footing';[4] and (*c*) because 'all the time the Romans were masters of our island, Rutupiae . . . was the only port where they disembarked'. Now Caesar, who never talks nonsense, does not say that the whole of Cantium, or even that part of it in which the Gauls used to land, faces the east. He merely says that of the side of Britain which faces Gaul 'one corner, by Kent—the part which almost all ships from Gaul make for—has an easterly . . . aspect' (*huius lateris alter angulus, qui est ad Cantium, quo fere omnes ex Gallia naves adpelluntur, ad orientem solem . . . spectat*[5]). Besides, nobody would argue that

[1] *B. G.*, iv, 24, § 3; 26, § 2.

[2] Mr. H. E. Malden (*Journal of Philology*, xvii, 1888, p. 167) says that 'the distance is given at seven or eight Roman miles in different MSS. of the Commentaries'. Why did he not specify the *MSS.* which have *VIII* or *octo*? No such *MS.* is mentioned in any critical edition.

[3] *Antiquitates Rutupinae*, 1711, pp. 23–4, 44–6, 49–50.

[4] Similarly, John. Harris (*Hist. of Kent*, 1719, p. 274) says that 'Caesar himself saith of his Men that they could not *firmiter insistere*, which implies the Ground was not Hard, Solid, and Good'. But Caesar only says that his men could not *firmiter insistere* while they were struggling in the water with the enemy; and in these circumstances a man could not *firmiter insistere* in a swimming bath, the floor of which is 'hard, solid, and good'.

[5] *B. G.*, v, 13, § 1. If, as I believe, *quo* means *ad quem*, referring to *angulus* and not to *Cantium*, if, that is to say, Caesar intended to convey that almost all ships from Gaul steered for the 'corner', Battely is demanding from Caesar a nicety and precision of geographical statement which it would be

Caesar landed at Dover; and the coast between Walmer and Deal has an easterly aspect no less than Richborough. Secondly, when Battely says that Dion's description of the landing-place 'directly contradicts' Caesar's narrative, and then elects to believe the inaccurate and rhetorical Greek, who wrote two hundred years after the event, rather than the eye-witness, he shows that he is incapable of serious criticism. Besides, it is not true that Dion's description of the landing-place[1] contradicts Caesar's narrative either directly or indirectly; and neither of them says that the shore was either 'marshy' or 'muddy'.[2] Thirdly, Rutupiae was not 'the only port' where the Romans disembarked after they had become masters of the island. It is certain that Dover Harbour was in use during the Roman occupation of Britain, for it is mentioned in the *Itinerary* of Antonine;[3] inscribed tiles found at Dover prove that it was one of the stations of the *Classis Britannica*,[4]—the Roman 'Channel Fleet'; and the mere fact that two disembarkations after the time of Caesar are recorded to have taken place at Rutupiae[5] does not prove the contrary.

The conclusion is that Caesar, on his first voyage, did not land either in the neighbourhood of Sandwich or at Richborough, and that he did land between Walmer and Deal. The disembarkation must of course have taken place along a front of considerable extent; and the most southerly point at which it would have been easily practicable is quite close to Walmer Castle.

In the following year, however, the fleet must have made the land somewhere between the site of Deal Castle and the latitude of Sandwich. For the Romans, as we have seen, on the morning after their disembarkation, fought a cavalry action on the banks of a river, at a spot about twelve miles from the camp which Caesar had constructed near the place of landing: the river, as we shall afterwards see,[6] was the Stour; and if the length of the march was estimated with tolerable accuracy, the camp must have been in the neighbourhood of Sandwich.[7] Besides, Caesar tells us that 'he felt little anxiety for the ships, as he was leaving them at anchor

idle to expect from an ancient writer. Dover is quite close to the *angulus*, even if we must rigidly limit the latter to the coast between the South and the North Foreland.

[1] *Hist. Rom.*, xxxix, 51, § 2.—*ἄκραν οὖν τινα προέχουσαν περιπλεύσας παρεκομίσθη· κἀνταῦθα τοὺς προσμίξαντάς οἱ ἐς τὰ τενάγη ἀποβαίνοντι νικήσας ἔφθη τῆς γῆς κρατήσας*, &c. *Τενάγη*, as we have seen (p. 631, *supra*) is simply Dion's translation of Caesar's *vada*.

[2] See pp. 628–31, *supra*.

[3] Ed. Wesseling, p. 473.

[4] V. J. Vaillant, *Classis Britannica*, pp. 41–2 and illustration facing p. 48.

[5] See A. Holder, *Alt-celtischer Sprachschatz*, ii, 1257–8.

[6] See pp. 678–82, *infra*.

[7] It may be argued that if Caesar had landed near Sandwich he would have landed in Richborough harbour. This objection, such as it is, would apply equally to Hythe and Pevensey; but it might have been dangerous to land in a harbour with a narrow entrance in the presence of an enemy; and Caesar may have had other reasons (see Lord Wolseley's *Soldier's Pocket-Book*, 1886, p. 240). Moreover, the shore of the harbour must have been very marshy.

on a nice open shore' (*eo minus veritus navibus, quod in litore molli atque aperto deligatas ad ancoras relinquebat*[1]). The meaning of the epithet, *mollis*, has been already explained;[2] and, moreover, although no commentator has noticed the fact, it does not need much acumen to see that Caesar was here excusing himself for having left his ships at anchor, in spite of the severe lesson which the storm of the previous year had given him, by the plea that he had selected a more favourable anchorage.[3]

. . . . . .

I began this inquiry early in 1900 with a mind absolutely unbiassed, resolved to do one of two things,—either to solve the problem, or, if that could not be done, to show, once for all, that it was insoluble. The reader knows that I have not neglected any means of ascertaining the truth; and I have provided him with the means of controlling every statement which I have made. I have set down fully and fairly the arguments of those from whom I differ; and I have kept back nothing, I have called attention to everything, that might appear to tell against the conclusion to which the evidence inevitably led. I need not say anything by way of recapitulation, for no man who has read this article attentively can be lacking either in patience or in intelligence; and I am sure that the reader is by this time convinced of these things:—that it has been demonstrated that Caesar did not land at Pevensey, or anywhere in Sussex; that it has been demonstrated that he did not land at Hythe, or anywhere in Romney Marsh; and that it has been demonstrated that he did land both in 55 and in 54 B.C. in East Kent,—in the former year between Walmer Castle and Deal Castle, in the latter north of Deal Castle. That some will still for a time dispute these conclusions is likely enough; but not those whose judgements count. For them the problem is solved.

NOTE.—The following is a transcript of the report of Messrs. Doak, Hudson, and Sprigge, sent to me from the Nautical Almanac Office, and alluded to on page 610, *supra*:—

'The calculations have proved somewhat more lengthy and complicated than was at first anticipated, since it was found on examination that no approximate process would give results of a satisfactory degree of accuracy. In consequence the following work has been done:—

1. Twelve complete places of the Moon were determined from Hansen's "Tables de la Lune", embodying all the inequalities. Longitude, Latitude, and Parallax were thus obtained.

2. The Sun's Longitude, the Obliquity of the Ecliptic and the Sidereal Time were then computed from Newcomb's Tables for seven successive Greenwich

[1] *B. G.*, v, 9, § 1.

[2] p. 630, *supra*.

[3] 'The anchorage in the Small Downs is much more secure than in the Downs, being more sheltered, with better holding ground, and shoaler water,' &c. *The Channel Pilot*, 9th ed., 1900, part i, p. 344. I am informed by Mr. Jordan, one of the Deal boatmen, that ships driven ashore between Sandown Castle and Sandwich would suffer far less damage than off Walmer or Deal; and they would probably have suffered somewhat less even when the Deal shingle was much less steep.

Mean Noons. A slight shortening was possible in the case of the Longitude, but the resulting error cannot, in the opinion of Professor Newcomb, exceed ± 30 seconds of arc, and is probably about ± 12 seconds.

3. The Moon's position was then converted from Longitude and Latitude to Right Ascension and Declination.

4. The Greenwich Mean Time of Moon's Transit was computed for twelve transits.

5. The times of High Water at Dover were obtained by applying the proper quantities from the Admiralty Tidal Curve for Dover to the times of Moon's Transit.

6. From the Longitudes of Moon and Sun the time of Full Moon was determined.

7. The whole of the calculations were then examined and duplicated where desirable.

With the one exception of the Sun's Longitude the calculations have been rigorous, and, so far as the computing is concerned, are of the same degree of accuracy as those published annually in the Nautical Almanac. A very slight divergence from the truth is, however, possible owing to the fact that the tables of the Moon and Sun are used for an epoch nineteen-and-a-half centuries ago; but it is very unlikely that this is large enough to affect the times of high water or of full moon.

4th October, 1902.']

THE CREDIBILITY OF CAESAR'S NARRATIVE OF HIS INVASIONS OF BRITAIN

I have already published in *Caesar's Conquest of Gaul* an essay upon the credibility of Caesar's *Commentaries*; but, as it only deals incidentally with the invasions of Britain, I intend in this place to examine certain charges which have been made against that section of Caesar's narrative. It has been objected, perhaps justly, to the essay by competent critics in this country (though not on the Continent [1]) that it devotes an undue amount of space to the refutation of inferior writers; but at all events those who have read it will believe that the charges which I leave unnoticed in the present article may safely be disregarded.

I agree with Dr. F. Vogel that, apart from internal evidence, virtually the only authority by which we can test the *Commentaries* is the correspondence of Cicero; and the opinion which I have already expressed of Dion Cassius [2] is supported by the German scholar.[3]

1. Dr. Vogel points to a passage in one of Cicero's letters to Atticus,[4] which, he says, has not yet received due attention or been correctly explained:—*Britannici belli exitus exspectatur; constat enim aditus insulae esse muratos*[5] *mirificis molibus; etiam illud iam cognitum*

[1] Prof. B. Niese devotes the greater part of his valuable review (*Hist. Zeitschrift*, xciii, 97–101) to a criticism of this section of my book.

[2] *Caesar's Conquest of Gaul*, 1899, pp. 179–80.

[3] *Neue Jahrbücher für Philologie*, &c., cliii, 1896, pp. 269–71. Cf. *Rev. celt.*, xxii, 1901, p. 87.

[4] iv, 16, § 7.

[5] The MS. reading is *miratos*, which is obviously absurd. The emendation generally accepted is *munitos*. Professor Tyrrell in an admirable note (*The*

est, neque argenti scripulum esse ullum in illa insula neque ullam spem praedae nisi ex mancipiis, &c. The following translation will be generally accepted as accurate :—' The result of the British war is a source of anxiety. For it is notorious that the approaches to the island are ramparted by astonishing masses of cliff; and, besides, it is now known that there isn't a pennyworth of silver in the island, nor any hope of loot except from slaves.' But Dr. Vogel insists that the words *Britannici belli exitus exspectatur* ' can only mean " the end of the British war is expected ", that is to say, it is expected that the war, being too hazardous and too unremunerative, will be entirely abandoned '.[1] The letter in question was written, as Dr. Vogel remarks, before the 3rd of July, A.U.C. 700, that is to say, before the 8th of June of the Julian calendar.[2] He goes on to say that for some time afterwards it must have continued an open question whether the expedition was not to be given up ; for in a letter written after the 15th of July,[3] Cicero says to his brother, ' I wish that you could come at the time you arranged ' (*cuperem te ad id tempus venire quod dixeras*). But, says Dr. Vogel, Caesar says not a word to show that the proposed expedition was almost abandoned. Not a word directly : but Dr. Vogel believes that he can detect a significant hint in the *Commentaries*. Caesar relates that, before he sailed for Britain, he was obliged to march into the country of the Treveri, who were said to be preparing for rebellion. Soon after the legions entered the country, Indutiomarus, the leader of the malcontents, finding that resistance would, for the time being, be hopeless, sent envoys to Caesar to assure him of his loyalty and to make excuses for not having come in person to pay his respects. On which Caesar remarks, ' Caesar was aware of his motive for saying this, and of the circumstances that deterred him from prosecuting the design which he had formed : still, in order to avoid having to waste the summer in the country of the Treveri after having made all his preparations for a campaign in Britain, he told Indutiomarus to present himself with two hundred hostages ' (*Caesar, etsi intellegebat qua de causa ea dicerentur quaeque eum res ab instituto consilio deterreret, tamen, ne aestatem in Treveris consumere cogeretur omnibus ad*

Correspondence of Cicero, ii, 1896, p. 134) remarks that it is incredible ' that any copyist found the obvious *munitos*, and wrote the inexplicable *miratos*. But if he found the ἅπαξ εἰρημένον *muratos*, he would be nearly certain to write *miratos*, a common word very near it in form, and that without at all troubling himself as to the sense of the passage : just as a compositor will set up " serious effusion " if one writes " serous effusion " '. And, anticipating the objection that *muratos* is a post-classical word, he says, ' We must remember that we have in these letters a unique department of literature. A man might easily write in a letter that the approach to Britain was " absolutely *ramparted* with masses of cliff ", though he would not use that word in a formal composition.' See also pp. vii–x of the preface to Professor Tyrrell's second volume.

[1] *Neue Jahrbücher für Philologie*, &c., cliii, 1896, p. 277. Dr. Vogel actually takes *molibus* to mean not ' masses of cliff ' but ' defensive works ' !

[2] See § 5 of the letter in question—*Drusus reus est factus a Lucretio. Iudicibus reiciendis a. d. V. Non. Quinct.* See also *Hermes*, xl, 1905, pp. 17–9.

[3] *Q. fr.*, ii, 14, §§ 3–4.

Britannicum bellum rebus comparatis, Indutiomarum ad se cum ducentis obsidibus venire iussit[1]). Now, says Dr. Vogel, although Caesar is silent as to his real motive, we may divine from the words 'in order to avoid having to waste the summer in the country of the Treveri after having made all his preparations for a campaign in Britain' that he hesitated for some time to carry out his resolve of making a second British expedition.[2]

This charge rests upon a mistranslation.[3] The words *Britannici belli exitus exspectatur* will certainly bear the interpretation which I have put upon them: why, then, suggest another interpretation, which, even if it could be got out of the Latin, would necessitate the assumption that Caesar did not know his own mind, and that, after he had kept his army busy for six months building six hundred ships, he told his staff that, all things considered, it would be better not to make use of them, and presently changed his mind again, and did make use of them? Again, in his quotation from Cicero's letter to Quintus, Dr. Vogel leaves out the important part. The passage runs as follows:—*Qua re suavitatis equidem nostrae fruendae causa cuperem te ad id tempus venire quod dixeras, sed illud malo tamen, quod putas magis e re tua; magis . . . illa etiam magni aestimo, ἀμφιλαφίαν illam tuam et explicationem debitorum tuorum.* Read Professor Tyrrell's translation:—'Wherefore I should indeed wish that you could come at the time you arranged, for the sake of our pleasure in each other's society; but yet I desire more that you should do what you think your interests demand [and stay in the camp of Caesar]; still more do I value other considerations, your being in easy circumstances, and free from embarrassments.' Professor Tyrrell goes on to point out that 'the words printed in italics [*e re tua; magis*], or some such words, must, as Wesenburg suggested, have fallen out',[4] &c. As for Caesar's words, *ne aestatem in Treveris consumere cogeretur omnibus ad Britannicum bellum rebus comparatis*, I confess that my powers of divination are not equal to those of Dr. Vogel. I am unable to infer from these words that Caesar hesitated for a moment to carry out his matured resolve.

Dr. Vogel then calls our attention to the well-known letter in which Cicero complained that he had had no news either from Quintus or from Caesar for more than fifty days, by which he meant that the last letters which he had received were *dated* more than fifty days before the time at which he was himself writing.[5] Caesar's last letter had been written on the 1st of September, and Quintus's apparently a few days earlier.[6] On the 25th of September they both wrote

[1] *B. G.*, v, 4, § 1.

[2] *Neue Jahrbücher für Philologie*, &c., cliii, 1896, pp. 278–80.

[3] See *Jahresberichte d. philol. Vereins*, pp. 240–1 (in *Zeitschrift f. d. Gymnasialwesen*, 1897).

[4] *The Correspondence of Cicero*, ii, 1886, p. 126.

[5] *Q. fr.*, iii, 3, § 1.—Sed me illa cura sollicitat angitque vehementer, quod dierum iam amplius L intervallo nihil a te, nihil a Caesare, nihil ex istis locis non modo litterarum, sed ne rumoris quidem adfluxit.

[6] *Ib.*, iii, 1, §§ 17, 25.

again.[1] Dr. Vogel admits that from the 1st to the 25th Caesar and Quintus were engaged in operations against Cassivellaunus. But, he says, this does not satisfactorily explain the long break in the arrival of letters. In order to understand it, one must take into account a circumstance which Caesar himself relates,[2] but the importance of which he minimizes, namely, that while he was campaigning on the north of the Thames, the four kings of Kent made an attack on his naval camp. It is true, says Dr. Vogel, that the attack was repulsed; but what did the Roman success amount to? All that Caesar can say for his troops is that they returned from their sortie unhurt. How long, owing to this outbreak in Kent, the communication between his army and his fleet was interrupted he tries to conceal by the meaningless expression, 'while the operations above mentioned were going on in this district' (*Dum haec in his locis geruntur* [3]). As he goes on to say that Cassivellaunus, owing to the failure of the attack on the naval camp, sent envoys, one may read between the lines of his narrative that it was the action of the four kings which induced him to accept the embassy, even if he did not actually invite it.[4]

To 'read between the lines' is always easy: the only difficulty is to avoid reading what is not there. The reason for 'the complete break in the arrival of letters' to which Dr. Vogel refers is intelligible to any reader who is not determined to convict Caesar of *suppressio veri*: the reason is either that, as I have elsewhere shown, Caesar was engaged, first in marching back from the coast into the interior, after a temporary visit to his naval camp, then in negotiations with Cassivellaunus, and, finally, in leading his army back to the sea; or, if he remained in his naval camp from the 1st to the 25th of September,[5] that he saw no reason for writing to Cicero, or did not think it worth while to send a ship to Gaul for the sole purpose of conveying a letter. What Dr. Vogel calls 'the meaningless expression', *Dum haec in his locis geruntur*, is one of a class of expressions, all containing the words *dum haec geruntur*, which Caesar uses thirteen times: [6] like our 'meanwhile', it is doubtless wanting in chronological precision, but it is not meaningless. Dr. Vogel asserts that 'the complete break in the arrival of letters' was due to 'the outbreak in Kent' on the part of the four kings. Yet he admits that the attack which the four kings made on the naval camp was repulsed; and Caesar adds that the garrison killed many of the enemy, and captured their leader, Lugotorix. In other words, their success was complete. All that they had to do was to beat off an attack, and this they effectually did. Even assuming that 'the complete break in the arrival of letters' was due to the action of the four kings, what then? It was not Caesar's business to chronicle postal irregularities, but simply to describe his campaign.

To the very end of the narrative Dr. Vogel continues to read

[1] *Att.*, iv, 18, § 5. [2] *B. G.*, v, 22, § 1. [3] *Ib.*
[4] *Neue Jahrbücher für Philologie*, &c., cliii, 1896, p. 282.
[5] See pp. 731–3, *infra*. [6] Cf. H. Meusel, *Lex. Caes.*, i, 967.

between the lines. 'Thoroughly characteristic,' he tells us, 'is the way in which Caesar describes the results of the expedition. It is true that his account[1] substantially tallies with what Cicero writes to Atticus.—"On the 24th of October I received letters from my brother Quintus and from Caesar, dated from the nearest coasts of Britain on the 25th of September. They had settled affairs in Britain, received hostages, and imposed tribute, though they had got no booty." (*A Quinto fratre et a Caesare accepi a. d. IX Kal. Nov. litteras, datas a littoribus Britanniae proximis a. d. VI Kal. Octobr. Confecta Britannia, obsidibus acceptis, nulla praeda, imperata tamen pecunia*,[2] &c.). But what about Cassivellaunus? How did Caesar balance accounts with him?' Dr. Vogel reminds us that Caesar's words have led Mommsen to believe that Cassivellaunus promised to pay tribute and to give hostages. But, says Dr. Vogel, Caesar nowhere says this: he only leads the reader to imagine it. What he says is this:—'On receiving news of the action [namely, the repulse of the four kings], Cassivellaunus, who was greatly alarmed by the defection of the tribes, following the numerous disasters which he had sustained and the ravaging of his country, availed himself of the mediation of the Atrebatian, Commius, and sent envoys to Caesar, to propose surrender. Caesar had resolved to winter on the Continent because disturbances had broken out suddenly in Gaul: not much of the summer remained; and the enemy, as he knew, could easily spin out the time. Accordingly he ordered hostages to be given, and fixed the tribute which Britain was to pay annually to the Roman People, at the same time expressly forbidding Cassivellaunus to molest Mandubracius or the Trinovantes.' (*Cassivellaunus, hoc proelio nuntiato, tot detrimentis acceptis, vastatis finibus, maxime etiam permotus defectione civitatum, legatos per Atrebatem Commium de deditione ad Caesarem mittit. Caesar, cum constituisset hiemare in continenti propter repentinos Galliae motus neque multum aestatis superesset atque id facile extrahi posse intellegeret, obsides imperat et quid in annos singulos vectigalis populo Romano Britannia penderet constituit; interdicit atque imperat Cassivellauno ne Mandubracio neu Trinovantibus noceat.*[3]) Now, observes Dr. Vogel, in the first sentence *Cassivellaunus* is grammatically the subject, and in the last the object; but the reader involuntarily supposes him to be the object in the intermediate sentence as well. In other words, the reader takes for granted that Cassivellaunus was ordered to give hostages, though Caesar does not say so. Moreover, the first sentence, taken by itself, leads one to suspect Caesar's good faith. For how came Caesar's understrapper, Commius, to be with Cassivellaunus? Is it not clear that Caesar had sent Commius to Cassivellaunus as his envoy? In other words, that, whereas Caesar represents Cassivel-

[1] *B. G.*, v, 22, §§ 3–5.

[2] *Att.*, iv, 18, § 5. Cicero does not mention that a large number of prisoners had also been taken; but Dr. Vogel admits this to have been the fact. Cf. *Q. fr.*, iii, 9, § 4, and *B. G.*, v, 23, § 2.

[3] *Ib.*, v, 22, §§ 3–5.

launus as having been driven by a series of reverses to offer submission, Caesar had in reality himself made overtures to Cassivellaunus ?[1]

If I were Caesar's advocate I should merely reply that as Cicero states, on the testimony of his brother, hostages were given. But, as Caesar was undoubtedly not incapable of misrepresentation, Dr. Vogel's suspicion is possibly not groundless. Caesar may have sent Commius to Cassivellaunus with an offer of terms ; and if so, his narrative is so far misleading. On the other hand, Cassivellaunus may first have signified his willingness to submit ; and Caesar may then have employed Commius as his agent.

Dr. Vogel's general conclusion is that, although Caesar's narrative is not expressly contradicted by Cicero's letters, yet it was, from first to last, written for effect. Always literally true, it is often substantially false. His most effective weapon is the apparent clearness and candour of his style, which puts the reader off his guard, and prevents him from noticing how very ambiguous many of the statements are. He conceals essential facts and exaggerates the importance of trivial successes ; and he prevents the unwary reader from noticing the slowness of his progress in Britain by inserting in the twelfth and the two following chapters of his Fifth Book a general description of the country and its inhabitants, from which point of view this otherwise very inartistic interpolation must be regarded as a masterpiece of ingenuity.

My general conclusion is that the charges which Dr. Vogel has brought against Caesar's narrative for the most part break down, but that in the one instance which I have noted he may have detected a flaw.

2. Thomas Lewin, who was a writer of considerable ability, remarked, in regard to Caesar's narrative of the events that immediately preceded his departure from Britain in 55 B.C., that ' it is easy to see, notwithstanding the veil attempted to be thrown over the transaction, that he wanted only a plausible pretext for transporting himself and his army back to Gaul '.[2]

Caesar's account runs as follows :—' On the same day the enemy sent envoys, who came to Caesar to sue for peace. Caesar ordered them to furnish twice as many hostages as before and take them across to the Continent ; for the equinox was near, and, as his ships were unsound, he did not think it wise to risk a voyage in stormy weather. Taking advantage of favourable weather, he set sail,' &c. (*Eodem die legati ab hostibus missi ad Caesarem de pace venerunt. His Caesar numerum obsidum quem ante imperaverat duplicavit eosque in continentem adduci iussit, quod propinqua die aequinoctii infirmis navibus hiemi navigationem subiciendam non existimabat. Ipse idoneam tempestatem nactus . . . naves solvit*,[3] &c.). Where is 'the veil attempted to be thrown over the transaction ' ? Dion's account,[4] so

[1] *Neue Jahrbücher für Philologie*, &c., cliii, 1896, p. 288.
[2] *The Invasion of Britain*, &c., 1862, p. 66.
[3] *B. G.*, iv, 36, §§ 1–3. [4] *Hist. Rom.*, xxxix, 52, §§ 2–3.

far as it goes, confirms that of Caesar. 'From Dion,' remarks Lewin,[1] 'we learn that negotiations were opened by the intervention of some Morini who were friends of the Britons.' Evidently he had not read Dion with due attention: the negotiations of which Dion speaks[2] were opened immediately after Caesar had, in spite of British resistance, effected his landing; and Lewin forgets to add that the Britons induced the Morini to intervene. The truth of Caesar's narrative is confirmed by the humorous frankness with which he avows that the Britons did not all obey his orders. After describing his return to Gaul, he says, 'Two British tribes and no more sent hostages: the rest neglected to do so' (*Eo duae omnino civitates ex Britannia obsides miserunt, reliquae neglexerunt* [3]).

But I am not concerned to maintain that Caesar's object was to tell the truth, the whole truth, and nothing but the truth.[4] The most serious omission in his account of his British campaigns has passed almost unnoticed. In the section of this book which deals with the chronology of his operations I have demonstrated that either he passed over in silence a hurried temporary visit to his naval camp which he made just before the 1st of September (the 5th of August of the Julian calendar), 54 B.C., or, if he remained in the camp from that day until his final departure from Britain, his account of his negotiations with Cassivellaunus and of the return march of his army to the coast is misleading.[5] Moreover, as I have pointed out in the seventh chapter,[6] there is reason to suspect that he and his officers may have known more than he would admit about the connexion between the tides and the moon's age. But he told the truth, so far as he could ascertain it, when he had no solid motive for falsification; and when he wrote the *Commentaries on the Gallic War*, he could generally afford to be true.

[1] *The Invasion of Britain*, &c., 1862, p. 66.

[2] *Hist. Rom.*, xxxix, 51, § 3.

[3] *B. G.*, iv, 38, § 4. A. J. Dunkin, an antiquary whom Sir Leslie Stephen thought worthy of a place in the *Dictionary of National Biography*, devoted a large portion of the second volume of his *History of the County of Kent* to an impeachment of Caesar's veracity; but his charges are based upon sheer inability to construe easy Latin, general lack of scholarship, or, in some cases, pure invention. Cf. *The Gentleman's Magazine Library*, ed. G. L. Gomme,—Romano-British Remains, part ii, 1887, pp. 520–2.

[4] See O. E. Schmidt, *Der Briefwechsel des M. Tullius Cicero*, 1893, pp. 377–92, and Rice Holmes, *Caesar's Conquest of Gaul*, 1899, p. 243.

[5] See pp. 731–3, *infra*.

[6] p. 319.

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THE DISEMBARKATION OF THE ROMANS IN 55 B.C.

'The men . . . weighed down with their heavy cumbrous armour, had to leap down from the ships and keep their foothold in the waves,' &c. (*militibus autem . . . magno et gravi onere armorum oppressis simul et de navibus desiliendum et in fluctibus consistendum*, &c.[1]) This passage has occasioned needless perplexity to commentators who forgot that the ships' bows may have projected considerably, and also that when they were run aground they would necessarily have been buried for a considerable depth.[2] Thus it would have been possible to jump into four feet six inches or five feet of water from the bow of a ship whose draught was a good deal more.

THE SITE OF CAESAR'S CAMP IN 55, AND OF HIS NAVAL CAMP IN 54 B.C.

I have proved in an earlier article that Caesar landed in 55 B.C. between Walmer Castle and Deal Castle, and in the following year between Deal Castle and Sandwich.[3] Camden,[4] who assumed that Caesar landed at the same place on both his expeditions, remarked that 'for a considerable length under this shore [in the neighbourhood of Deal] are a number of heaps like banks which some suppose to have been blown up by the wind; but', he added, 'I rather take them for the fortifications or defences for ships which Caesar was ten days and nights throwing up.' If Camden was referring to the sand-dunes between Sandown and Sandwich, it is of course unnecessary to discuss his conjecture. Hasted[5] thought that the camp of 55 B.C. might be represented by 'remains of entrenchments still visible', (1) 'close to the shore between *Deal* and *Walmer Castle*', or (2) 'within the country round *Walmer Church*', or (3) 'upon a rise . . . between *Deal* and *Upper Deal*'. But these are mere guesses; and Professor Flinders Petrie, in his 'Notes on Kentish Earthworks',[6] says nothing which can support either Hasted or Camden, except that 'there is said to be a fosse around Walmer Church'. I can only add that nobody will learn anything by going to look for it. Dowker,[7] who deluded himself into the belief that Caesar had landed near

[1] *B. G.*, iv, 24, § 2.

[2] From a foot and a half to two feet, so Commander Boxer, R.N., the harbourmaster of Folkestone, tells me.

[3] See pp. 595–665, *supra*.

[4] *Britannia*, ed. R. Gough, i, 219. Cf. E. Hasted, *Hist. of Kent*, iv, 1799, p. 163, note *d*.

[5] *Ib.*, p. 162 and note *c*.

[6] *Archaeol. Cant.*, xiii, 1880, pp. 8–16.

[7] *Archaeol. Journal*, xxxiii, 1876, pp. 66, 68.

Sandwich in 55 as well as in 54 B.C., held that he had encamped in the first year between Sandwich and Worth, in the second on Richborough Hill. But Dowker himself maintained that Richborough Hill was isolated at spring tides; and would Caesar have omitted to mention that he had sailed into the Rutupian estuary? I am inclined to believe that he encamped in 54 B.C. on the gently rising ground near Worth.

Last year (1902) I spent two days in examining the coast between Kingsdown and Sandown.[1] The conclusion to which I came was that in 55 B.C. Caesar must have encamped either on the plateau between Walmer Castle and Kingsdown, or, much more probably, on the rising ground north-west of the plateau which is now covered by part of the town of Walmer. On returning to London, I opened the second volume of Napoleon's *Histoire de Jules César*, and found on pages 160 and 161 that Colonel Stoffel had adopted the latter alternative.

THE WAR-CHARIOTS OF THE BRITONS

'Chariots,' says Caesar,[2] 'are used in action in the following way. First of all the charioteers drive all over the field, the warriors hurling missiles; and generally they throw the enemy's ranks into confusion by the mere terror inspired by their horses and the clatter of the wheels. As soon as they have penetrated between the troops of cavalry the warriors jump off the chariots and fight on foot. The drivers meanwhile gradually withdraw from the action, and range the cars in such a position that if the warriors are hard pressed by the enemy's numbers, they may easily get back to them. Thus they exhibit in action the mobility of cavalry combined with the steadiness of infantry; and they become so expert from constant habit and practice that they will drive their horses at full gallop, keeping them well in hand, down a steep incline, check and turn them in an instant, run along the pole, stand on the yoke, and step backwards again to the car with the greatest nimbleness' (*Genus hoc est ex essedis pugnae. Primo per omnes partes perequitant et tela coiciunt atque ipso terrore equorum et strepitu rotarum ordines plerumque perturbant, et cum se inter equitum turmas insinuaverunt, ex essedis desiliunt et pedibus proeliantur. Aurigae interim paulatim ex proelio excedunt atque ita currus conlocant ut, si illi a multitudine hostium premantur, expeditum ad suos receptum habeant. Ita mobilitatem equitum, stabilitatem peditum in proeliis praestant, ac tantum usu cotidiano et exercitatione efficiunt uti in declivi ac praecipiti loco incitatos equos sustinere et brevi moderari ac flectere et per temonem percurrere et in iugo insistere et se inde in currus citissime recipere consuerint*).

M. G. Lafaye[3] gathers from this description that the object of the

[1] See pp. 736–7, *infra*.
[2] *B. G.*, iv, 33.
[3] Daremberg and Saglio, *Dict. des ant. grecques et rom.*, ii, 815–7.

warrior in running along the pole was to jump down in order to throw his javelin and to avoid being impeded in his movements by the proximity of the driver. M. Lafaye assures us that certain coins represent warriors moving on to the poles of their chariots in order to hurl their javelins: but they do not represent them as about to jump down;[1] and Caesar says that the warrior, after he had run along the pole, stepped back again on to the car.

Caesar[2] tells us that, after he had crossed the Thames in 54 B.C., Cassivellaunus dismissed the whole of his forces except 4,000 *essedarii*. Most commentators have inferred, I think rightly, from this statement that Cassivellaunus had 4,000 chariots; but it seems possible that by 4,000 *essedarii* Caesar may have meant 2,000 warriors and 2,000 drivers.[3] Napoleon the Third,[4] on the other hand, assumes that there were 'six *essedarii* par char'. It is unnecessary to make any assumptions; for, according to Diodorus Siculus,[5] who derived his information from Posidonius, every Gallic chariot carried a driver and one warrior. Furthermore, a coin of the Hostilian family, which was struck between 49 and 46 B.C.,[6] depicts a chariot drawn by two horses, and driven by a charioteer, who is accompanied by one warrior, armed with an oblong buckler.

Professor E. B. Tylor,[7] referring to Pomponius Mela,[8] Lucan,[9] and Silius Italicus,[10] argues that the Britons used chariots armed with scythes: Tacitus,[11] who derived his information from Agricola, says that the British army which encountered the latter in the Grampians included *covinnarii*, who, according to Pomponius Mela, were warriors who fought in scythed chariots;[12] and Jornandes[13] says that the chariots of the Britons were armed with scythes: but if the Britons whom Caesar encountered had used such chariots, he would certainly have mentioned the fact;[14] and no scythes are to be seen on the

[1] See E. Babelon, *Descr. des monn. de la république rom.*, i, 1885, pp. 243, 435–6, 462–4, 552.

[2] *B. G.*, v, 19, § 1.

[3] In the one passage (*ib.*, iv, 33, § 2) in which he calls the drivers *aurigae* he is obliged to do so in order to distinguish them from the warriors.

[4] *Hist. de Jules César*, ii, 192.

[5] v, 29, § 1. Cf. 1 Kings, xxii, 34.

[6] E. Babelon, *Descr. . . . des monnaies de la république rom.*, i, 549, 552.

[7] *Journ. Anthr. Inst.*, x, 1881, p. 128.

[8] *Chorographia*, iii, 6, § 52—[Britanni] dimicant non equitatu modo aut pedite, verum et bigis et curribus Gallice armatis: covinnos vocant, quorum falcatis axibus utuntur.

[9] *Pharsalia*, i, 426—et docilis rector *rostrati* Belga covinni. *Rostrati* is a conjecture, the MSS. having *monstrati*.

[10] *Punica*, xvii, 416–7.—Caerulus haud aliter, cum dimicat incola Thyles | Agmina falcigero circumvenit arta covinno.

[11] *Agricola*, 35–6.

[12] See W. Smith, *Dict. of Greek and Rom. Ant.*, 3rd ed., i, 560.

[13] *Monumenta Germaniae hist,—Iordanis Getica*, ed. Th. Mommsen, 1882, ii, 15—bellum inter se . . . saepius gerunt, non tantum equitatu vel pedite, verum etiam bigis curribusque falcatis, &c.

[14] M. Théodore Reinach (*Rev. celt.*, x, 1899, pp. 123–30) points out that the testimony of Frontinus (*C. Caesar Gallorum falcatas quadrigas eadem ratione palis defixis excepit inhibuitque* [*Strat.*, ii, 3, § 18]), if it is genuine, is negatived by Caesar's silence, and that it is probably an interpolation; that it may be inferred from a passage in Martial (*O iucunda, covinne, solitudo,* | *Carruca magis essedoque gratum Facundi mihi munus Aeliani*, &c. [xii, 24]) that a *covinnus*

Roman coins which depict war-chariots. Moreover, of the numerous interments of warriors with chariots that have been discovered in the department of the Marne not one showed any traces of scythes ;[1] nor have any such traces ever been found in Great Britain.[2]

I have remarked in my narrative of Caesar's second invasion[3] that very few chariot-burials have been found in this country. It is noteworthy that of the whole number—not more than a dozen—all but two were in Yorkshire, and not one in Scotland ; and also that whereas in many of the Gallic interments the chariot was placed in the grave entire, only the wheels and other detached parts were buried in Britain. The most famous of these discoveries was made nearly a century ago in a barrow on Arras Farm, close to the road between Beverley and York. Here in a large round grave in the chalk was found the skeleton of a man, inclining from which, one on each side, were two wheels, each two feet eleven inches in diameter.[4] 'Under and adjoining to each wheel,' writes Thurnam,[5] 'were the remains of the skeletons of two small horses, neither of them exceeding thirteen hands.'[6]

In the paragraph in which Caesar describes the tactics of the charioteers he says that 'as soon as they have penetrated between the troops of cavalry, the warriors jump off the chariots and fight on foot' (*cum se inter equitum turmas insinuaverunt, ex essedis desiliunt et pedibus proeliantur*). The editors generally assume that *equitum turmas* means 'the *hostile* troops of cavalry'; but von Göler,[7] with whom Napoleon III[8] agrees, rejects this view. He argues that in Caesar's first campaign in Britain, in his account of

was simply 'un cabriolet attelant à deux'; that Arrian (*Ars tactica*, 19) expressly distinguished British war-chariots from scythed chariots; and that neither Polybius, nor Livy, nor Diodorus Siculus, nor Dion Cassius ever describe the war-chariots of the Celts as scythed, although they often mention them. M. d'Arbois de Jubainville (*La civilisation des Celtes*, pp. 339–41) quaintly argues that the silence of Caesar can be explained by the assumption that scythed chariots, being as dangerous to friends as to foes, were only used exceptionally.

[1] A. Nicaise, *L'époque gaul. dans le dépt de la Marne*, 1884, pp. 23–4. Cf. *Rev. celt.*, x, 1889, pp. 233–6, and *L'Anthr.*, xiii, 1902, p. 66.

[2] Pitt-Rivers (*Excavations in Cranborne Chase*, iii, 109) is 'almost tempted to suggest' that a scythe blade, which he found at Woodyates, 'may be one of the war scythes which were attached to the [British] chariots, as mentioned by Strabo.' But Strabo (xvii, 3, § 7) does not say that the Britons had scythed chariots, but the Pharusii and Nigretes of Mauritania.

[3] p. 342.

[4] The diameters of the British chariot-wheels that have been found vary between 2 ft. 11 in., and 2 ft. 4½ in.

[5] J. B. Davis and J. Thurnam, *Crania Britannica*, ii, pl. 6 and 7, pp. 2–3, 6; J. Evans, *Anc. Bronze Implements*, pp. 134–5.

[6] This discovery proves that Arrian (*Ars tactica*, 19) and Dion Cassius (lxxvi, 12, § 3) were right in saying that British chariot-horses were small. Cf. p. 152, *supra*. For further details of the discoveries of British chariot-wheels, axles, &c. (by which various quaint conjectures in von Göler's *Gall. Krieg*, 1880, p. 156, n. 3, are stultified), see *Archaeologia*, xxi, 1827, pp. 41–2 ; W. Greenwell, *Brit. Barrows*, pp. 454–7 ; and a valuable article by Canon Greenwell, a proof of which he has kindly sent to me, and which, I presume, will be published in vol. lx of *Archaeologia*.

[7] *Gall. Krieg*, 1880, p. 137, n. 1.

[8] *Hist. de Jules César*, ii, 153, n. 7.

which the passage in question occurs, the Britons were not opposed by any cavalry, for Caesar had none with him ; and that the paragraph is not to be regarded simply as a general description of the tactics of the charioteers, but also as an explanation of the tactics which they had pursued in the combat described in the preceding paragraph. Moreover, he insists that if the warriors had jumped off their chariots when they had penetrated between troops of hostile cavalry, and had then allowed the drivers to turn round and move back, it would have been impossible for them to get on to the chariots again in case of need : hostile cavalry which allowed them to do this would have been worthless. 'According to my interpretation,' von Göler concludes, 'we are to understand by *et cum se inter equitum turmas insinuaverunt* that the warriors had penetrated within the intervals of *their own* cavalry . . . the moment of jumping down, always hazardous, was protected by their own cavalry, just as nowadays cavalry protect the limbering up and unlimbering of the horse-artillery associated with them.' See also pp. 136–7, and Taf. vii, fig 7, of von Göler's book, and pp. 688–91, *infra*.

THE OPERATIONS OF THE BRITONS DURING THE LAST FEW DAYS OF CAESAR'S FIRST EXPEDITION

After describing how he rescued the 7th legion, which had been sent out on a foraging expedition and surprised by a British force, Caesar tells us that he led this legion and the force with which he had marched to its assistance back to camp. 'Meanwhile,' he continues, 'our people were all busy, and the Britons who were still in their districts moved off' (*dum haec geruntur, nostris omnibus occupatis, qui erant in agris reliqui discesserunt*[1]). The words 'the Britons who were still in their districts' (*qui erant in agris reliqui*) evidently refer back to two passages in the thirtieth and thirty-second chapters of the Fourth Book of the *Commentaries*. In the former we read that after the storm which wrecked several of Caesar's ships the British chiefs who had disbanded their levies and come into the Roman camp 'renewed their oaths of mutual fidelity, and began to move away one by one from the camp and *to fetch their tribesmen secretly from the districts*' (*itaque rursus coniuratione facta, paulatim ex castris discedere et suos clam ex agris deducere coeperunt*). In the thirty-second chapter Caesar says that, just before the 7th legion was attacked, 'some of the natives *still remained in the districts*' (*pars hominum in agris remaneret*). Evidently, then, the meaning of the passage which I quoted at the beginning of this note is that during and after the attack on the 7th legion, and while the Roman soldiers were employed in various duties, those Britons who had not yet left their respective districts in order to rally round their leaders did so. However, the meaning which is obvious to the ordinary

[1] *B. G.*, iv, 34, § 3.

mind does not satisfy von Göler,[1] who insists that the *MS.* reading, *nostris omnibus occupatis, qui erant in agris reliqui discesserunt*, yields no satisfactory sense, and offers in place of it one of his conjectural emendations :—(nostris omnibus occupatis,) *quae erant in agris relicta* (discesserunt). After a few moments of bewilderment the reader suddenly apprehends von Göler's meaning. He fancied that *occupatis* meant 'having been taken possession of', and was ignorant that *nostris omnibus* in Caesarian Latin could not mean 'all our belongings'; so he persuaded himself that Caesar intended to convey that the Britons, 'having appropriated all our property, which had been left in the fields, made off'! 'The Romans,' he explains, 'had not only not been able to convey into camp the corn which they had cut, but, on account of the surprise, they must even have abandoned their tools for cutting and gathering the corn.'

Comment is needless.

WHERE DID CAESAR ENCOUNTER THE BRITONS ON THE MORNING AFTER HIS SECOND LANDING IN BRITAIN?

'Caesar disembarked the army, and chose a suitable spot for a camp. Having ascertained from prisoners where the enemy's forces were posted, he marched against them about the third watch ... After a night march of about twelve miles he descried the enemy's force. Advancing with their cavalry and chariots from higher ground towards a river,[2] they attempted to check our men, and forced on an action. Beaten off by the cavalry, they fell back into the woods and occupied a well-fortified post of great natural strength, which they had apparently prepared for defence some time before with a view to war with their neighbours; for all the entrances were blocked by felled trees laid close together' (*Caesar exposito exercitu et loco castris idoneo capto, ubi ex captivis cognovit quo in loco hostium copiae consedissent ... de tertia vigilia ad hostes contendit ... ipse noctu progressus milia passuum circiter XII hostium copias conspicatus est. Illi equitatu atque essedis ad flumen progressi ex loco superiore nostros prohibere et proelium committere coeperunt. Repulsi ab equitatu se in silvas abdiderunt locum nacti egregie et natura et opere munitum, quem domestici belli, ut videbatur, causa iam ante praeparaverant: nam crebris arboribus succisis omnes introitus erant praeclusi*[3]).

Such is the description which Caesar gives of his first encounter with the Britons in 54 B.C. The question of the site is closely connected with the question of the place where he landed. I have proved that this place was between Walmer and Sandwich.[4] It is clear, therefore, that Caesar could not have encountered the Britons either at Robertsbridge on the Rother, where Airy believed that he had discovered the battle-field,[5] or at Wye on the Great Stour,

[1] *Gall. Krieg*, 1880, p. 138, n. 4.
[2] *B. G.*, v, 9, §§ 1–5.
[3] See p. 680, *infra*.
[4] See pp. 595–665, *supra*.
[5] *Essays on the Invasion of Britain*, pp. 35–6.

the site adopted by Lewin.[1] It is certain, however, that the battle was fought either on the Great Stour or on the Little Stour.

1. Napoleon the Third[2] asserts that the river mentioned by Caesar was 'unquestionably the Little Stour'; and he maintains that the left bank in the neighbourhood of Barham and Kingston corresponds with the description of the combat. The rising ground on this bank is not, he remarks, too uneven to have prevented war-chariots and cavalry from acting, and, as the text of the *Commentaries* requires, the Britons would have occupied a commanding position (*locus superior*) on the gentle slopes which terminate at the water's edge. We may safely conclude, he adds, from Caesar's narrative that the combat was unimportant, and that his cavalry crossed the river without difficulty.

Lewin[3] dismisses this view with the remark that the Little Stour is 'too insignificant to have been designated by Caesar as a river'; but the Emperor, who had anticipated this objection, replied that Caesar used the same word (*flumen*) to denote the Oze and the Ozerain,—the two rivulets which encompass Mont Auxois in the Côte-d'Or, where Vercingetorix made his final stand.[4] No one, however, who has seen the Little Stour at Kingston, the Oze, and the Ozerain, will admit that the Kentish 'nailbourne' deserves to be treated as respectfully as the two Burgundian streams.

On the 1st of May, 1902, I walked along the Little Stour from Barham to Bekesbourne. There was not so much as a teaspoonful of water in the channel; and a policeman whom I met near Kingston told me that it had been dry for the last five years. On the other hand, an old labourer, who had lived in the valley for sixty-four years, remarked that he could remember a time when the rivulet often overflowed its banks: on the 18th of April of this year (1904) I myself saw it running past Barham with a strong stream; and a porter at Barham station said, 'Yes, and it isn't half as strong as it was a month ago.' Moreover, Bryan Faussett,[5] writing between 1767 and 1773, said that the Little Stour about a mile below Kingston was 'seldom or never dry'; and Philippott,[6] speaking of Bekesbourne, affirms that in the reign of Edward the Third 'there was a small navigation out of the river of Stoure up to this place'.

[1] *The Invasion of Britain*, &c., 1862, pp. 87–8, 90.

[2] *Hist. de Jules César*, ii, 186, note 4.

[3] *The Invasion of Britain*, &c., 1862, p. xciv.

[4] *B. G.*, vii, 69, § 2.

[5] *Inventorium Sepulchrale*, ed. C. Roach Smith, 1856, p. 35. Faussett goes on to say (p. 36) that, 'as a proof of this Aylesbourne [the Little Stour at Kingston] having been much deeper and broader than it ever now is, I myself saw the shells of muscles (*sic*) turned plentifully out of the ground in digging a hole for a post, at the distance of at least ten rods from the present channel, and at the perpendicular height of at least three feet above its usual level.' But this argument is irrelevant. No geologist would deny that the Little Stour, when it was cutting out its channel, was 'broader than it ever now is'. But when? Perhaps at the inconceivably remote epoch when the Thames was depositing gravel at a height of 100 feet above its present level.

[6] *Villare Cantianum*, 1776, p. 62.

There need be no difficulty, then, in believing that the Little Stour, in Caesar's day, was a running stream, which he might perhaps have called a *flumen*, though it must not be forgotten that both in 55 B.C. and in the following year, the summer, at all events in Gaul, was exceptionally dry.[1] Nevertheless, it is certain that the Britons did not encounter Caesar on the Little Stour. Consider the meaning of his words :—*illi equitatu atque essedis ad flumen progressi ex loco superiore nostros prohibere et proelium committere coeperunt.*[2] At one time I thought that this passage meant, 'Advancing towards a river with their cavalry and chariots, they attempted from their commanding position (*ex loco superiore*) to check our men,' &c. : but a passage in the twenty-third chapter of the Second Book of the *Gallic War*—*alia in parte diversae duae legiones, XI et VIII, profligatis Viromanduis, quibuscum erant congressae, ex loco superiore in ipsis fluminis ripis proeliabantur*—in which the words *ex loco superiore* unquestionably belong to *profligatis*, leads me to believe that the former of the alternative translations which I have given—'Advancing with their cavalry and chariots from higher ground towards a river, they attempted to check our men,' &c.—is to be preferred. At all events the *locus superior* was not without tactical significance, and was either the left bank of the stream or high ground in close proximity to the left bank. Now between Barham and the northern end of Charlton Park, which is below Kingston, the depth of the channel of the Little Stour does not exceed 18 inches ; and even at Bekesbourne it is only about two feet. Therefore, unless the depth of the channel was considerably greater in 54 B.C. than it is now, and unless the water flowed considerably below the level of the banks, the words *locus superior* could not have been applied to the bank itself anywhere between Barham and Bekesbourne.[3] Moreover, although there are well-defined heights on the left bank between Barham and Bridge, the lowest slopes, except opposite Kingston and for a very short space on either side of it, are at a considerable distance from the channel. Assuming that Caesar crossed the Little Stour at or near Kingston, the Britons could have opposed him more effectually when he was ascending Barham Downs than by attempting to defend the passage of the rivulet. And, since he would in any case be obliged to cross the Stour itself, is it not obvious that they would have waited for him behind the river which might fairly be called an obstacle rather than on the banks of the streamlet which an active lad could have jumped over ?

2. If Napoleon's view is inadmissible, it is difficult to characterize that adopted by the Reverend Francis Vine.[4] He assures us that Caesar 'descried the British forces . . . lining the crest of the hill (described in the 'Commentaries' as "superior locus") from Garrington (near Littlebourne) on his right hand, to probably the

[1] *B. G.*, iv, 38, § 2 ; v, 24, § 1. [2] *Ib.*, 9, § 3. [3] See Addenda, p. 742.
[4] *Caesar in Kent*, 2nd ed., 1887, pp. 165–8. I should not notice this work if it had not been quoted even by antiquaries of repute, and included by Mr. Gross in his generally valuable bibliography.

part of Barham Downs opposite Bridge and Bishopbourne on his left. This was the best position which the Britons could possibly have chosen for the purpose of arresting the progress of an army marching upon Caer Caint (Canterbury)'. In other words, Mr. Vine holds that the Britons awaited Caesar's approach not on the further but on the nearer side of the river! 'That the Britons were traditionally reported to have opposed Caesar's progress *before he reached* the river, rather than after passing it, may,' he says, 'be inferred from the following passage from Pomponius Sabinus, out of Seneca: "And in the night marching twelve miles up into the country, Caesar finds out the Britons, who *retreated as far as the river*, but gave him battle there."' So we are to prefer the authority of Seneca to that of the general who fought the battle! Besides, Mr. Vine fails to see that the passage which he quotes (and which is not to be found in Seneca) need only mean that the Britons, when Caesar descried them, had retreated from the seashore to the banks of the river. Why Pomponius Sabinus, an Italian scholar who was born 1,470 years after Caesar died, should be summoned as a witness it is not easy to understand. The only inference which can be deduced from Caesar's narrative—the only inference which has ever been deduced from it by any scholar—is that the Britons, when Caesar descried them, were on the further side of the river, and that they advanced to its left bank in order to dispute his passage.

Mr. Vine's view[1] of the route which Caesar followed appears to be partly based upon 'traces of encampments which still remain'. But who made them? Certainly not these Britons, who, only a few hours before Caesar began his night march, had retreated from the coast into the interior in order to oppose his progress, and whose stronghold was not on the right but on the left bank of the river. Certainly not Caesar, who, in marching from the coast to encounter the Britons, made no camp at all. The tumuli which have been opened on Kingston Down were, as Roach Smith says, 'neither more nor less than those of Saxons.'[2] Mr. Vine[3] asserts that 'there were

[1] *Caesar in Kent*, p. 163.

[2] Bryan Faussett, *Inventorium Sepulchrale*, p. 39, n. 2. See also pp. 36, n. 1, 37, 144–59. 'That these tumuli,' says Roach Smith (*ib.*, p. 37), 'were not cast up in consequence of any battle fought on the spot, is evident from . . . their containing the remains not only of men . . . but also of women and children.'

Hasted (*Hist. of Kent*, iii, 1790, p. 752, note *a*) says that 'all the learned agree that *Barham down* was his [Caesar's] main camp, to which from his landing in *the Downs* by *Mongeham*, *Sutton*, *Eythorne*, *Barston*, and *Snowdown*, there is a continual course of military works', &c. (see also vol. iv, 1799, p. 163). But in the time of the 'learned' contemporaries and predecessors of Hasted, it was not yet understood that the question whether this or that mound was a 'military work', and the further question whether it had been constructed by Romans, should be settled not by imagination, but by pick and shovel.

Professor Flinders Petrie (*Archaeol. Cant.*, xiii, 1880, p. 12) remarks that 'the works on Barham Down, half a mile NE. of Kingston, appear to be ancient'; but, being a competent archaeologist, he does not suggest that they were made by Caesar.

[3] *Caesar in Kent*, p. 186. When Mr. Vine (*ib.*, p. 185) gravely appeals to 'the direct statement recorded on the chart found in Dover Castle, that "Caesar,

probably two large oblong castra [constructed by Caesar], the one extending along Barham Downs opposite Charlton, the other at the western extremity of the Downs extending over part of Bridge Hill, Bourne Park, and perhaps the grounds of Higham'. But without excavation it would be impossible to prove that any 'castra' had been erected on Barham Downs by Caesar; and without going to this trouble any man who can understand the *Commentaries* may conclude that he certainly did not construct two.

3. Lyon, the author of the *History of the Town and Port of Dover*,[1] maintains that the combat took place near Littlebourne, about two miles lower down than Bekesbourne; but this place is on the road from Sandwich to Canterbury, and barely nine miles from the former. If Caesar marched along the line of this road, he must have encountered the Britons on the Great Stour near Canterbury. Assuming that he marched from the neighbourhood of Deal, Lyon's view might perhaps be defended if there were any reason to believe that on the left bank of the Little Stour near Littlebourne there was a stronghold;[2] but in a former article[3] I have given cogent reasons for believing that in 54 B.C. Caesar encamped some miles north of Deal.

It may, then, be regarded as morally certain that the river on which the Britons encountered Caesar was the Great Stour. It has, indeed, been objected that the least distance of the Great Stour from Deal is not twelve, but fifteen miles; but while this argument may be valid against the theory that Caesar landed in 54 B.C. at Deal, the position that he defeated the Britons on the Great Stour remains unshaken.

4. The Reverend R. C. Jenkins[4] holds that the scene of the encounter was Chilham, about six miles above Canterbury, on the Ashford road. 'The only obstacle,' he pleads, 'is the increased distance, which is sixteen, instead of twelve miles . . . a difference which the loss of a single stroke might account for [the scribe being supposed to have written XII instead of XVI], if, indeed, it is not sufficiently explained by the possible miscalculation of the time of the march . . . everything else falls into perfect harmony with the narrative—the high wooded ground at the back, the steep banks, the wide and rapid stream,' &c. And again, 'Let us remember that the journey was during the night, when the ground would be rapidly passed over, and the actual distance would be less apparent . . . the space traversed is only described as "milia passuum *circiter* duodecim", and even then the position of the enemy was merely

having landed at Deal, afterwards conquered the Britons on Barham Down "', one can only wonder why he does not also cite a 'direct statement' more ancient even than Camden's 'chart',—the statement of Nennius, that Caesar's second invasion took place three years after the first.

[1] Vol. ii, 1814, p. 9.

[2] Mr. George Payne (*Collectanea Cantiana*, p. 172) speaks of 'a great *oppidum* in Pine Wood, Littlebourne'; but no trace of an entrenchment in this wood is to be found in the 6-Inch Ordnance Map (Sheet 47).

[3] See pp. 664–5, *supra*.

[4] *Proc. Soc. Ant.*, 2nd ser., iii, 1864–7, p. 506.

discerned afar off . . . here we have ancient mounds and earthworks, which give silent testimony to the fact that Chilham was a military position of the highest importance even during the British period.'[1]

These arguments have no weight. When numbers attested by the consensus of the *MSS.* are not manifestly wrong, we have no right to distort them into agreement with our own preferences. The distance, in a straight line, from the nearest point on which Caesar could have encamped, if he had landed near Sandwich,[2] to Chilham, is about 20 Roman miles; from the place where he would have encamped if he had landed near Deal,[3] approximately 16; and the actual distance which he would have had to march is of course considerably longer. Why he should have marched more rapidly by night than by day it would be difficult to explain;[4] and the remark that 'the position of the enemy was merely discerned afar off' is a pure invention. If 'we have ancient mounds and earthworks' at Chilham, they prove nothing about Caesar;[5] and we have them also at other places near the course of the river. Besides, why should Caesar have made a forced march in order to cross the Stour at Chilham when, by making an ordinary march of 12 miles, he could have crossed it near Canterbury?[6]

Our search for the site is now confined within narrow limits. Below Fordwich, the Stour, in Caesar's time, would certainly have been impassable in the face of an enemy; for it flowed through a broad morass.[7] Between Canterbury and the bridge above Sturry the river is virtually flush with its banks. It appears to me, then, that Caesar must have crossed it either between Fordwich and Sturry, or in the neighbourhood of Thanington, just above Canterbury, or possibly at Canterbury itself. Above Canterbury it flows through

[1] *Archaeol. Cant.*, vii, 1868, pp. li–lii.

[2] See p. 674, *supra*.

[3] *Ib.*

[4] 'Night marches,' says Lord Wolseley (*The Soldier's Pocket Book*, 1886, p. 325), 'require at least half as much time again as the same distance would require by daylight.'

[5] No military earthworks exist at Chilham. See *Archaeol. Cant.*, xiii, 1880, pp. 11–2.

[6] It is hardly necessary, I suppose, to mention the argument which various writers, from Camden to Lewin, have based upon the name of the tumulus near Chilham, called 'Julliberrie's Grave'. 'I am almost persuaded,' wrote Camden, 'that Laberius Durus . . . was buried here' (*Britannia*, ed. R. Gough, i, 215). Laberius Durus, as the reader will remember, was the name of the tribune who was killed in the action fought on the day on which Caesar, after he had constructed his naval camp, returned to the neighbourhood of the place where he had defeated the Britons on the day after his second landing (*B. G.*, v, 15, § 5). 'Julliberrie's Grave' is a neolithic long barrow (*Archaeologia*, xlii, 1869, p. 176, note b), and was erected more than a millennium before Laberius Durus was born.

[7] *Archaeol. Journal*, xxxiii, 1876, p. 69. Canon Isaac Taylor (*Words and Places*, 3rd ed., 1873, p. 237) says that 'the name of FORDWICK, the "bay on the arm of the sea", proves that in the time of the Danes the estuary must have extended nearly as far as Canterbury'. Canon Taylor's etymologies are not to be taken upon trust; but, granting his conclusion, it does not follow that the estuary was not fordable at Fordwich, just as the estuary of the Somme was forded near its mouth by the English army before the battle of Crecy.

nearly level meadows: its width is about 15 or 16 yards: the banks are about 2 feet high; and the depth of the water at present is apparently about 2 feet. The bottom at the bridge above Thanington is sandy and gravelly. Opposite this point and at a distance of, say, 600 yards, the ground begins to rise into wooded heights. Opposite Thanington, and east of it, the heights are considerably nearer the river; but they gradually sink as they approach Canterbury.

At Sturry the lower slopes of the low hills which extend along the northern side of the valley approach very close to the river, say to within 100 yards; but opposite Fordwich they are much further away. Just below the mill at Sturry the Stour is from 15 to 20 yards wide: the banks are 3 or 4 feet above the water: the average depth is apparently from about 18 inches to 2 feet; and the bottom is sand mixed with stones. At Fordwich the depth of the water, as seen from the bridge, is about 5 feet: the banks are 4 or 5 feet above the water; and the bottom from this point downwards is mud.

5. The eminent geographer, Major Rennell,[1] believed that Caesar crossed at the place where 'the western road intersects the course of the Stour'; but he gave no reasons for preferring this site to Fordwich, Sturry, or Thanington.

6. Von Göler[2] and Guest[3] maintain that the battle was fought at or near Sturry; and Heller[4] appears to agree with them. So does Roach Smith,[5] who, at Guest's request, made 'a survey of the vicinity of Grove Ferry', which survey, I presume, extended as far up the river as Sturry. 'There,' he says, 'I found the river with the high bank ... the woods and *oppidum*,'[6] &c. Napoleon[7] argues that the banks at Sturry are so steep that the Roman cavalry could not have forced a passage without great difficulty, whereas it would appear from Caesar's account that they crossed easily; and also that Sturry is 15, not 12 miles from Deal. The latter objection may be disposed of at once. Napoleon himself maintains that Caesar's anchorage in 54 B.C. was 'some kilometres' north of the spot where he had landed in the preceding year;[8] and this spot he rightly fixes between Walmer and Deal. Therefore the place

[1] *Archaeologia*, xxi, 1827, p. 505.
[2] *Gall. Krieg*, 1880, p. 148.
[3] *Archaeol. Journal*, xxi, 1864, p. 240. Mr. Malden (*Journal of Philology*, xvii, 1890, p. 168) speaks of Caesar's narrative as 'excluding the mile broad estuary of the greater Stour at Grove Ferry where Dr. Guest placed the battle'. Dr. Guest did no such thing. George Long (*C. J. Caesaris comm. de b. G.*, 1880, p. 226), in a note on Caesar's account of the battle, says of Grove Ferry that 'the locality fits the description'; and Dr. Guest, commenting on Long's note, says (*Origines Celticae*, ii, 1883, pp. 366–7), 'I know of no reason for his fixing it at this place, which appears to me to have hardly one of the necessary requisites.'
[4] *Zeitschrift für allgemeine Erdkunde*, xviii, 1865, pp. 129–30.
[5] *Retrospections*, ii, 15.
[6] No *oppidum* is marked anywhere near Sturry, either on the One-Inch Ordnance Map (Sheets 273 and 289) or on the map which illustrates Mr. George Payne's 'Archaeological Survey of Kent' (*Archaeologia*, li, 1888, facing p. 446).
[7] *Hist. de Jules César*, ii, 186, n. 4.
[8] *Ib.*, n. 2.

from which Caesar descried the Britons just before they advanced to the bank of the stream was not 12 miles from Deal, but, as I have shown in a previous article,[1] from a point in the neighbourhood of Sandwich. Certainly it would have been more difficult to force the passage of the Stour at Sturry than of the Little Stour near Kingston; but, as we have seen, the latter would practically have presented no obstacle at all. Airy,[2] who holds that if the Britons had been posted on the Stour at all, 'Caesar would have crossed at the sound ground of Canterbury or above it,' observes that 'the place had been selected by the Britons as a defensive post at least two days previously, and may therefore be presumed to have had the qualifications necessary for a defensive post, namely that it could not be turned, and that enemies could attack it in front only at a disadvantage'; and, remarking that 'there can scarcely be a doubt that Canterbury existed then as an important town', he adds that 'of this there is no mention in Caesar'. But why should Caesar have mentioned Canterbury? It was not a strategical point: there was nothing to be gained by attacking it except perhaps a little plunder; and anything worth plundering would certainly have been removed into the stronghold which he did attack. 'The place [on the banks of the river] had been selected [or, at all events, occupied] as a defensive post' not two days but one day previously; and, generally speaking, to select a defensive post on a river which cannot be turned is impossible.[3] The stronghold to which the Britons retreated was probably, as Mr. George Payne holds,[4] and as I have shown in my narrative,[5] the British *oppidum* in Bigberry (or Bigbury) woods, about a mile and a half west of Canterbury, of which traces still exist; and it seems most likely that the passage of the river took place at some point between Canterbury and Thanington.

CAESAR'S EARLIER OPERATIONS IN 54 B.C. (*B. G.*, V, 9–11)

Caesar's account of the events which occurred on the day after his first encounter with the Britons in 54 B.C. has been interpreted in several different ways; and yet his narrative is so clear that one would have thought it impossible to misunderstand it. After describing the first encounter, he proceeds, 'Caesar, however, forbade them [the legionaries] to pursue the fugitives far, partly because he had no knowledge of the ground, partly because the day was

[1] See pp. 664–5, *supra*.

[2] *Archaeologia*, xxxiv, 1852, pp. 243–4. See p. 660, *supra*.

[3] Read Clausewitz, *On War* (translated by Col. J. J. Graham, iii, 1873, p. 9; Sir E. B. Hamley, *Operations of War*, 1878, pp. 233–76; Lord Wolseley, *The Soldier's Pocket-Book*, 1886, pp. 393–7; and Gen. Clery, *Minor Tactics*, 12th ed., 1893, pp. 230–5.

[4] *Journ. Brit. Archaeol. Association*, xliv, 1888, pp. 290–1. See also *Archaeol. Cant.*, ix, 1874, pp. 13–5, and *Archaeol. Journal*, lix, 1902, pp. 213–7.

[5] See p. 337, *supra*.

far spent and he wished to have time for entrenching his camp. On the following morning he sent a light division of infantry and cavalry, in three columns, to pursue the fugitives. They had advanced a considerable distance, the rear-guard being still in sight, when some troopers from Quintus Atrius came to Caesar with the news that there had been a great storm on the preceding night, and that almost all the ships had been damaged and gone ashore . . . On receiving this information, Caesar recalled the legions and cavalry, ordering them to defend themselves as they marched, and went back himself to the ships . . . Although it involved great trouble and labour, he decided that the best plan would be to have all the ships hauled up and connected with the camp by one entrenchment. About ten days were spent in these operations, the troops not suspending work even in the night.' (*Sed eos fugientes longius Caesar prosequi vetuit, et quod loci naturam ignorabat, et quod magna parte diei consumpta munitioni castrorum tempus relinqui volebat. Postridie eius diei mane tripertito milites equitesque in expeditionem misit, ut eos qui fugerant persequerentur. His aliquantum itineris progressis, cum iam extremi essent in prospectu, equites a Q. Atrio ad Caesarem venerunt, qui nuntiarent superiore nocte maxima coorta tempestate prope omnes naves adflictas atque in litus eiectas esse . . . His rebus cognitis Caesar legiones equitatumque revocari atque in itinere resistere iubet, ipse ad naves revertitur . . . Ipse, etsi res erat multae operae ac laboris, tamen commodissimum esse statuit omnes naves subduci et cum castris una munitione coniungi. In his rebus circiter dies X consumit ne nocturnis quidem temporibus ad laborem militum intermissis.*[1]

The camp for the construction of which Caesar wished to allow time was of course quite distinct from the one for which, on the previous day, he had selected a site near his landing-place,[2] and was to be in the neighbourhood of the place where he had beaten the enemy, that is to say, twelve Roman miles or more from the sea.[3] Next morning he dispatched three flying columns in pursuit of the fugitives; and his rear-guard was just visible when the messengers arrived with the news of the shipwreck. He sent a galloper to recall the pursuing columns; and, as their retreat would naturally encourage the fugitives to rally and return to the attack, they were to offer the best resistance they could as they marched back to the coast.

C. Schneider[4] misunderstands the passage as far as misunderstanding is possible. He holds that *cum iam extremi essent in prospectu* means 'when the rear *of the enemy* was just in sight'. But Caesar was not with any of the three pursuing columns, for he tells us that he sent them in pursuit: therefore, if Schneider were right, we should be forced to believe that he learned afterwards that the enemy's rear had been just in sight. But why resort to this fanciful explanation, seeing that Caesar could by no possibility have ascertained that the rear of the fugitives was just visible to his troops

[1] *B. G.*, v, 9, § 8; 10; 11, §§ 1, 5–7.
[2] *Ib.*, 9, § 1.
[3] *Ib.*, 9, §§ 2–7.
[4] *Comm. de bellis C. I. Caesaris*, ii, pp. 48–9.

at the very moment when he himself, separated from the troops, received the news of the shipwreck; and, further, that *extremi* must grammatically refer to *his*, that is to say, the pursuing columns? It has been objected that if *extremi* meant the Roman rear-guard, *iam* would not have its proper meaning, 'already' (ἤδη). But *iam* of course often means 'by this time', or 'at length' (nearly *tandem*). In the passage in question its sense may, I think, be illustrated by a clause in *B. G.*, vii, 83, § 7,—*cum iam meridies adpropinquare videretur* ('at length when it was evidently near noon'). Caesar meant to say that his pursuing columns had advanced so far that by the time when the news of the shipwreck reached him the rear-guard only was in sight. Again, Schneider takes the words *in itinere resistere iubet* to mean that Caesar ordered his troops 'to stop in their march and halt where they were'. But, as Long[1] points out, 'if this is so, he returned to his ships himself ("ipse"), leaving his men in the country doing nothing for ten days. But he tells us that his soldiers were employed in making his "munitio", and therefore they must have come back to the camp.'

So far Long is quite right; but he too makes a great mistake. He actually believes[2] that the words *munitioni castrorum tempus relinqui volebat* ('he wished to leave time for entrenching his camp') refer to the camp near the seashore, for which he had selected a site immediately after the disembarkation. In other words, he believes that Caesar compelled his troops, who had already made a night-march of twelve miles, beaten the enemy on the banks of the Stour, stormed the stronghold to which the beaten enemy had retreated, and then pursued the fugitives for some distance, to march back all the way to the camp by the seashore, entrench it, and next morning march back again twelve miles or more into the interior, and then start in pursuit of the fugitives! The legionaries were hardy fellows; but if they did all this, they must have been as breathless as the reader will be when he has got to the end of the preceding sentence. And what had the fugitives been about? If Long is right, they must have got such a start that to pursue them would have been a wild-goose chase indeed. Needless to say, what Caesar meant was simply this:—when the Britons had been dislodged from their stronghold, he would not allow his troops to pursue them far because he wished to leave time for the construction of the temporary camp in which he intended to pass the night.

Von Göler[3] needlessly quarrels with the text of the *Commentaries*, and makes matters worse by an absurd emendation. 'The reading *legiones equitatumque revocari atque in itinere resistere iubet* (*B. G.*, v, 11, § 1),' he says, 'is obviously corrupt. *Atque* requires that the following clause should involve a climax' (*Steigerung*). But so it does:—Caesar 'orders the legions and cavalry to be recalled, and [not only to come back but also] to defend themselves upon the march'. In this literal translation a climax is as evident as in the

[1] *C. J. Caesaris comm. de b. G.*, 1880, p. 228. [2] *Ib.*, p. 227.
[3] *Gall. Krieg*, 1880, p. 150, n. 2.

emendation, to be quoted presently, which von Göler offers ; and any one who is not familiar with the *Commentaries* will find in Meusel's *Lexicon Caesarianum* abundant evidence that *atque* in the passage which von Göler brands as corrupt is in accord with Caesar's *usus loquendi*.

'As finally,' continues von Göler, 'one may conclude with certainty from the later substance of the narrative that all the legions returned to the shore, but the necessary express mention of the march thither is nowhere to be found in the *Commentaries*, I believe that the passage originally ran as follows :—*legiones equitatumque revocari atque in itinere persistere iubet ad naves, ipse revertitur*.'

If we may 'conclude with certainty' that 'the legions returned', why should the 'express mention' of their return be necessary ? Heller[1] has taken the trouble to show that the text is above suspicion.

CAESAR'S SECOND COMBAT WITH THE BRITONS IN 54 B.C.

There is a passage in Caesar's account of his second combat with the Britons in 54 B.C. which has greatly exercised the minds of the commentators ; and editors have put upon it an interpretation which soldiers will not accept. The passage runs as follows :—'Throughout this peculiar combat, which was fought in full view of every one and actually in front of the camp, it was clear that our infantry, owing to the weight of their armour, were ill fitted to engage an enemy of this kind ; for they could not pursue him when he retreated, and they dared not abandon their regular formation : it was clear too that the cavalry fought at great risk, because the enemy generally fell back on purpose, and, after drawing our men a little distance away from the legions, leaped down from their chariots and fought on foot with the odds in their favour. [On the other hand, the mode in which their cavalry fought exposed the Romans, alike in retreat and in pursuit, to an exactly similar danger.[2]]' (*Toto hoc in genere pugnae cum sub oculis omnium ac pro castris dimicaretur, intellectum est nostros propter gravitatem armorum, quod neque insequi cedentes possent neque ab signis discedere auderent, minus aptos esse ad huius generis hostem, equites autem magno cum periculo proelio dimicare, propterea quod illi etiam consulto plerumque cederent et, cum paulum ab legionibus nostros removissent, ex essedis desilirent et pedibus dispari proelio contenderent.* [*Equestris autem proelii ratio et cedentibus et insequentibus par atque idem periculum inferebat*[3]]). It is the last sentence which has caused all the trouble.

1. 'After describing the difficulties of the infantry,' says Long,[4]

[1] *Philologus*, xxxi, 1872, pp. 536–7.

[2] Or 'exposed the Romans to the same danger, whether they retreated or pursued'. See pp. 690–1, *infra*.

[3] *B. G.*, v, 16, §§ 1–3.

[4] *C. J. Caesaris comm. de b. G.*, 1880, pp. 234–5.

'Caesar explains the danger to which the cavalry was exposed, when they pursued the "essedarii", for the Britanni quitted their "esseda" and fought on foot among the Roman "equites" . . . This was an unequal kind of fighting ("dispar proelium") for the Roman "equites". Caesar adds, that on the other hand ("autem") the British mode of fighting from the "esseda" ("equestris proelii ratio") was equally dangerous to his cavalry and legions in the pursuit and the retreat. This is Schneider's explanation, and I believe that it is right. The Britanni had no cavalry: they had only "essedarii", to whom Caesar (iv, 33) applies the term "perequitant". It follows that "equites hostium essedariique" (c. 15) are no more than the "essedarii" (iv, 24) [in other words that Caesar, who was not a prolix writer, used four words when one would have sufficed.] In iv, 34, however, Caesar says, "peditatus equitatusque." ' Yes, he does; and the reader will please note the significance of this admission.

The assumption upon which Long's explanation rests, namely, that 'the Britanni had no cavalry', is unsupported by any evidence, and is wholly inadmissible. Caesar says that in his first campaign in Britain a party of Roman foragers were surrounded by *equitatu atque essedis*.[1] If these words do not mean 'cavalry *and* chariots', what do they mean? Warriors on horseback are depicted on ancient British coins;[2] and the Britons certainly had cavalry as well as chariots in A.D. 61[3] and in the time of Agricola.[4] Besides, as von Göler points out,[5] the charioteers could not have fought with effect unless they had been supported by cavalry. And when Long says that, according to Caesar, 'the British mode of fighting from the "esseda" . . . was equally dangerous to his cavalry and legions in the pursuit and the retreat,' his explanation refutes itself. Caesar himself says that the legions could not pursue the charioteers; and, on the other hand, it is evident that they were not pursued by the charioteers.

2. According to von Göler,[6] 'by *cedentibus et insequentibus* the chariot-fighters only can be meant' (*Unter den* 'cedentibus et insequentibus' *können nur die Wagenstreiter verstanden werden*); and, placing a comma after *par*, he translates the passage thus:— 'The mode of fighting of the [British] cavalry corresponded with that of their charioteers, whether they retreated or pursued, and brought the Roman cavalry into precisely the same danger' (*Der Gefechtsmechanismus der* [*britischen*] *Reiterei war aber der Fechtweise ihrer Wagenstreiter, ob sie wichen oder verfolgten, entsprechend* [par] *und brachte die römische Reiterei gerade in jenes nachtheilige Verhältniss*[7]). But no Latin scholar would admit that a comma could be placed after *par*, which would of course make it necessary to supply the word *erat*.

[1] *B. G.*, iv, 32, § 5.
[2] J. Evans, *Coins of the Anc. Britons*, pp. 234, 239, 271–2; *ib.*, Suppl., pp. 520, 535–6.
[3] Dion Cassius, lxii, 12, § 3.
[4] Tacitus, *Agricola*, 36.
[5] See p. 677, *supra*.
[6] *Gall. Krieg*, 1880, p. 154, note.
[7] *Ib.*, p. 153.

3. According to Dittenberger-Kraner,[1] the words *equestris autem proelii ratio*, if they are genuine, can only mean, in opposition to *ex essedis . . . contenderent*, the mode of fighting of the charioteers, which, while it was dangerous to the Romans when they pursued, was no less dangerous to them when they retreated. I confess that I cannot understand this comment.

4. Doberenz-Dinter[2] also insist upon the opposition between *equestris autem proelii ratio* and *ex essedis . . . contenderent*, and maintain that the former refers to 'a regular cavalry combat (on the part of the Romans)',—*ein regelrechtes Reitertreffen (von Seiten der Römer)*. Now in the passage which ends with the words *ex essedis . . . contenderent* Caesar describes a combat between the Roman cavalry and the British charioteers, who were supported by cavalry. Therefore the editors (if I have succeeded in grasping their meaning) suppose that the 'regular cavalry combat' to which they allude was fought between the Roman cavalry and the British cavalry alone. This must, I think, be what they intend to convey; for, according to Mr. Peskett,[3] who may have followed them, 'Caesar means that when the British and Roman cavalry were engaged, the danger was equalized [*equestris autem proelii ratio et cedentibus et insequentibus par atque idem periculum inferebat*], whereas when they used chariots the Britons were at an advantage.' But if Caesar simply meant that the British cavalry and the Roman in the (hypothetical) 'regular cavalry combat' were each exposed to the same danger, what is the point of the words *et cedentibus et insequentibus*? Surely retreating cavalry are in greater danger than the cavalry which has forced them to retreat and is pursuing them! I do not see how Mr. Peskett's explanation can be got out of the Latin. And who will believe that the Britons would have used their cavalry alone when, by associating them with chariots, they 'were at an advantage'?

5. It might possibly be suggested that the words *equestris . . . inferebat* refer to a cavalry combat between the Romans and the Britons, distinct from the combat between Roman cavalry and the British charioteers, in which the British cavalry, like that of the Germans,[4] were associated with light infantry. But there is no evidence that the Britons had cavalry of this kind; and, as we have seen, it is certain that their cavalry acted in support of their charioteers.

6. Köchly and Rüstow[5] offer the following explanation:—'As the enemy were also supported by their cavalry, our men [the Romans] were exposed to the same danger, whether they advanced or retreated' (*Da aber der Feind auch die Unterstützung seiner*

[1] *C. I. Caesaris comm. de b. G.*, 1890, p. 205.
[2] *C. I. Caesaris b. G. libri VII*, II. Heft, 1890, p. 48.
[3] *Gai Iuli Caesaris de b. G. comm.*, iv, v, 1887, p. 90.
[4] *B. G.*, i, 48, §§ 4–7.
[5] Quoted by von Göler (*Gall. Krieg*, 1880, p. 154). He does not give the reference, and I have failed to discover it.

Reiterei hatte, so war für die unsrige die Gefahr immer dieselbe, mochte sie vorgehen oder zurückgehen).

7. Napoleon the Third[1] explains the matter thus:—'Un désavantage plus grand encore existait pour les cavaliers. Les Bretons, par une fuite simulée, les attiraient loin des légions, et alors, sautant à bas de leurs chars, engageaient à pied une lutte inégale; car, toujours soutenus par leur cavalerie, ils étaient aussi dangereux dans l'attaque que dans la défense.' These words, which appear to be virtually identical in sense with those of Köchly and Rüstow, undoubtedly give an accurate account of what took place. Apparently Köchly, Rüstow, and Napoleon do not take *par atque idem periculum* as meaning 'an exactly similar danger' (to that which Caesar described in the preceding sentence, *Toto hoc . . . contenderent*), but as meaning that the danger which beset the Romans was the same whether they pursued or retreated. Now the Roman infantry, as Caesar says, did not pursue; obviously therefore *et cedentibus et insequentibus* can only refer to the Roman cavalry. I suppose then that what Köchly, Rüstow, and Napoleon meant was this:—if the Roman cavalry pursued the Britons, they were attacked by the charioteers, who jumped off their cars and fought as infantry: as soon as they retreated they were pursued by the British cavalry, and if they turned to bay the charioteers had time to mount their cars again, come up, and engage them anew. If this was what Caesar meant by *equestris . . . inferebat*, his language was not lucid.

The words *equestris proelii ratio*, if they were really written by Caesar, must refer either to a combat between the Roman and the British cavalry or to a combat between the Roman cavalry and the combined British charioteers and cavalry. There is, as we have seen, no reason to suppose that a purely cavalry combat took place; and if it did, the idea that the Roman cavalry was as much in danger when it pursued as when it retreated is absurd. If we accept the other alternative, the meaning of the passage must be either (as Köchly, Rüstow, and Napoleon explain) '*On the other hand*, the mode in which the British *cavalry* fought (in co-operation with the charioteers) exposed the Romans, alike in retreat and in pursuit, to exactly the same danger'; or '*In fact* the nature of the combat of horse [that is to say, the combat between the Roman cavalry and the combined British charioteers and cavalry] exposed the Romans', &c. In the former case *autem* would be an adversative, in the latter merely a connecting particle. The passage is not in the *editio princeps* of the *Commentaries*, and is bracketed in Meusel's edition; and perhaps it is an interpolation.

[1] *Hist. de Jules César*, ii, 188.

THE COMBAT BETWEEN TREBONIUS AND THE BRITONS

'At midday Caesar having sent three legions and all his cavalry on a foraging expedition under one of his generals, Gaius Trebonius, they [the enemy] suddenly swooped down from all points on the foragers, not hesitating to attack the ordered ranks of the legions' (*Sed meridie cum Caesar pabulandi causa tres legiones atque omnem equitatum cum C. Trebonio legato misisset, repente ex omnibus partibus* [hostes] *ad pabulatores advolaverunt, sic uti ab signis legionibusque non absisterent*[1]). To a plain man these words are perfectly intelligible; and no military commentator, so far as I know, has ever found any difficulty in them: but Kraner[2] must needs rewrite the last clause. This is what he makes of it:—*sicubi ab signis legionibusque absisterent.* So, according to Kraner, the enemy attacked the foragers at every point where they were separated from the legions. The unpractical fellow fails to perceive that, as the foragers could not forage while they were in their ranks, there was no point where they were not separated from the legions. The legions, or rather a due proportion of the cohorts which composed them, were there to protect the foragers; and of course what Caesar means is that the enemy, flushed with their easy success in driving off the foragers and compelling them to rejoin their respective cohorts, had the temerity to attack the cohorts themselves.

WHERE DID CAESAR CROSS THE THAMES?

The only indications which Caesar gives as to the place where he crossed the Thames are these. At an early stage of his narrative he tells us that 'the chief command and the general direction of the campaign had been entrusted by common consent to Cassivellaunus, whose territories are separated from those of the maritime tribes by a river called the Thames, about eighty miles [or seventy-three English miles] from the sea' (*summa imperii bellique administrandi communi consilio permissa Cassivellauno, cuius fines a maritimis civitatibus flumen dividit, quod appellatur Tamesis, a mari circiter milia passuum LXXX*[3]). In a later chapter he describes his passage of the river. 'Having ascertained the enemy's plans, Caesar led his army to the Thames, into the territories of Cassivellaunus. The river can only be forded at one spot, and there with difficulty. On reaching this place, he observed that the enemy were drawn up in

[1] *B. G.*, v, 17, § 2.

[2] *C. I. Caesaris comm. de b. G.*, 1890, p. 205.

[3] *B. G.*, v, 11, § 8.

great force near the opposite bank of the river. The bank was fenced by sharp stakes planted along its edge; and similar stakes were fixed under water, and concealed by the river. Having learned these facts from prisoners and deserters, Caesar sent his cavalry on in front, and ordered the legions to follow them speedily; but the men advanced with such swiftness and dash, though they only had their heads above water, that the enemy, unable to withstand the combined onset of infantry and cavalry, quitted the bank and fled.' (*Caesar, cognito consilio eorum, ad flumen Tamesim in fines Cassivellauni exercitum duxit; quod flumen uno omnino loco pedibus, atque hoc aegre, transiri potest. Eo cum venisset, animadvertit ad alteram fluminis ripam magnas esse copias hostium instructas. Ripa autem erat acutis sudibus praefixisque munita, eiusdemque generis sub aqua defixae sudes flumine tegebantur. Eis rebus cognitis a captivis perfugisque, Caesar praemisso equitatu confestim legiones subsequi iussit. Sed ea celeritate atque eo impetu milites ierunt, cum capite solo ex aqua extarent, ut hostes impetum legionum atque equitum sustinere non possent ripasque dimitterent ac se fugae mandarent*).[1]

When Caesar says that the Thames was only fordable at one spot, he evidently means that there was only one ford available for his purpose, that is to say, only one by which he could cross the river into the territories of Cassivellaunus. When he says that these territories were separated from those of the maritime tribes by the Thames, about 80 miles from the sea, he means, I suppose, that it was 80 miles from the place where he landed to the eastern frontier of Cassivellaunus; but, according to some commentators, he reckoned the distance from the place where he landed to the point at which he crossed the river. Heller[2] insists that his statement of the distance was based upon hearsay: I am inclined to believe that it was a rough estimate based upon the number of the marches which he made.

1. The view which has gained most adherents is that Caesar crossed the Thames at 'Coway Stakes', about a furlong west of Walton Bridge. It has been said that this view is based upon a 'tradition which has certainly prevailed for many ages';[3] but I can find no evidence that the tradition existed before the publication of Camden's *Britannia*; and I believe that it was created by him. Camden[4] referred, in support of his conjecture, to a well-

[1] *Ib.*, v, 18.

[2] *Philologus*, xxii, 1865, p. 310. 'Es wird öfter behauptet, dass er selbst durch seinen marsch vom landungsplatz bis zur Themse die breite des landes gemessen habe; zu einer solchen voraussetzung geben seine worte keine veranlassung [naturally! he would not have taken the trouble to indicate the grounds upon which he based his estimate]: er berichtet hier, wie an andern orten, nur was er von andern erfahren hat.' Heller seems to forget that this conclusion also is not authorized by Caesar's words. If Caesar had formed his estimate from hearsay, he, or his interpreter, would have had to reduce the terms in which the estimate of his native informant was expressed to Roman miles.

[3] O. Manning and W. Bray, *Hist. and Ant. of . . . Surrey*, ii, 1809, p. 759.

[4] *Britannia*, ed. R. Gough, 1789, i, 168.

known passage in Bede ;[1] but Bede did not mention Coway Stakes at all. He merely said that, at the point of the river where the enemy had planted their stakes, the stakes were still to be seen, and that they were as thick as a man's thigh and cased with lead. S. Gale[2] affirms, in support of Camden's view, that there is 'a large Roman encampment . . . about a mile and a half distant from the ford'. This camp, which is on St. George's Hill, is really about two miles and three-quarters[3] from the place where the ford is assumed to have been. It is not Roman, but British ; but even if it were Roman, the fact would throw no light upon the question which we are discussing unless the camp could be proved to have been constructed by Caesar. Daines Barrington, an antiquary of the eighteenth century, made an attempt, which, at first sight, would appear completely successful, to demolish Camden's theory.[4] A fisherman of Shepperton, he tells us, rowed him across the river in the line along which the stakes had been planted. He illustrated his story by a sketch-plan, which shows that the stakes were at right angles with the river-banks ; and, he argues, 'such stakes could not possibly have obstructed the passage of an army.' In reply it has been asserted that 'the line of the ford was not transversely straight across the stream, but formed a curve nearly in a semi-circle, so that . . . the stakes must have twice intercepted the passage' ;[5] but who will believe that Caesar would have attempted the passage of the river, which, as he tells us, was barely practicable, by a ford so intricate as this ? A more effective answer might be based upon the fact, attested by Lord Wolseley,[6] that fords 'almost always run diagonally across the river' : if the ford by which Caesar crossed was no exception, stakes planted in the direction indicated by Barrington would obviously have obstructed the passage. Dr. Guest has made another and most ingenious attempt to remove the difficulty.[7] He believes that the stakes which impeded Caesar's advance had not been planted for the purpose of stopping him, but had existed for many years. 'I think,' he says, 'the stakes formed part of what may be called a fortified ford, and were distributed so as to stop all transit . . . save along a narrow passage, which would bring the passenger directly under the command of the watch, stationed on the northern bank to . . . receive the toll. The shallow at Coway was probably of considerable extent, and through its whole length must have extended the line of stakes which Caesar observed on the

[1] *Hist. eccl.*, lib. i, cap. ii (ed. C. Plummer, 1896).—In huius ulteriore ripa Cassobellauno duce inmensa hostium multitudo consederat, ripamque fluminis ac pene totum sub aqua uadum acutissimis sudibus praestruxerat ; quarum uestigia sudium ibidem usque hodie uisuntur, et uidetur inspectantibus, quod singulae earum ad modum humani femoris grossae, et circumfusae plumbo in-mobiliter erant in profundum fluminis infixae, &c.

[2] *Archaeologia*, i, 1770, p. 188.

[3] *One Inch Ordnance Survey*, Sheet 269.

[4] *Archaeologia*, ii, 1773, pp. 143–53.

[5] E. W. Brayley, *Topographical Hist. of Surrey*, ii, 1841, p. 344, n. 29.

[6] *The Soldier's Pocket-Book*, 1886, p. 312.

[7] *Origines Celticae*, ii, 384–5, 388, 391–2.

northern bank. But there must also have been two other lines of stakes across the river to . . . define the passage.'[1] The stakes to which Barrington referred were, in Guest's opinion, the remains of these. 'The remaining portion,' he continues, 'of the shallow was, no doubt, covered with the short stakes that were "concealed by the river" . . . that such was really the disposition of the stakes may, I think, be gathered, not only from the reports of the fishermen, but also from Caesar's narrative. When he saw the Britons ranged along the northern bank with the stakes in front of them, he ordered the cavalry to pass the river, and the legions to follow them. How could either cavalry or infantry cross the river if the stakes were ranged as our antiquaries assume them to have been? . . . Besides, what were the Britons doing while the Roman soldiers were removing the stakes? . . . Caesar says not a word about taking the Britons in flank, nor about removing the stakes.'[2]

It may, perhaps, be thought that Guest has succeeded in showing that the particular objection which Barrington raised against Camden's theory is not necessarily insuperable. If, however, the stakes to which Barrington referred were the remains of those which had served, on Guest's theory, to 'define the passage', the ford in question was an exception to the rule that fords 'almost always run diagonally across the river'. Again, Guest argues that because 'Caesar says not a word about . . . removing the stakes', therefore the Romans did not remove them. But it is needless to insist that they must have removed them, unless at the particular part of the 'shallow' by which they crossed there were none to remove. Guest, therefore, is obliged to make the incredible assumption that the Britons, having left intact the stakes which 'defined the passage', and having thus pointed out to the Romans the best way of crossing the river, were so obliging as to plant no stakes to bar this passage either in the bed of the river or on the bank, while they planted them everywhere else.

Guest undertakes to prove not only that Coway Stakes may, but that it must have been the spot at which Caesar crossed the Thames. 'From the Coway Stakes,' he says, 'the ground rises gradually for about three miles, and then dips almost precipitously into the valley of the Wey. On the top of the hill [St. George's Hill] is an ancient British stronghold which commands the whole valley, and as the valley certainly belonged to the Atrebates, I infer that . . . this people constructed the fortress. Aubrey tells us that "a trench" went from this fortress to Walton, and gave that village its name. A dyke still runs from the ramparts towards Walton. I have traced it for more than one-third of the distance, and I have no doubt that it once reached the village . . . *The ditch is towards the river.* For what purpose could this dyke have been made? The only object for which I can conceive it was made, was to bar progress along the trackway which led from the Coway Stakes eastward to the maritime states. If such were its object, we have another strong

[1] *Origines Celticae*, ii, 384. [2] *Ib.*, pp. 384–5.

proof that the great means of access to the country of Cassivellaunus was at the spot where Camden placed it.'[1] Nor is this all. 'From Hurleyford,' says Guest, 'to the sea, a distance of nearly 100 miles, taking into account the windings of the river, there is but one place on the banks of the Thames bearing a name which indicates a ford over it.[2] This solitary place is Halliford, at the Coway Stakes. Caesar says there was but one ford on the Thames—meaning, of course, the lower Thames, with which alone he was acquainted, and we now have but one place on its banks, the name of which points to the existence of a ford. Our topography is in perfect agreement with his statement; and, to my mind, this coincidence is almost decisive of the question.'[3] We shall see.

2. Colonel Stoffel, by whose advice Napoleon was mainly guided in his attempts to solve the topographical problems presented by the *Commentaries*, was informed by the Thames boatmen whom he consulted that between Shepperton and London there were eight or nine fords, the most favourable of which was at Sunbury; and here accordingly the Emperor concluded that Caesar had crossed the river. Still he (or the colonel) was sagacious enough to doubt. 'La seule chose,' he wrote, 'qui nous paraisse évidente, c'est que l'armée romaine n'a point passé en aval de Teddington.'[4]

But even this conclusion rested upon a rotten foundation. The Emperor, as Guest[5] observes, 'reasons from the present to the past without taking any note of the changes that have occurred during 2,000 years. In the time of Caesar the river ran . . . to the sea uninterruptedly. *Now*, from Teddington westward it is a canal, crossed every two or three miles by weirs and locks . . . as the lock [at Teddington] did not exist in the time of Caesar, any inference drawn from the fact that the tide now ends there, is beside the question.' And, to quote Guest again, 'the shallow at Sunbury is a mere consequence of Sunbury weir. Remove the weir, and Caesar's ford at Sunbury would be swept away in a twelvemonth by the natural scour of the river.' Guest[6] thought it probable that 'when the river was in its natural state, spring-tides ran up the river eight or nine miles further [than they do now]—in other words, to Coway: and that the deposit which they now leave at Teddington then contributed to form the shallow over which Caesar passed.' Mr. F. C. J. Spurrell, on the other hand, has given reasons for believing that 'the estuary did not reach so far west as at the present day'.[7]

3. Von Göler[8] peremptorily decides that the ford was 'undoubtedly at Kingston'. An English engineer, he tells us, informed him that the depth of the water there was only from 3½ to 4½ feet;

[1] *Origines Celticae*, ii, 391–2.
[2] Hurleyford is about 2½ miles west of Great Marlow.
[3] *Origines Celticae*, ii, 388.
[4] *Hist. de Jules César*, ii, 191, n. 2.
[5] *Origines Celticae*, ii, 386–7.
[6] *Ib.*, p. 387.
[7] *Archaeol. Journal*, xlii, 1885, pp. 269–302; xlvi, 1889, pp. 75–6; xlvii, 1890, pp. 43–7, 170; *Proc. Geologists' Association*, xi, 1891, p. 224.
[8] *Caesars gall. Krieg*, 1880, p. 155, n. 2.

and he remarks further that Kingston is just 80 Roman miles from the sea. If the engineer whom he consulted could have informed him that the depth of the water at Kingston was the same in 54 B.C. as at the time when he sounded it, his information would have been more valuable. Assuming that the tide in Caesar's time, as now, did not flow beyond Teddington, W. H. Black may have been justified in saying that Kingston 'presents geographically the most favourable place for crossing the Thames' to an invader coming from the neighbourhood of Deal.[1] But we must not assume that Caesar crossed the Thames at the place which was 'geographically the most favourable': he could not pick and choose; he had to cross where there was a ford, and there was only one.

It has been shown that the Thames has, at various times and at certain states of the tide, been fordable at Westminster,[2] at Chelsea,[3] at Old Brentford,[4] and at Petersham;[5] and the claims of these places have been advocated by zealous antiquaries: but, with one exception, which I shall notice presently, I do not recommend students of the *Commentaries* to read what they have written. As Guest points out, 'the name of Brentford had no reference to a ford over the Thames; it certainly designated the ford over the Brent by which the Roman road from London to Staines crossed the latter river.'[6] Moreover, the fact that the Thames has occasionally been crossed on foot at various points near London proves nothing: these cases, as Guest truly says, were exceptional, and were recorded because they were exceptional; 'Caesar,' he concludes, 'knew the river in its natural state, and had . . . adequate means of acquiring knowledge . . . he tells us distinctly that the Thames was passable on foot only in one place.'[7]

The claims of Brentford have, however, recently been advocated by Mr. Montagu Sharpe in a pamphlet which contains some real evidence. From information supplied by Messrs. W. S. Bunting and W. Benell of the Thames Conservancy, and by Conservancy Inspector G. J. Rough, he shows that a line of stakes, of which some still remain 'for about 400 yards below Isleworth Ferry,' extended thirty years ago for about a mile up the river from 'Old England', opposite the mouth of the Brent; and that 'no other ancient stakes have been discovered in the lower river during dredging operations'.[8]

Except Coway Stakes and Brentford, there is no spot in the Thames valley for the identification of which with the scene of Caesar's exploit a shred of real argument has been advanced.

[1] *Archaeologia*, xl, 1866, pp. 51–2.
[2] *Gentleman's Magazine*, xxvi, 1846, pp. 256–7.
[3] W. Maitland, *Hist. of London*, i, 1756, p. 8; *Journ. Brit. Archaeol. Association*, N. S., iii, 1897, p. 102.
[4] *Ib.*, xvi, 1860, p. 135; Camden's *Britannia*, ed. Edmund Gibson, i, 1772, p. 329.
[5] Manning's *Surrey*, ii, 760.
[6] *Origines Celticae*, ii, 388.
[7] *Ib.*, pp. 404–5.
[8] *Bregant-forda and the Hanweal*, 1904, pp. 1, 22–7. Mr. Sharpe reasonably suggests that Bede referred not to the Coway but to the Brentford stakes.

Guest's arguments are of very unequal value; but the one which he founds upon the name 'Halliford' is worth considering, though it would be going too far to say that it is 'almost decisive of the question'. The claim which Mr. Sharpe makes for his own discovery rests upon somewhat better grounds. [See Addenda, p. 742.]

CAESAR'S PASSAGE OF THE THAMES

The excessively laconic chapter in which Caesar describes how he crossed the Thames in the face of a British force seems at first sight hard to explain. He tells us that 'the river can only be forded at one spot, and there with difficulty'. 'On reaching this place,' he continues, 'he observed that the enemy were drawn up in great force near the opposite bank of the river. The bank was fenced by sharp stakes planted along its edge; and similar stakes were fixed under water and concealed by the river. Having learned these facts from prisoners and deserters, Caesar sent his cavalry on in front, and ordered the legions to follow them speedily; but the men advanced with such swiftness and dash, though they only had their heads above water, that the enemy, unable to withstand the combined onset of infantry and cavalry, quitted the bank and fled' (*quod flumen uno omnino loco pedibus, atque hoc aegre, transiri potest. Eo cum venisset, animadvertit ad alteram fluminis ripam magnas esse copias hostium instructas. Ripa autem erat acutis sudibus praefixisque munita, eiusdemque generis sub aqua defixae sudes flumine tegebantur. His rebus cognitis a captivis perfugisque Caesar praemisso equitatu confestim legiones subsequi iussit. Sed ea celeritate atque eo impetu milites ierunt, cum capite solo ex aqua extarent, ut hostes impetum legionum atque equitum sustinere non possent ripasque dimitterent ac se fugae mandarent*).[1]

Von Göler[2] explains the passage as follows:—'he first sent his cavalry against the enemy, making them swim across the adjacent part of the river, which, though deeper, was not barricaded, and by their attack so effectually protected the passage of the infantry, which was begun immediately afterwards, that they gained time to remove the stakes'; and in a note[3] he says, 'Only in this way can the expression *praemisso equitatu confestim legiones subsequi iussit* be understood, for the cavalry could by no possibility swim across or disregard the stakes or the palisade.' This explanation, as far as it goes, is substantially identical with that of Turpin de Crissé,[4] who supposes, further, that while part of the Roman infantry cut down the stakes in the river, they were supported by their comrades in the rear, who discharged missiles against the enemy, and, he might have added, by the auxiliary troops,—slingers and archers. Napoleon the Third[5] hardly differs from his predecessors

[1] *B. G.*, v, 18. [2] *Gall. Krieg*, 1880, p. 155. [3] *Ib.*, n. 2.
[4] *Comm. de César*, i, 1785, p. 334. [5] *Hist. de Jules César*, ii, 191-2.

in suggesting that the cavalry were sent '*à une certaine distance* en amont ou en aval'. Mr. A. G. Peskett[1] objects to Napoleon's explanation on the ground that it involves a mistranslation of the word *praemittit*, which, he insists, 'must mean that Caesar sent the cavalry across the river, ordering the infantry to follow them.' Mr. Peskett evidently means that the infantry crossed directly in the rear of the cavalry. But, as any soldier would tell Mr. Peskett, the operation which he supposes would have been absolutely impossible; and, moreover, his rigidly literal interpretation of the word *praemisso*, which is not shared by that competent Caesarian scholar, C. Schneider, is irreconcilable with the word *sed*, which opens the next sentence. This word, as Schneider[2] remarks, is intended to show that the infantry, in their ardour, outvied the cavalry, and crossed the river before them. The use of the word *praemisso* may surely be defended if the cavalry were sent into the water before the infantry. Similarly it will be evident to any one who carefully reads the last two sentences in the twenty-fifth chapter of Caesar's Fourth Book[3] that the word *subsecuti* cannot there mean 'following directly in rear'. For in that case 'the nearest ships' (*proximis navibus*), the troops in which jumped into the sea and followed (*subsecuti*) their comrades, would have been drawn up in a second line *behind* the other ships. If so, being in deeper water, they evidently could not have been run aground; and the soldiers who descended from them would have been drowned.

THE SITE OF CASSIVELLAUNUS'S STRONGHOLD

The indications which Caesar gives as to the geographical position of the stronghold of Cassivellaunus are of the vaguest kind. After describing his passage from the southern to the northern bank of the Thames, which brought him into the territory of Cassivellaunus, he gives the following account of his operations:—

'Cassivellaunus, abandoning, as we have remarked above,[4] all thoughts of regular combat, disbanded all his forces, except some four thousand charioteers, watched our line of march, and, moving a little away from the track, concealed himself in impenetrable wooded spots, and removed the cattle and inhabitants from the open country into the woods in those districts through which he had learned that we intended to march. Whenever our cavalry made a bold dash into the country to plunder and devastate, he sent his

[1] *Gai Iuli Caesaris de b. G. comm.*, iv, v, 1887, p. 92.

[2] *Comm. de bellis C. I. Caesaris*, ii, 1849, p. 80.

[3] Tum nostri cohortati inter se, ne tantum dedecus admitteretur, universi ex navi desiluerunt. Hos item ex proximis navibus cum conspexissent, subsecuti hostibus adpropinquaverunt.

[4] See *B. G.*, v, 17, § 5.

charioteers out of the woods (for he was familiar with every track and path), engaged our cavalry to their great peril, and by the terror which he thus inspired prevented them from moving far afield. Caesar had now no choice but to forbid them to move out of touch with the column of infantry, and, by ravaging the country and burning villages, to injure the enemy as far as the legionaries' powers of endurance would allow.

'Meanwhile the Trinovantes—about the strongest tribe in that part of the country—sent envoys to Caesar, promising to surrender and obey his commands. Mandubracius, a young chief of this tribe, whose father had been their king and had been put to death by Cassivellaunus, but who had saved his own life by flight, had gone to the Continent to join Caesar, and thrown himself upon his protection. The Trinovantes begged Caesar to protect Mandubracius from harm at the hands of Cassivellaunus and to send him to rule over his own people with full powers. Caesar sent Mandubracius, but ordered them to furnish forty hostages and grain for his army. They promptly obeyed his commands, sending hostages to the number required and also the grain.

'As the Trinovantes had been granted protection and immunity from all injury on the part of the soldiers, the Cenimagni, Segontiaci, Ancalites, Bibroci, and Cassi sent embassies to Caesar and surrendered. He learned from the envoys that the stronghold of Cassivellaunus, which was protected by woods and marshes, was not far off, and that a considerable number of men and of cattle had assembled in it. The Britons apply the name of stronghold to any woodland spot, difficult of access and fortified with a rampart and trench, to which they are in the habit of resorting in order to escape a hostile raid. Caesar marched to the spot indicated with his legions, and found that the place was of great natural strength and well fortified: nevertheless he proceeded to assault it on two sides. The enemy stood their ground a short time, but could not sustain the onset of our infantry, and fled precipitately from another part of the stronghold.' (*Cassivellaunus, ut supra demonstravimus, omni deposita spe contentionis, dimissis amplioribus copiis, milibus circiter IIII essedariorum relictis, itinera nostra servabat paulumque ex via excedebat locisque impeditis ac silvestribus sese occultabat, atque iis regionibus quibus nos iter facturos cognoverat pecora atque homines ex agris in silvas compellebat et, cum equitatus noster liberius praedandi vastandique causa se in agros eiecerat, omnibus viis notis semitisque essedarios ex silvis emittebat et magno cum periculo nostrorum equitum cum iis confligebat atque hoc metu latius vagari prohibebat. Relinquebatur ut neque longius ab agmine legionum discedi Caesar pateretur, et tantum agris vastandis incendiisque faciendis hostibus noceretur quantum labore atque itinere legionarii milites efficere poterant.*

Interim Trinovantes, prope firmissima earum regionum civitas, ex qua Mandubracius adulescens Caesaris fidem secutus ad eum in continentem [*Galliam*] *venerat, cuius pater in ea civitate regnum obtinuerat interfectusque erat a Cassivellauno, ipse fuga mortem vitaverat,*

legatos ad Caesarem mittunt pollicenturque sese ei daturos atque imperata facturos : petunt ut Mandubracium ab iniuria Cassivellauni defendat atque in civitatem mittat qui praesit imperiumque obtineat. Iis Caesar imperat obsides XL frumentumque exercitui, Mandubraciumque ad eos mittit. Illi imperata celeriter fecerunt, obsides ad numerum frumentumque miserunt.

Trinovantibus defensis atque ab omni militum iniuria prohibitis, Cenimagni, Segontiaci, Ancalites, Bibroci, Cassi legationibus missis sese Caesari dedunt. Ab iis cognoscit non longe ex eo loco oppidum Cassivellauni abesse silvis paludibusque munitum, quo satis magnus hominum pecorisque numerus convenerit. Oppidum autem Britanni vocant, cum silvas impeditas vallo atque fossa munierunt, quo incursionis hostium vitandae causa convenire consuerunt. Eo proficiscitur cum legionibus ; locum reperit egregie natura atque opere munitum ; tamen hunc duabus ex partibus oppugnare contendit. Hostes paulisper morati militum nostrorum impetum non tulerunt seseque alia ex parte oppidi eiecerunt.[1])

1. Many commentators have identified the stronghold with Verulam, or Verulamium, which was situated immediately west of St. Albans.[2] The arguments which can be adduced in support of this view are that marshes might have been formed by the river Ver; that Verulamium, under Tasciovanus, who began to reign not later than 30 B.C., was the chief town of the Catuvellauni ; and that the territory of the Catuvellauni belonged to Cassivellaunus.

2. Others point to Cassiobury in Hertfordshire.[3] Cassiobury, they argue, evidently preserves the name of the Cassi,[4] who were as evidently subject to Cassivellaunus.

3. Von Göler[5] remarks that, 'judging from the configuration and nature of the terrain,' the *oppidum* 'may be the hill lying on the south-western side of Wendover'. May be, or may not be ; for Caesar's vague description of 'the configuration and nature of the terrain' would apply to other sites as well.

4. The most interesting theory is that of T. Lewin,[6] who maintains that the *oppidum* was no other than London, that is to say, the settlement which many writers believe to have existed long before the Roman conquest, in the neighbourhood of Ludgate Hill. As

[1] *B. G.*, v, 19–21. [2] See *Archaeol. Journal*, xxii, 1865, pp. 299–301.
[3] *Archaeologia*, i, 1770, p. 189.
[4] *B. G.*, v, 21, § 1. The habitat of the Cassi is unknown ; and it is very doubtful whether Cassiobury preserves their name. Sir John Evans (*Archaeologia*, liii, 1892, p. 247) remarks that 'at the time of the invasion of Julius Caesar this [Hertfordshire] . . . appears to have been occupied by the Cassi, who not improbably were the same tribe as . . . the Catyeuchlani', or Catuvellauni. With all due deference to so high an authority, I take leave to say, first, that there is no evidence that the Cassi occupied Hertfordshire ; secondly, that there is no evidence for identifying them with the Catuvellauni ; and, lastly, that the Cassi, who surrendered before the capture of Cassivellaunus's stronghold, cannot have been identical with the people who were under the immediate control of Cassivellaunus.
[5] *Gall. Krieg*, 1880, p. 157, n. 2.
[6] *Archaeologia*, xl, 1866, pp. 65–6.

Cassivellaunus, he argues, had conquered the Trinovantes,[1] 'whose western border was the Lea,' we may assume that his dominions extended westward from that river, and comprised Middlesex and Hertford. As Caesar says that he prohibited his soldiers from plundering the Trinovantes, it is clear that, after crossing the Thames, he marched into Essex. There he learned that the *oppidum* of Cassivellaunus was not far off; and 'this situation answers to London'. Moreover British London, which was situated on the rising ground between Ludgate and Dowgate, and protected on the south by the marshes of the Thames, on the west by the marshes of the river Flete, and on the east by the marshes of the river Wallbrook, was just such a stronghold as Caesar described.

Who would not accept such an attractive theory if he could only give rein to his imagination? But unhappily the very existence, in 54 B.C., of British London is matter of inference and conjecture, however reasonable.[2] And is it reasonable to assume that if the stronghold which Caesar captured had been situated on the banks of the Thames he would have neglected to mention the fact?

5. W. H. Black[3] argues that the *oppidum* was most probably somewhere in 'the woody lands about Pinner, Harrow, and Cashiobury Park.'

6. The Reverend H. Jenkins[4] maintains that it was in Essex; for, he argues, in order to fulfil the compact which he had made with the Trinovantes, Caesar's 'chief object, after he had crossed the Thames, must have been to lead his army into Essex'. Certainly Caesar's army, or a part of it, must have entered the country of the Trinovantes; for, as we have seen, he would not allow his soldiers to plunder that people. Jenkins's theory is demolished by Caesar's statement that the stronghold belonged to Cassivellaunus, which shows that it was not in the territory of the Trinovantes. And since, immediately after the sentence in which he tells us that he prohibited his soldiers from plundering the Trinovantes, he goes on to say that the stronghold of Cassivellaunus was not far off, it is fair to conclude that it was near the common frontier of the Trinovantes and of Cassivellaunus.[5] Of the places which fulfil this condition more can be said for Verulam than for any other; but its identity with the *oppidum* in question has not been proved.

[1] There is no evidence that Cassivellaunus had *conquered* the Trinovantes, though he had killed their king, the father of Mandubracius.

[2] See pp. 703–5, *infra*.

[3] *Archaeologia*, xl, 1866, pp. 51–2.

[4] *Journ. Brit. Archaeol. Association*, xvi, 1860, pp. 136–7, 142.

[5] At Redbourn in Hertfordshire is 'an oval encampment probably pre-Roman' (*Archaeologia*, liii, 1892, p. 259); and near Therfield in the same county there is a British camp 'on right of road from Baldock' (*ib.*, p. 261; J. E. Cussans, *Hist. of Herts* [Hundred of Osney], i, 116). It is perhaps just possible that if these camps were excavated, some light might be thrown upon the question.

DID *LONDINIUM* EXIST IN CAESAR'S TIME?

The earliest mention of London occurs in the *Annals*[1] of Tacitus, who, describing the events of the year 61, speaks of it as a busy centre of commerce. It has been argued[2] that a settlement existed there before the Roman conquest of Britain, because the name *Londinium* is Celtic. Lewin[3] maintains further that if London had been founded by the Romans, it would have been a strong military post, whereas in 61, eighteen years after the invasion of Britain by Aulus Plautius, it was attacked by the Iceni and Trinovantes because it was defenceless and wealthy.[4] 'It must,' he insists, 'have attained to this height of prosperity, not under the Romans, who did not patronise it, but by the silent progress of trade, a work that could not... have been accomplished in... 19 years.' 'We know,' he continues, referring to Dion Cassius,[5] 'that Camulodunum was a flourishing British city before the... time of Plautius, and, if so, London, which enjoyed far greater advantages, must also have been a British city.' Lewin holds that this city stood upon the hill between the river Flete and the Wallbrook. On the west was *Ludgate*, the name of which is Celtic: on the east Dowgate,—'a corruption of the Celtic Dwrgate or water-gate.' The river Flete, or Fleet, entered the Thames just below the site of Blackfriars Bridge; while at Dowgate, about 1,000 yards to the east, was the mouth of the Wallbrook.[6]

Mr. W. J. Loftie[7] finds the site of British London on the western side of the Wallbrook. In his *History of London*,[8] however, he affirmed that the British settlement stood 'on the eastern hill [that is to say, on the gently rising ground east of the Wallbrook], if anywhere'. Canon Isaac Taylor[9] asserts that the British hill-fort was 'formed by Tower Hill, Cornhill, and Ludgate Hill, and effectually protected by the Thames... the Fleet... the great fen of Moorfields and Finsbury', &c. Seeing that it has been proved that 'the great fen' did not exist in Roman times,[10] and that the very existence

[1] xiv, 33, § 1.—Suetonius... Londinium perrexit, cognomento quidem coloniae non insigne, sed copia negotiatorum et commeatuum maxime celebre.

[2] *E.g.* by W. H. Black in *Archaeologia*, xl, 1866, pp. 50–2.

[3] *Ib.*, pp. 59–66.

[4] Tacitus, *Ann.*, xiv, 33. 'The chief commercial town,' says Professor Haverfield (*Vict. Hist. of... Northampton*, i, 164), 'was from the earliest times, Londinium.'

[5] *Hist. Rom.*, lx, 21, §§ 3–4. Lewin would have found more conclusive proof of the pre-eminence of Camulodunum in Sir John Evans's *Coins of the Ancient Britons*.

[6] See W. J. Loftie's *Hist. of London*, i, 1883, map facing p. 1; and *Historic Towns,—London*, 1887, map facing p. 16. See also *Archaeol. Journal*, lx, 1903, pp. 137–204, and particularly 155–6.

[7] *Historic Towns,—London*, p. 2.

[8] Vol. i, p. 16.

[9] *Words and Places*, p. 185.

[10] *Archaeol. Journal*, lx, 1903, p. 174.

of the British hill-fort can as yet only be inferred, it is plainly useless to attempt to determine its site.

Dr. Guest [1] holds that 'the notion . . . that a British town preceded the Roman camp [of Aulus Plautius] has no foundation . . . and is inconsistent with all we know of the early geography of this part of Britain.' 'Such town,' he adds, 'could not have belonged to the Trinobantes, for it lay beyond their natural limits, nor to the settled district of the Catuvellauni, for then Caesar's statement that the Thames divided their country from the maritime states "about eighty miles from the sea" would be grossly inaccurate.' I cannot see the force of these arguments. 'The notion that a British town preceded the Roman camp' *has* a foundation,—the solid foundation of etymology. *London* is indisputably a Celtic name;[2] and if London had been founded by the Romans, why should it have had a *purely* Celtic name[3] at all, and why should its Celtic name have outlasted *Augusta*,—the name which the Romans gave to their London? Assuming that Caesar's statement is to be interpreted in the sense which Dr. Guest attaches to it, the distance by road of British London from Sandwich, in the neighbourhood of which Caesar landed,[4] is 67 English miles, or nearly 73 Roman miles. Is the difference between 73 and 'about 80' so great as to justify the use of the words 'grossly inaccurate'?

More recently John Richard Green has endeavoured to disprove the existence of British London.[5] 'Much,' he says, 'has been made of its name, but "Llyn-dyn" . . . is as likely to be the designation of a spot as of a town on it. An almost conclusive proof, however, that no such town existed west of the [river] Fleet may be drawn from the line of the old British road from Kent (the predecessor of the Watling Street), which, instead of crossing the river as in Roman and later times at the point marked by London Bridge, passed, according to Higden, to a point opposite Westminster . . . (Loftie, "Roman London", Archaeological Journal, volume xxxiv, page 165) . . . the rise of such a town [Roman London] is the best explanation of the later change in the line of this road.'

'According to Higden!' According to the monk of the fourteenth century who wrote the *Polychronicon*! And Higden does not so much as mention 'the old British road from Kent',—a road the very existence of which can only be conjectured. What he says is

[1] *Origines Celticae*, ii, 405–6.

[2] *London* is commonly derived from two Celtic words—*llyn*, *din*—meaning 'the lake fort' (see *Geogr. Journal*, xiii, 1899, p. 299). One objection to this etymology is that Mr. F. C. J. Spurrell (*Archaeol. Journal*, xlii, 1885, pp. 300–2) has proved that the lake, which was described so picturesquely by J. R. Green (*The Making of England*, i, 1897, p. 113) did not exist. Moreover, Dr. Henry Bradley (*Morning Post*, Jan. 8, 1907, p. 4, col. 3) tells us that 'the only explanation which is philologically possible is that it [Londinium] denoted a plot of ground belonging to a person named Londinos, which means "fierce"'.

[3] I say 'a *purely* Celtic name' in contradistinction to such hybrid names as Augusto-*dunum* (Autun), &c.

[4] See pp. 664–5, *supra*.

[5] *The Making of England*, i, 1897, p. 117, n. 1.

that 'Watlingstrete' crossed the Thames west of Westminster;[1] and Mr. Loftie, to whom Green appeals, affirms that he is 'driven to the conclusion that there was a British town'.[2] Accepting the statement of Higden, he argues that the 'Watlingstrete' which is said to have crossed the Thames west of Westminster was a pre-Roman road and followed the line of Park Lane and Edgware Road. I will only add that, having failed to discover any paper worth reading about the direction of Watling Street in that part of its course which passed through or by Roman London, I consulted Professor Haverfield. 'I know nothing satisfactory,' he replied, 'about the line of Watling Street, and nothing to suggest that it existed before A.D. 43.'

The very large number of palaeolithic implements which have been found in London and its environs prove that in the earliest times it was a centre of population;[3] but it would hardly be safe to infer from the discoveries of bronze and iron tools and weapons and of British coins[4] that the Romans found a town on the site. If there was such a town, it certainly had little political importance; for while numerous British coins issued from the mints of Verulamium and Camulodunum, not one has been discovered which bears the name of Londinium.[5] Nevertheless it may reasonably be affirmed that London existed before the Roman conquest: first, because the same advantages that attracted the traders of Rome would also have commended themselves to those of Britain; and secondly, I repeat, because it is improbable that a Celtic name would have been given to a town which the Romans had built upon a virgin site.[6]

[1] *Polychronicon Ranulphi Higden*, ed. Churchill Babington, vol. ii, 1869, pp. 44–6.—Secunda via principalis dicitur Watlingstrete . . . Incipit enim a Dovoria, transiens per medium Cantiae ultra Thamisiam juxta Londoniam ad occidentem Westmonasterii, &c.

[2] *Archaeol. Journal*, xxxiv, 1877, p. 166.

[3] Sir J. Evans, *Anc. Stone Implements*, 1897, p. 586; Worthington G. Smith, *Man, the Primeval Savage*, pp. 190, 214.

[4] J. Evans, *Anc. Bronze Implements*, pp. 95, 158, 174–5, 245, 248–9, 272, 278–81, 303, 312, 321, 327–8, 330, 339, 351, 356, 400–1, 411, 424, 450, 467; *Coins of the Anc. Britons*, pp. 70, 83, 122, 125, 232; *ib.*, Suppl., p. 559; *Guide to the Ant. of the Early Iron Age* (Brit. Museum), p. 98. Mr. F. W. Reader (*Archaeol. Journal*, lx, 1903, p. 213) argues that 'it is difficult to conceive that if any considerable British town preceded [the Roman] *Londinium*, all traces of it in the shape of pottery fragments, &c., should . . . have been so entirely obliterated', &c. But the same argument would apply to Calleva, Camulodunum, and other towns which were certainly British.

[5] See p. 359, *supra*.

[6] Cf. Ammianus Marcellinus, xxvii, 8, § 7,—Lundinum, vetus oppidum quod Augustam posteritas appellavit; *ib.*, xxviii, 3, § 7,—Augusta, quam veteres appellavere Lundinum.

THE JULIAN CALENDAR AND THE CHRONOLOGY OF CAESAR'S INVASIONS OF BRITAIN

I. The chronology of Caesar's first invasion of Britain is simple enough, so far as it can be ascertained, and requires no knowledge of the intricacies of the Roman calendar. I have shown in an earlier article that the disembarkation took place on the 26th or 27th of August, 55 B.C.[1] After describing the storm which occurred on the night of August 30–31, and the consequent loss of many of his ships, Caesar goes on to say that the Britons endeavoured to protract the war by cutting off his supplies, and that he had corn brought in daily from the open country into his camp, and ordered the materials necessary for the repair of those ships which were only partially damaged to be fetched from the Continent. While the ships were being repaired an attack was made upon the 7th legion, which was engaged in cutting corn. This attack evidently took place several days after the 31st of August; for the field in which the legion was reaping was the only one accessible from the camp in which the corn had not been cut. The day on which the legion was attacked was followed by several days of stormy weather, during which all military operations were suspended. At the end of this time the Britons attacked the camp unsuccessfully, and on the same day sued for peace, which Caesar granted on condition of their giving hostages. Instead of waiting for the arrival of the hostages he ordered the British chiefs to send them over to the Continent, 'because the equinox was at hand, and he did not think it wise to expose his unseaworthy ships to a voyage in stormy weather.'[2] The equinox occurred on the 26th of September.[3] Our data, then, are as follows:—the attack on the 7th legion occurred several days after the 31st of August; and Caesar returned to Gaul several days after the attack on the 7th legion, but before—probably several days before—the 26th of September.[4] Let us say that he returned

[1] See pp. 600–3, *supra*.

[2] *B. G.*, iv, 29–36.

[3] Le Verrier *apud* Napoleon III, *Hist. de Jules César*, ii, 522. See the next footnote.

[4] T. Bergk (*Jahrbücher für classische Philologie*, 13 Supplementband, 1884, p. 618, n. 2) remarks that Caesar himself regarded the 24th, not the 26th, of September as the date of the equinox. His authority is, I suppose, Vegetius, iv, 39, who says that the autumnal equinox occurred *VIII Kal. Oct.*: but, according to Pliny (*Nat. Hist.*, xviii, 25 [59], §§ 220–1), it fell on the 28th of September, and according to Varro (*Rerum rust.*, i, 28, §§ 1–2), on the 27th. Columella (*De re rust.*, ix, 14) places it *about* the 24th of September (*circa VIII calend. Octobris*); and the 24th was, according to Mommsen (*Die röm. Chron. bis auf Caesar*, 1859, p. 301), the date adopted in the Julian calendar. But that date was fixed by the calculations of Sosigenes: what right, then, has Bergk to assume that Caesar regarded it as the date of the equinox in 54 B.C., nine years before his reform of the calendar took effect?

about the middle of the month. Napoleon, to whom indefiniteness was an abomination, fixed the 12th of September as the date.[1]

II. If we were to believe certain writers of high reputation, we should be deterred from attempting to fix any dates for the second of Caesar's expeditions; for our principal sources of information are dates mentioned by Cicero in his letters; and, as everybody knows, the Roman calendar, before Caesar's reform, was often in disagreement with the Julian calendar. The writers of the article CALENDARIUM in Smith's *Dictionary of Greek and Roman Antiquities*[2] affirm that 'it is very difficult or rather quite impossible to determine the actual dates which correspond to the nominal dates of any events before the Julian reform of the calendar'. But for the period comprised between the beginning of the year 696 (58 B.C.), just after Caesar became Governor of Gaul, and his reform of the calendar, which took effect in 709 (45 B.C.), this view is quite incorrect. Every date in Cicero's correspondence which relates to the subject of this essay can be reduced, if not with absolute precision, at all events with a possible error of not more than one day, to its corresponding date in the Julian calendar. First of all, however, we must find out the nature of the reform which Caesar initiated, and understand the chronological disturbances which made reform necessary.

Every scholar knows that, after the period of the Decemvirate, the Roman year consisted of 355 days only, and that every other year an additional month, consisting alternately of 22 and 23 days, was, or rather ought to have been, intercalated after the 23rd of February. January, April, June, August, September, November, and December each contained 29 days; February 28; and March,

[1] Napoleon's reasoning is based upon assumptions, one of which is certainly incorrect, while all are doubtful. We know that Caesar started on his return voyage soon after midnight (*B. G.*, iv, 36, § 3). 'If,' says Napoleon (*Hist. de Jules César*, ii, 180, note), 'we assume that he had a favourable wind, as he had on his return from the second expedition, and that his voyage lasted nine hours, Caesar would have reached Boulogne about nine o'clock in the morning. As the fleet could only enter the harbour on a rising tide, all that we need do in order to ascertain approximately the date of his return, is to find out on what day of September, 699, there was high tide at that hour at Boulogne. Now in that harbour there is always a high tide about nine o'clock in the morning two or three days before full moon and before new moon. Therefore, as the full moon of September, 699, took place on the 14th of the month, Caesar must have returned to Gaul about the 11th or 12th of September.'

There is no fault to be found with the conclusion (except that it is uncertain), but much with the argument. To begin with, as there had been a full moon on the 31st of August, it is obvious that not the full moon but the new moon of September took place on the 14th of the month. This error, indeed, is immaterial; but Napoleon has no right to assume that Caesar reached Boulogne about nine o'clock in the morning, for the circumstances of his return voyage in the second expedition were totally different from those of the preceding year. In 54 B.C. there was a dead calm (*summa tranquillitate*, *B. G.*, v, 23, § 6), and the ships were rowed: in 55 they sailed. Moreover, it is untrue that the fleet could only enter the harbour of Boulogne at high tide (see p. 586, *supra*).

[2] Vol. i (3rd ed.), p. 343. See also J. P. Postgate, *M. Annaei Lucani de bello civili liber VII*, 1900, p. xiv, n. 3; A. G. Peskett, *C. I. Caesaris comm. de bello civili liber tertius*, 1900, p. 68; and H. Meusel, *C. I. Caesaris comm. de b.c.*, pp. xiv, 367 ff.

May, July, and October 31. If I may remind the general reader of what he learned at school, the first day of every month was called the Kalends; the fifth day of January, February, April, June, August, September, November, and December was called the Nones; and the thirteenth day of each of those months was called the Ides. But in March, May, July, and October the Nones were on the seventh, and the Ides on the fifteenth day. In mentioning dates the Romans described any given day as occurring so many days before the Kalends, Nones, or Ides, as the case might be; and in so doing they adopted the inclusive method of reckoning. Thus the last day of December was called 'the day before the Kalends of January', *pridie Kalendas Ianuarias*, or shortly *prid. Kal. Ian.* But the 27th of December was called, not the third, but the fourth day before the Kalends of January, *ante diem quartum Kalendas Ianuarias*, or shortly *a. d. IV Kal. Ian.*

To return to the month which was or ought to have been intercalated every other year. As the ordinary year contained 355 days, and the solar year was believed to contain 365¼ days, it is obvious that to intercalate a month of 22 and 23 days alternately every other year was to make an excessive correction, the excess amounting to 4 days in every period of 4 years. Macrobius [1] tells us that in order to remedy this error, 24 days were omitted from every twenty-fourth year. For various reasons, however, this regulation, if indeed it ever really took effect,[2] was not always properly carried out; and accordingly in the year of the city 563, or 191 B.C., a reform was introduced, the college of pontiffs being authorized to intercalate or to omit intercalations at their discretion.[3] But this innovation, as we learn from Cicero, Censorinus, and other writers, only made matters worse. Speaking of the pontiffs, Censorinus complains that 'most of them, either from hatred or from favour, to cut short or to extend the tenure of office, or that a farmer of the public revenue might gain or lose more by the length of the year, by intercalating more or less at their pleasure, deliberately made worse what had been entrusted to them to set right'.[4]

III. In 708 (46 B.C.), which is generally called 'the year of con-

[1] *Sat.*, i, 13, §§ 12–3.—sed octavo quoque anno intercalares octo affluebant dies ex singulis, quibus vertentis anni numerum apud Romanos super Graecum abundasse iam diximus. Hoc quoque errore iam cognito haec species emendationis inducta est. Tertio quoque octennio ita intercalandos dispensabant dies, ut non nonaginta sed sexaginta sex intercalarent, compensatis viginti et quattuor diebus pro illis qui per totidem annos supra Graecorum numerum creverant.

[2] See Th. Mommsen, *Die röm. Chron. bis auf Caesar*, 1859, pp. 45–6, and *Rev. hist.*, xlii, 1890, p. 401.

[3] Censorinus, *De die natali*, xx, 4, § 6.—Quod delictum ut corrigeretur, pontificibus datum negotium eorumque arbitrio intercalandi ratio permissa.

[4] *Ib.*, § 7.—Sed horum [pontificum] plerique ob odium vel gratiam, quo quis magistratu citius abiret diutiusve fungeretur aut publici redemtor ex anni magnitudine in lucro damnove esset, plus minusve ex libidine intercalando rem sibi ad corrigendum mandatam ultro quod depravarunt &c. See also Plutarch, *Caesar*, 59; Ammianus Marcellinus, xxvi. 1, § 12; and Macrobius, *Sat.*, i, 14, § 1.

fusion', Caesar intercalated a certain number of days, in order to bring the calendar into harmony with the solar year before inaugurating the reformed calendar in 709. It is expressly stated by Asconius, whose testimony is unanimously accepted, that there was an intercalary month in 702;[1] and it is admitted by all chronologists that there was no other intercalary month in the seven years between 700 and 708.[2] There is no doubt that the Kalends of January, 709, corresponded either with the 1st or the 2nd of January, 45 B.C. It is clear, therefore, that when we have ascertained how many days were intercalated in 708, we shall be able, by reckoning backwards, to ascertain the correspondence of any given date in the summer of 700, the year of Caesar's second expedition, with the Julian calendar,—with a possible error of one day only. This error will

[1] Asconius, *in Milonianam*, p. 35 (*M. Tullii Ciceronis opera*, ed. J. C. Orelli and J. G. Baiter, vol. v, pars ii, 1833).

[2] It may be well to give the proof. Cicero (*Att.*, v, 13, § 1) tells us that the period from the 18th of January, 702, the day on which Clodius was murdered, to the 22nd of July, 703, reckoning inclusively, comprised 560 days; and the reader may satisfy himself that this statement is untrue if there was an intercalary month in 703, and true if there was not. From these data and from the further statement made by Cicero in his speech *Pro Milone*, 98, that the day on which he delivered the speech, namely the 8th of April, 702, was the 101st day since the murder of Clodius, it follows that the intercalary month in 702 amounted to 23 days. It is stated by Curio in a letter to Cicero (*Fam.*, viii, 6, § 5) and by Dion Cassius (xl, 62, §§ 1–2) that there was no intercalary month in 704. It can be proved from the chronological statements which have come down to us regarding the movements of Caesar and Pompey in 705 that there was no intercalary month in that year. Plutarch (*Caesar*, 35, § 1) tells us that Caesar made himself master of Italy in 60 days. Shortly before the 17th of January, 705, the day on which Pompey fled from Rome, Caesar crossed the Rubicon (*Att.*, ix, 10, § 4; Caesar, *B. C.*, i, 14, § 3); and it has been proved (Stoffel, *Guerre civile*, i, 202–3; O. E. Schmidt, *Der Briefwechsel des M. Tullius Cicero*, 1893, p. 104, n. 2) that the exact date was either January 10 or January 11. On the 18th of March he took Brundisium (*Att.*, ix, 15, § 6),—65 days, reckoning inclusively, after his passage of the Rubicon, if there was no intercalary month, but 87 or 88 if there was one. Again, he took Corfinium on the 21st of February, quitted it the same day (*ib.*, viii, 14, § 1), and marched direct to Brundisium, where he arrived on the 9th of March (*ib.*, ix, 13, § 13A). The distance between the two places, measured along the route which Colonel Stoffel believes Caesar to have followed, is 465 kilometres, or about 289 miles. O. E. Schmidt (*Der Briefwechsel des M. T. Cicero*, pp. 385–9) decides for another route; but the difference of opinion between him and Colonel Stoffel does not affect my argument. If there was an intercalary month in 705, Caesar occupied 39 or 40 days on the march, which, considering the notorious rapidity of his movements, is incredible: if there was not, he occupied 17 days (see Stoffel, *Guerre civile*, i, 196–7). That there was no intercalary month either in 706 or in 707 is evident from a statement in one of Cicero's letters to Atticus (x, 17, § 3), written on the 16th of May, 705,—'At present the equinox is delaying us, which has been very stormy' (*Nunc quidem aequinoctium nos moratur, quod valde perturbatum erat*). The equinox actually occurred on the 24th of March. If there was no intercalary month either in 706 or in 707, the 16th of May, 705, fell on the 24th or the 25th of March, 49 B.C. of the Julian calendar. If there was an intercalary month in either of those years, it fell on the 2nd or the 3rd of March. [Le Verrier, who also holds that there was no intercalation in 706 or 707, says that May 16, 705, fell on April 16, 49 B.C.; but Le Verrier assumed, wrongly, as we shall see, that 'the year of confusion' contained only 422, not 445 days.]

be removed if we can ascertain whether the Kalends of January, 709, corresponded with the 1st or the 2nd of January, 45 B.C.

All German scholars who have written upon Roman chronology are agreed that 'the year of confusion' contained 445 days, in other words, that 90 days, amounting to four ordinary intercalary months, were intercalated; and they hold that these 90 days were actually composed of three months, namely the *Mercedonius*, which, in the ordinary course, should in that year have been intercalated immediately after the 23rd of February, and two extraordinary months, amounting to 67 days, which were intercalated between the last day of November and the 1st of December. This view is supported in every detail by Censorinus,[1] who wrote about 240 A.D. The principal dissentients are De La Nauze, Napoleon the Third and his collaborator, the famous astronomer, Le Verrier, who held that 67 days only were intercalated in 708, and Colonel Stoffel, who, in his *Histoire de Jules César—Guerre civile*, published in 1887, reaffirmed the same view, but who does not appear to have informed himself of what any of the Germans, except Ideler, had written on the subject. Moreover, the theory of Napoleon, Le Verrier, and Colonel Stoffel is frequently referred to by scholars in terms which imply that they regard the question as still open. The reason which Le Verrier[2] gives is that in 700 Caesar re-embarked his troops for the return voyage from Britain to Gaul 'because the equinox was at hand' (*quod aequinoctium suberat*[3]); that the equinox actually took place on the 26th of September of the Julian calendar; and that Caesar informed Cicero on the sixth day before the Kalends of October (which corresponded, on Le Verrier's theory, with the 21st of September) that he was on the point of bringing back the army.[4] He remarks that, on the theory of Ideler (who, like all other German scholars, held that 90 days were intercalated in 708), the sixth day before the Kalends of October, 700, would have corresponded with the 30th of August, 54 B.C.;[5] and he argues that this theory must be wrong because Caesar would not have troubled himself about the approach of the equinox 27 days before it occurred. He also remarks that, although the view that 90 days were intercalated in 708 is supported by Suetonius[6] and Censorinus,[7] Dion Cassius[8]

[1] *De die natali*, xx, 4, §§ 8–10.—[adeo aberratum est] ut C. Caesar . . . duos menses intercalarios dierum LXVII in mensem Novembrem et Decembrem interponeret, cum iam mense Februario dies III et XX intercalasset, faceretque eum annum dierum CCCCXLV, &c.

[2] Napoleon III, *Hist. de Jules César*, ii, 521–3; or Stoffel, *Hist. de Jules César, —Guerre civile*, ii, 387–9.

[3] *B. G.*, v, 23, § 5.

[4] Cicero, *Att.*, iv, 18, § 5.

[5] On the theory of Ideler the sixth day before the Kalends of October, 700, corresponded with the 29th of August, 54 B.C. See his *Handbuch der . . . Chron.*, ii, 1826, pp. 115–7, &c.

[6] *Divus Iulius*, 40.

[7] *De die natali*, xx, 4, § 8. According to Macrobius (*Sat.*, i, 14, § 3), the year 708 contained 443 days; according to Solinus (i, 45) 344. These figures are obviously incorrect.

[8] xliii, 26, §§ 1–2.—Τὰς ἡμέρας τῶν ἐτῶν οὐ πάντῃ ὁμολογούσας σφίσι . . . κατεστή-

affirmed that 67 days were intercalated, and that Dion expressly added that other writers had asserted that a greater number had been intercalated, but that his own statement was true; and he insists that Dion derived his information from authentic sources.[1]

It will be shown presently that if Le Verrier was right in his interpretation of Dion's words, Dion made a mistake. G. F. Unger[2] holds that he misunderstood the authority from whom he borrowed his statement, and 'who, like Suetonius, unquestionably regarded the month intercalated in February as an *ordinary* intercalary month'. Von Göler,[3] on the other hand, holds that Dion was quite right; that he was referring, not to the ordinary intercalary month, but only to the two extraordinary months intercalated between November and December, which amounted to 67 days; and therefore that his statement tallies with that of Censorinus. Be this, however, as it may, it is absolutely certain that more than 67 days were intercalated in 708. For on the 16th of May, 705, Cicero wrote from Cumae, 'At present the equinox is delaying us, which has been very stormy' (*Nunc quidem aequinoctium nos moratur, quod valde perturbatum erat*[4]). Now, on Le Verrier's theory, the 16th of May, 705, corresponded with the 16th of April, 49 B.C. of the Julian calendar; but the equinox occurred on the 24th of March. In order to dispose of this difficulty, Le Verrier is obliged to have recourse to flagrant special pleading: 'l'équinoxe,' he says, 'était passé depuis 21 jours,[5] et les troubles atmosphériques pouvaient durer encore. Était-ce d'ailleurs autre chose qu'un prétexte pour Cicéron'?[6] A very thin pretext, one would say,—a pretext which a man of Cicero's intelligence was hardly likely to resort to.[7]

σατο ἐς τὸν νῦν τρόπον ἑπτὰ καὶ ἑξήκοντα ἡμέρας ἐμβαλών, ὅσαιπερ ἐς τὴν ἀπαρτιλογίαν παρέφερον. ἤδη μὲν γάρ τινες καὶ πλείους ἔφασαν ἐμβληθῆναι, τὸ δ' ἀληθὲς οὕτως ἔχει.

[1] Colonel Stoffel (*Guerre civile*, ii, 299–304), while agreeing with Le Verrier's conclusion, argues that the statement of Suetonius is in perfect accord with that of Dion; for, he remarks, Suetonius tells us that three months were intercalated in 708, namely, the ordinary month which should have been intercalated in that year, and two others between November and December; and, says Colonel Stoffel, three intercalary months of 22, 23, and 22 days respectively would have amounted to 67 days.

[2] *Neue Jahrbücher für Philologie*, &c., cxxix, 1884, p. 588.

[3] *Cäsars gall. Krieg und Theile seines Bürgerkriegs*, ii, 1880, p. 199. See also A. W. Zumpt (*Jahrbücher für classische Philologie*, vii. Supplementband, 1873–5, p. 556), who, in my opinion, proves his point. Mommsen (*Die röm. Chron. bis auf Caesar*, 1859, p. 277) maintains that the mere fact that the two extraordinary intercalary months were called *prior* and *posterior* respectively, not *secundus* and *tertius*, proves that the intercalary month inserted before March, 708, was not regarded as belonging to the calendar year 708 at all, but only to the consular year; in other words, that the calendar year began on the 1st of January in 709 for the first time. This view is severely, and I think justly, criticized by Bergk (*Jahrbücher für classische Philologie*, 13 Supplementband, 1884, pp. 631–5).

[4] *Att.*, x, 17, § 3. [5] Le Verrier should of course have written '23 jours'.

[6] Col. Stoffel, *Guerre civile*, ii, 389.

[7] Le Verrier overlooks or ignores the fact that in his very next letter (x, 18, § 1), also written at Cumae, Cicero described the weather as 'an absolutely dead calm' (*mirificae tranquillitates*).

But this is not the only proof of the unsoundness of Le Verrier's theory. The argument which he bases upon Caesar's words, *quod aequinoctium suberat*, shows that he completely misunderstands both Caesar's narrative and the letter of Cicero to which he appeals. Cicero writes, 'On the 24th of October I received letters from my brother Quintus and from Caesar, dated from the nearest coasts of Britain on the 25th[1] of September . . . they were on the point of bringing back the army' (*a Quinto fratre et a Caesare accepi a. d. IX Kal. Nov. litteras, datas a litoribus Britanniae proximis a. d. VI Kal. Octobr. . . . exercitum* [*e*] *Britannia reportabant*[2]). The letters to which Cicero refers were written, according to Napoleon and Le Verrier, on the 21st of September of the Julian calendar; and, they triumphantly remark, this tallies with Caesar's statement, that he hurried on his departure from Britain 'because the equinox was at hand'.[3] But there is one fact which they overlook. Caesar had a large number of hostages and prisoners, and some of his ships had been lost. He was therefore obliged to transport the hostages, prisoners, and troops to Gaul in two successive trips. Only a few of the ships which made the first trip ever returned to Britain, almost all the

[1] Mr. Shuckburgh (*The Letters of Cicero*, i, 1899, p. 327) says by mistake, 'the 26th of September,' forgetting that in the unreformed Roman calendar there were not 30, but only 29 days in September.

[2] *Att.*, iv, 18, § 5. The MS. reading is (a litoribus Britanniae) *proximo*, which is nonsense. Dr. Vogel, however, attempts to translate the untranslatable. 'What other meaning,' he asks (*Neue Jahrbücher für Philologie*, &c., cliii, 1896, p. 283), 'can the somewhat extraordinary expression *datas a litoribus Britanniae proximo* have than that the letter was written in the neighbourhood of the coast of Britain, and therefore not quite at the sea?' The words will not bear this or any other meaning; and it is obvious that Caesar would have gained nothing by writing when he was 'not quite at the sea'; unless, indeed, in order to save a few hours' delay in the transmission of an unimportant private letter, he had sent on a messenger to the coast with orders to embark on a special galley! For *proximo* Boot substituted *proximis*, a conjecture which is generally accepted. Whatever Cicero may have written, it is certain that the letters which he received from his brother and from Caesar were written in Britain; but T. Bergk, to whom the conclusions which commend themselves to plain men are generally distasteful, insists (*Jahrbücher für classische Philologie*, 13 Supplementband, 1884, p. 616) that *litoribus Britanniae proximo* means 'the coasts nearest *to* Britain, that is to say, Boulogne'; and he defends this interpretation by the argument that Caesar had a rooted dislike of mentioning unknown names. But, as Bergk himself maintains, Boulogne was the Portus Itius; and, as Caesar twice mentioned the Portus Itius in his *Commentaries* (*B. G.*, v, 2, § 3; 5, § 1), it is difficult to see why he should have shrunk from doing so in a letter.

Bergk's theory leads him to the absurd conclusion that Caesar quitted Britain for Gaul on the day before he wrote this letter, that is to say, on the 29th (or 30th) of August of the Julian calendar. Absurd, because, as I show in the text (p. 713), Cicero would in that case have written, not (exercitum *e* Britannia) *reportabant*, but *reportaverant*; and because Caesar, who had not quitted Britain in the preceding year until, at the earliest, September 11, would not have felt obliged to sail four weeks before the equinox 'because the equinox was at hand', and would certainly have thought it perfectly safe to wait several days longer for the return of the ships which carried the first detachment of his army back to Gaul, and which he could ill spare.

[3] *B. G.*, v, 23, § 5.

rest having been driven back to Gaul by adverse winds. Caesar tells us that he waited for these ships 'a considerable time (*aliquamdiu*) in vain'. Then, and not till then, he made the second trip, being obliged to crowd the soldiers into the few ships that he had, and not daring to wait any longer, 'because the equinox was at hand.'[1] The letters to which Cicero refers were evidently written before the first trip; for neither Caesar nor Quintus had written to Cicero for a long time;[2] and they would naturally have dispatched their letters by one of the ships which made the first voyage. It is clear, therefore, that the letters in question were written, not on the 21st of September of the reformed calendar, but 'a considerable time' before that day. In fine, if Le Verrier and Napoleon were right, the letters would have been written not from Britain but from Gaul, and Cicero would have written not *reportabant* ('they were on the point of bringing back') but *reportaverant* ('they had brought back'[3]).

Another fact, which Napoleon, Le Verrier, and Colonel Stoffel appear to have overlooked, alone proves that not 67 days only but 90 days were intercalated in that year. The word *nundinae* is familiar to many readers of Cicero's letters. To quote the authors of the article NUNDINAE in Smith's *Dictionary of Greek and Roman Antiquities*,[4] 'the Romans had a system of eight-day weeks, which, like our seven-day weeks, ran on from one month to another and from one year to another without breaking.' Every eighth day was a market-day, and was called *nundinae*. Thus, if the 1st of January was a market-day, the next was the 9th, the next the 17th, and so on. We learn from Dion Cassius[5] that the Kalends of January, 702, was a market-day; the same writer says that, in order to prevent the Kalends of January, 714, from falling on a market-day—a coincidence which was regarded as ill-omened—a day was intercalated extraordinarily in 713;[6] and it follows that, if there had been no intercalation in 713, the number of days that elapsed from the Kalends of

[1] Cicero, *Att.*, iv, 18, § 5; *B. G.*, v, 23. [2] Cicero, *Q. fr.*, iii, 3, § 1.

[3] Cf. Unger (*Neue Jahrbücher für Philologie*, &c., cxxix, 1884, p. 586), and Zumpt (*Jahrbücher für classische Philologie*, vii Supplementband, 1873–5, p. 564). Bergk (*ib.*, 13 Supplementband, 1884, p. 618), remarking that Napoleon admits that Caesar quitted Britain in 55 B.C. as early as the 12th of September, says that the rise of Arcturus, which, according to Pliny (*Nat. Hist.*, xviii, 31 [74], § 310), took place on that day, marked the commencement of the stormy season, and that it is therefore inconceivable that Caesar would have postponed his departure until the middle of the month. I do not attach the least importance to this argument. Caesar went by the equinox, not by the rise of Arcturus, and he waited as long as he thought safe. Moreover, Bergk apparently forgets that the date fixed by Pliny for the rise of Arcturus was borrowed from the Julian calendar, the astronomical calculations for which were not made until 46 B.C. [It should be noted that, according to Columella (*De re rust.*, xi, 2), whom Bergk also quotes, the rise of Arcturus took place on the 17th of September, but the 13th presaged the approach of stormy weather (*tempestatem significat*).]

[4] Vol. ii (3rd ed.), pp. 251–2. See also Varro, *Rerum rust.*, ii, 1, and W. Soltau, *Röm. Chron.*, 1889, p. 38, n. 1.

[5] *Hist. Rom.*, xl, 47, § 1. [6] *Ib.*, xlviii, 33, § 4.

January, 702, to the last day of December, 713, would have been a multiple of 8. Now, on the theory of Napoleon the Third, Le Verrier, and Colonel Stoffel this number would have been 4,401; on the theory that the year 708 contained 445 days, 4,424. The latter number is divisible by 8; the former is not.

There has never been any question but that the number of days intercalated in 708 was either 67 or 90; and the former number has been proved to be wrong.

IV. In order to obtain an absolutely firm foundation for the chronology of Caesar's second expedition, one more question remains to be answered. In this country it is generally taken for granted that the Kalends of January, 709, the year in which Caesar's reform of the calendar came into operation, corresponded with the 1st of January, 45 B.C. Various German scholars have, however, attempted to prove that the Kalends of January, 709, fell on the 2nd of January of the Julian calendar.

Let us first see what there is to be learned from the ancient writers.

Pliny[1] says that when the error in the execution of Caesar's reform was discovered, it was corrected by the omission of intercalary days during twelve successive years.

Solinus[2] tells us that Caesar's reform was vitiated by the pontiffs; for, whereas it had been enjoined that a day should be intercalated on the completion of the fourth year, they made the intercalation at the beginning of the fourth year, not at the end. Thus, Solinus continues, twelve days were intercalated in thirty-six years, whereas only nine ought to have been intercalated.

Suetonius[3] says that the calendar, as reformed by Caesar, was thrown into confusion by 'negligence', and rectified by Augustus; also that Caesar made the calendar year consist of 365 days, so as to bring it into harmony with the solar year, and, abolishing the intercalary month, ordered that one day should be intercalated every fourth year.[4]

According to Censorinus,[5] Caesar directed that, in order to compensate for the quarter of a day by which the solar exceeded the calendar year, one day should be intercalated at the end of every quadriennial cycle, after the Terminalia [that is to say, after the 23rd of February].

[1] *Nat. Hist.*, xviii, 25 (57), § 211.—ea ipsa ratio postea comperto errore correcta est, ita ut duodecim annis continuis non intercalaretur.

[2] *Collect. rerum memorabilium*, i, 45–6.—vitium admissum est per sacerdotes. Nam cum praeceptum esset, anno quarto ut intercalarent unum diem, et oporteret confecto quarto anno id observari, antequam quintus auspicaretur, illi incipiente quarto intercalarunt, non desinente. Sic per annos sex et triginta cum novem dies tantummodo sufficere debuissent, duodecim sunt intercalati.

[3] *Divus Augustus*, 31.—Annum a Divo Iulio ordinatum, sed postea neglegentia conturbatum atque confusum, rursus ad pristinam rationem redegit.

[4] *Ib.*, 40.—annumque ad cursum solis accommodavit, ut trecentorum sexaginta quinque dierum esset, et intercalario mense sublato unus dies quarto quoque anno intercalaretur.

[5] *De die natali*, xx, 4, § 10.—Praeterea pro quadrante diei, qui annum verum suppleturus videbatur, instituit, ut peracto quadrienni circuitu dies unus, ubi mensis quondam solebat, post Terminalia intercalaretur.

According to Macrobius,[1] Caesar directed that one day should be intercalated every fourth year. Macrobius then makes substantially the same charge against the pontiffs as Solinus, and goes on to say that, after the error for which they were responsible had continued for thirty-six years, Augustus corrected it by ordering that twelve years should pass without any intercalation.

Now it is absolutely certain that of the first five years during which the reformed calendar was in force, namely 709, 710, 711, 712, and 713, not one only but two contained an intercalary day.[2] For, as we have already seen,[3] Dion Cassius[4] states that the Kalends of January, 702, was a market-day, and also that, in order to prevent the Kalends of January, 714, from falling on a market-day, a day was intercalated extraordinarily (*παρὰ τὰ καθεστηκότα*[5]) in 713;[6] and, as I had occasion to remark before, it follows that, if there had been no intercalation in 713, the number of days that elapsed from the Kalends of January, 702, to the last day of December, 713, would have been a multiple of 8. This would have been the case if one of the four years 709, 710, 711, and 712 had contained an intercalary day, but not otherwise.[7] Which was it?

[1] *Sat.*, i, 14, §§ 6, 13.—[Caesar] statuit ut quarto quoque anno sacerdotes . . . unum intercalarent diem . . . sic annum civilem Caesar habitis ad lunam dimensionibus constitutum edicto palam posito publicavit et [error] huc usque stare potuisset, ni sacerdotes sibi errorem novum ex ipsa emendatione fecissent. Nam cum oporteret diem qui ex quadrantibus confit quarto quoque anno confecto antequam quintus inciperet intercalare, illi quarto non peracto sed incipiente intercalabant. Hic error sex et triginta annos permansit . . . sed hunc quoque errorem . . . correxit Augustus, qui annos duodecim sine intercalari die transigi iussit, ut illi tres dies . . . sequentibus annis duodecim nullo die intercalato devorarentur.

[2] See W. Soltau, *Röm. Chron.*, p. 171, and L. Holzapfel in *Philologus*, xlix, 1890, p. 67.

[3] See p. 713, *supra*.

[4] xl, 47, § 1.—*καὶ ἡ ἀγορὰ ἡ διὰ τῶν ἐννέα ἀεὶ ἡμέρων ἀγομένη ἐν αὐτῇ τῇ τοῦ Ἰανουαρίου νουμηνίᾳ ἦχθη.*

[5] xlviii, 33, § 4.—*ἡμέρα ἐμβόλιμος παρὰ τὰ καθεστηκότα ἐνεβλήθη, ἵνα μὴ ἡ νουμηνία τοῦ ἐχομένου ἔτους τὴν ἀγορὰν τὴν διὰ τῶν ἐννέα ἡμερῶν ἀγομένην λάβῃ* &c.

[6] Mommsen, falling into an inexplicable confusion of thought, insists (*Die röm. Chron. bis auf Caesar*, 1859, pp. 283–6) that the extraordinary intercalation mentioned by Dion took place not in 713 but in 714. Dion's words, he says (*ib.*, p. 283, n. 5), belong to a passage which immediately follows his description of the events of 714; which deals with the events of 713 and 714; which begins with the words *ἔν τε τῷ πρὸ τούτου ἔτει* (713); and which ends with the words *ταῦτα μὲν ἐν τοῖς δύο ἔτεσιν* (713–4) *ἐγένετο*. He says that Dion's words, taken by themselves, allow us to refer the extraordinary intercalation either to 713 or to 714; but he maintains that it must be referred to 714, because otherwise the sequence of the nundinal letters would be inexplicable. But the truth is, as the simple arithmetical calculation which I have given on pp. 713–4 shows, that the sequence is perfectly explicable if the extraordinary intercalation took place in 713, hopelessly inexplicable if it took place in 714. Except Unger, all recent chronologists (see, for instance, *Hermes*, xxiii, 1888, p. 51 and n. 1) have recognized Mommsen's blunder. Dion says that the extraordinary intercalation took place *ἐν τῷ πρὸ τούτου ἔτει*: Mommsen himself affirms that *τὸ πρὸ τούτου ἔτος* was the year 713; yet he will have it that the extraordinary intercalation took place in 714!

[7] The year 702, as we have already seen, contained 378 days; each of the

The year 711 may be set aside at once: nobody has ever argued that it was a Leap Year; and no reason can be given to show that it was.

1. Wilhelm Soltau,[1] the author of a valuable work on Roman chronology, maintains that 709 was the first Leap Year of the reformed calendar. He argues that Matzat's theory, according to which an intercalation occurred in 710, must be wrong, because it is inconceivable that Caesar, who was then alive, should have allowed an intercalation to take place in the second year of his calendar, in defiance of his own edict, that the intercalation should be made every fourth year (*quarto quoque anno intercalandum esse*). The theory that the first intercalation was in 712 is, Soltau continues, based only upon the statements, derived from the same source, of Solinus and Macrobius. If, says Soltau, they are right, the Leap Years, before Augustus rectified the error which had been made in carrying out Caesar's intentions, would have been 712, 715, 718, 721, 724, 727, 730, 733, 736, 739, 742, and 745. But Dion's statement, that 713 was an intercalary year, proves that this cannot have been the case. The series must, therefore, have been 709, 713, 716, 719 . . . 743. Soltau holds that the twelve years during which intercalation was, by the order of Augustus, suspended, lasted from 745 to 756; that Augustus disapproved of Caesar's (assumed) anticipatory method of reckoning, that is to say, of his having intercalated a day in the first year of his calendar instead of after the conclusion of the fourth; that accordingly the next intercalation, which would naturally have taken place in 757, was omitted; and that the first intercalation after the reform of Augustus occurred in 761. Thus, like other writers[2] who differ from him on points of detail, Soltau identifies the Kalends of January, 709, with the 2nd of January, 45 B.C.

It will presently be shown that the statements of Macrobius and Solinus do not *necessarily* lead to the conclusion which Soltau condemns. Meanwhile I may remark that Soltau's theory is irreconcilable with the very passage in Dion Cassius to which he refers.[3] Dion says that a day was intercalated *extraordinarily* (*παρὰ τὰ καθεστηκότα*) in 713, in order to prevent New Year's Day in 714 from falling on a market-day,[4] and that subsequently an intercalary day was struck out.[5] It is therefore obvious that the intercalation of 713 took place earlier than had been contemplated; and conse-

five years 703, 704, 705, 706, and 707 contained 355 days; the year 708 contained 445 days; one of the four years 709, 710, 711, and 712 contained *ex hypothesi* 366 days, and the other three 365; and if there had been no intercalation in 713, that year would have contained 365 days. Then the number of days from the Kalends of January, 702, to the last day of December, 713, would have been 378 + 355 × 5 + 445 + 366 + 365 × 4 = 4,424 days, which is a multiple of 8.

[1] *Röm. Chron.*, pp. 171–3. [2] Holzapfel, A. Mommsen, and Unger.

[3] See *Philologus*, xlix, 1890, p. 88. [4] See p. 715, *supra*.

[5] *καὶ δῆλον ὅτι [ἡμέρα ἐμβόλιμος] ἀνθυφηρέθη αὖθις, ὅπως ὁ χρόνος κατὰ τὰ τῷ Καίσαρι τῷ προτέρῳ δόξαντα συμβῇ* (*Hist. Rom.*, xlviii, 33, § 4).

quently that the previous intercalation must have occurred later than 709; for if, as Soltau maintains, the previous intercalation had taken place in 709, the intercalation of 713 took place at the proper time. If, on the other hand, Dion's words, *παρὰ τὰ καθεστηκότα*, mean 'contrary to the regulations erroneously attributed to Caesar by the pontiffs', that is to say, contrary to the triennial cycle which they themselves followed, the intercalation, on Soltau's theory, took place a year too late; for, if the first intercalation had occurred in 709, the object which Dion mentions could have been attained by intercalating in 712.

2. Let us now examine the theory of Matzat,[1] namely, that the first intercalation took place in 710. This writer believes that Caesar's reason for intercalating in 710 was to prevent the Kalends of January in the following year from falling on a market-day.[2] He holds that Dion's words, *παρὰ τὰ καθεστηκότα*, mean 'contrary to the actual regulations of Caesar'; and accordingly he believes that those regulations were at the time understood. He maintains, however, that, after 713, the pontiffs intercalated every three years,—namely in 716, 719, 722 . . . 743; but he insists that they did this simply for the same reason which had prompted the intercalation in 713, namely to prevent the Kalends of January in each following year from falling on a market-day. The statement of Dion, that, in order to compensate for the day extraordinarily intercalated in 713, another intercalary day was omitted, he takes to mean that the next intercalation, which, on his theory, ought to have occurred in 714, was left out. Finally, he believes that the three superfluous days which had accumulated during the twelve triennial cycles were compensated for by the omission of all intercalations in the years 745–756; and that the first intercalation under the reform of Augustus occurred in 757. On this theory the Kalends of January, 709, corresponded with the 1st of January, 45 B.C.

I have already mentioned the objection which Soltau has brought against Matzat's theory;[3] but that objection is inconclusive. It is not true that if Caesar had allowed an intercalation to take place in 710, he would have done so 'in defiance of his own edict, that the intercalation should be made every fourth year (*quarto quoque anno intercalaretur*)'. If, according to his scheme, the next intercalation was to take place in 714, the next in 718, and so on, the intercalation would still be made every fourth year. Provided it took place every four years, what difference would it make whether it took place first in 709, 710, 711, 712, or 713? Holzapfel[4] blames Matzat for disregarding the testimony of Solinus and Macrobius. But Matzat does not disregard their testimony: he simply refuses to admit that they make any definite statement as to the year in which the *first* intercalation of the Julian calendar occurred. The only statement which would appear to support Holzapfel's criticism is contained in

[1] *Röm. Chron.*, i, 1883, pp. 11–8.
[2] *Hermes*, xxiii, 1888, p. 56, note.
[3] See p. 716, *supra*.
[4] *Philologus*, xlix, 1890, p. 85.

the words of Solinus, that, 'whereas it had been enjoined that they [the pontiffs] should intercalate one day in the fourth year, and this ordinance ought to have been carried out on the completion of the fourth year . . . they intercalated at the beginning of the fourth year, not at the end' (*nam cum praeceptum esset, anno quarto ut intercalarent unum diem, et oporteret confecto quarto anno id observari . . . illi incipiente quarto intercalarunt, non desinente*). If by 'the beginning of the fourth year' Solinus meant the fourth year of the Julian calendar, that is to say, 712, and if he had original authority for his statement, then Holzapfel is right. But observe the looseness with which Solinus expresses himself. Immediately after saying that the intercalation ought to have taken place '*in* the fourth year', he says that it ought to have taken place 'on the completion of the fourth year'. To state the facts correctly required extraordinary precision and nicety of expression; and this requirement he failed to satisfy. His meaning may have been that, in whatever year of the Julian calendar the first intercalation took place, the next ought, by Caesar's ordinance, to have taken place four years later, and so on. If it be objected that I have suggested an arbitrary interpretation of his meaning, I reply that this interpretation is dictated by the passage in which Dion Cassius states that the intercalation of 713 was 'contrary to the regulations' (*παρὰ τὰ καθεστηκότα*). Holzapfel[1] says that these words, *taken by themselves*, may mean one of two things. They may mean that the intercalation of 713 was contrary to the actual regulations of Caesar; or they may mean that it was contrary to the regulations adopted by the pontiffs in misunderstanding or in contravention of Caesar's regulations. The question, says Holzapfel, can only be settled by other evidence; and the only other evidence is that of Solinus and Macrobius, which shows that the pontiffs misunderstood Caesar's regulations. As a matter of fact, their evidence does not show this, unless misunderstanding is connoted by the words *vitium* and *error*. Matzat[2] contends that such a misunderstanding would have been impossible, for Caesar must have made his intentions clear. Holzapfel replies that Caesar would no doubt have done so if he had foreseen his own imminent death; but, as he certainly intended, in his capacity as Pontifex Maximus, to superintend the execution of his own arrangements, and thus establish the rule of intercalation which he contemplated, the regulation *ut quarto quoque anno intercalaretur* might seem sufficient. But Dion Cassius, if his testimony may be accepted, settles the question. Immediately after saying that a day was extraordinarily intercalated in 713, he adds that 'of course an intercalary day was in turn omitted, *in order that the calendar might be brought into harmony with Caesar's intentions*' (*καὶ δῆλον ὅτι [ἡμέρα ἐμβόλιμος] ἀνθυφῃρέθη αὖθις, ὅπως ὁ χρόνος κατὰ τὰ τῷ Καίσαρι τῷ προτέρῳ δόξαντα συμβῇ*). Now these words, as Matzat[3] unanswer-

[1] *Philologus*, xlix, 1890, p. 72.
[2] *Hermes*, xxiii, 1888, p. 50.
[3] *Hermes*, xxiii, 1888, p. 48.

ably argues, prove that by παρὰ τὰ καθεστηκότα Dion meant 'contrary to the regulations' actually made by Caesar.[1] Holzapfel,[2] however, tries to explain away Dion's remark by the argument that the authority whom he followed may have been a contemporary who shared the misconception of the pontiffs; and this I cannot gainsay. Moreover, although I have argued that the words of Solinus *may* be interpreted in a sense different from that which Holzapfel ascribes to them, I admit that the conclusion which they suggest is that Caesar intended to make his first intercalation in 713, and that the pontiffs made it in 712.

Secondly, it is expressly stated by Macrobius[3] that the Kalends of January in the year of the *Lepidianus tumultus* fell on a market-day; and if, as Holzapfel maintains, this statement refers to the Lepidus who, as one of the Triumvirs, revived Sulla's policy of proscription in 711, it proves that no intercalation occurred either in 710 or in 709.[4] Matzat, however, maintains that the words *Lepidianus*

[1] Assuming that the pontiffs misunderstood Caesar's regulation, and did not simply set it aside, is it possible to explain their mistake? It is often taken for granted that the Romans only used the inclusive method of reckoning. This, however, is an error: Holzapfel shows that our method was generally adopted by Cicero, except of course in the case of dates. Generally, however, in ordinary speech, when the number in question was less than ten, the tendency was to employ the inclusive method; and, as the same tendency prevailed in official phraseology, Holzapfel argues (*Philologus*, xlix, 1890, p. 74) that it would not have been unnatural for the pontiffs to interpret Caesar's regulations in this sense. See also Th. Mommsen, *Die röm. Chron. bis auf Caesar*, 1859, pp. 162–3, 317; L. Holzapfel, *Röm. Chron.*, pp. 353–4; and p. 602, n. 5, *supra*. But, apart from the question of Roman methods of reckoning, is it likely that the pontiffs should have been ignorant of the astronomical reason which led Caesar to enact that one year in every four must contain an intercalary day? Holzapfel thinks that it is. 'We shall hardly do the pontiffs an injustice,' he says (*Philologus*, xlix, 1890, p. 74), 'if we assume that they knew about as much of the actual duration of the [solar] year as Censorinus, who treats the matter as not yet thoroughly ascertained.' The passage in Censorinus (*De die natali*, xix, 2), to which Holzapfel refers, runs as follows:—*Hoc tempus quot dierum esset, ad certum nondum astrologi reperire potuerunt.* He then quotes various astronomers, all of whom agreed of course that the number of days was 365, but differed in regard to the fraction of a day by which the duration of the year exceeded 365 days. Perhaps the pontiffs did not know that Sosigenes, upon whose calculations Caesar relied, estimated that fraction at one quarter (see p. 725, *infra*). If they set aside Caesar's regulation not from ignorance but deliberately, their motive must have been to avoid the coincidence of the Kalends of January in every third year with a nundinal day.

[2] *Philologus*, xlix, 1890, p. 72 and n. 1.

[3] *Sat.*, i, 13, § 17.—quotiens incipiente anno dies coepit qui addictus est nundinis, omnis ille annus infaustis casibus luctuosus fuit, maximeque Lepidiano tumultu opinio ista firmata est.

[4] To spare the reader the trouble of doing a sum, I give the proof. The 1st of January, 702, fell, as we have already seen (p. 713), on a market-day; therefore, if the 1st of January, 711, did the same, the number of days that elapsed from the 1st of January, 702, to the last day of December, 710, inclusive, must have been divisible by 8. The year 702 contained 378 days; each of the years 703–7 contained 355 days; 708 contained 445; and *ex hypothesi* 709 and 710 each contained 365. Now 378 + 355 × 5 + 445 + 365 × 2 = 3,328, which is exactly divisible by 8.

tumultus designate the outburst of Lepidus in 676 (78 B.C).[1] Unger,[2] who agrees with Holzapfel, maintains that the official recognition of the superstitious dread with which the Roman populace contemplated the coincidence of the Kalends of January with a market-day, was due to the acts of Lepidus in 711. But, replies Matzat,[3] for this 'official recognition' the approbation of the Pontifex Maximus was necessary. Now in 713, as in 711, the Pontifex Maximus was Lepidus himself; and, according to Macrobius, it was the *Lepidianus tumultus* which strengthened the popular belief that whenever the Kalends of January fell upon a market-day, the whole year would be darkened by ill-omened events. If, then, says Matzat, we are to believe Unger, Lepidus described the deeds which he had himself done in 711, and by which the joint supremacy of himself and the other two Triumvirs had been established, as a *tumultus*,—the most horrible events of a year full of horrors! This argument is clever, but I think that it is hardly fair. Lepidus was not obliged to describe anything. Assuming that the Kalends of January, 711, had fallen on a market-day, it is surely intelligible that he should have recognized the wisdom of allaying superstitious fears, even though they had been roused by his own acts, when he could do so by the simple expedient of intercalating a day in 713. I agree with Unger that there does not appear to have been anything very alarming in the affair of 677, even though Lucan[4] describes it as *truces*

[1] Cf. Th. Mommsen, *Die röm. Chron. bis auf Caesar*, 1859, pp. 25, 286. Unger remarks (*Neue Jahrbücher für Philologie*, &c., cxxix, 1884, p. 760) that the outburst of 676 [or rather 677] was too insignificant to have been selected by Macrobius as an illustration. Moreover, says Holzapfel, the particular *tumultus* owing to which the superstitious dread of the coincidence of *nundinae* with the Kalends of January was intensified must have been preceded by other calamities associated with the same coincidence. In the earlier part of 702, when the Kalends of January fell on a market-day, there were no consuls, which might well awaken apprehensions. In 705, when the same coincidence occurred, the Civil War broke out. The *Lepidianus tumultus* of 711 was accompanied by proscriptions; therefore the superstition would have been confirmed, as Macrobius says, by that *tumultus*.

Undoubtedly,—if, as Holzapfel maintains, it is true that in 711 the Kalends of January fell upon a market-day. But this is the very point at issue; and Holzapfel seems to ignore the possibility that the *Lepidianus tumultus* of 677 may also 'have been preceded by other calamities associated with the same coincidence'. Moreover, Matzat objects that of the events of 711 the outbreak of Lepidus was the least important, and that if Macrobius had intended to refer to that year, he would have said *tumultus Antonianus*. Holzapfel replies that when Lepidus joined Antony, the war which the latter had begun assumed a new phase, and Lepidus became commander-in-chief of the united armies (Velleius Paterculus, ii, 63, § 1; Appian, *B. C.*, iii, 84), a fact which justifies the phrase, *Lepidianus tumultus*. Further, to show how flagitious the conduct of Lepidus appeared to contemporaries, he refers to Cicero, *Fam.*, xii, 8, § 1 (*Scelus adfinis tui Lepidi . . . cognosse te arbitror*), 9, § 2 (*Nos, confectum bellum quom putaremus, repente a Lepido tuo in summam sollicitudinem sumus adducti*), and 10, § 3 (*Praeclare viceramus, nisi spoliatum, inermem, fugientem Lepidus recepisset Antonium. Itaque numquam tanto odio civitati Antonius fuit quanto est Lepidus; ille enim ex turbulenta re p., hic ex pace et victoria bellum excitavit*).

[2] *Neue Jahrbücher für Philologie*, &c., cxxix, 1884, p. 760.

[3] *Hermes*, xxiii, 1888, pp. 60–1.

[4] *Pharsalia*, viii, 808.

Lepidi motus; and, judging the question without bias on its own merits, I can only conclude that the *tumultus Lepidianus* was most probably the outbreak of 711. If so, the first intercalary year of the Julian calendar cannot have been either 709 or 710, but must have been 712.

Thirdly, Holzapfel[1] points out that, if Matzat is right, the intercalary cycle introduced by Augustus did not correspond with that of Caesar. For after the reform of Augustus the intercalary years were odd years, 761, 765, 769, and so on; while Caesar's first intercalary year is supposed by Matzat to have been 710. Or, if we reckon the quadriennial cycles contemplated by Caesar from the year 709, the intercalations, according to his regulation, would, on Matzat's theory, occur in the second, those made by Augustus in the first year of each successive cycle.

All this is perfectly true: but what does it matter? The one really important point, namely, that the intercalation should take place every four years, was duly secured by Augustus. Whether it took place in the first, the second, the third, or the fourth year of the cycle, mattered not a jot. Holzapfel's objection is purely academic.

Fourthly, says Holzapfel, if, as Matzat maintains, Caesar's only reason for intercalating in 710 was to prevent the Kalends of January in the following year from falling on a market-day, it is difficult to believe that Caesar should not have foreseen that for the same reason it would be necessary to intercalate in 713, 716, and so on, that is to say, every three years; in other words, that it would be impossible to carry out the arrangement which he had himself made.[2]

This is certainly a reasonable objection, and Matzat has not, so far

[1] *Philologus*, xlix, 1890, p. 69.

[2] Matzat's theory, Holzapfel insists (*Philologus*, xlix, 1890, pp. 71–2), forces him to contradict himself. First, he argues (*Röm. Chron.*, i, 1883, p. 17) that Caesar fixed the time of his first intercalation *simply* with the object of preventing the Kalends of January, 711, from falling on a market-day; in other words, he holds that the intercalary day contemplated by Caesar was a movable one. But if so, we must disregard the testimony of Dion, who says that the intercalation of 713 was 'contrary to the regulations' (*παρὰ τὰ καθεστηκότα*). Accordingly in *Hermes*, xxiii, 1888, p. 51, Matzat silently abandons his earlier view, and assumes that Caesar intended to intercalate in 714, 718, 722, &c. But if this cycle had been observed, the 1st of January, 714, 717, 720, and so on, would have fallen on a market-day; and therefore Matzat's revised theory is obviously irreconcilable with his original view, that Caesar intercalated in 710 in order to prevent the Kalends of January, 711, from falling on a market-day.

Matzat has not, so far as I can discover, made any rejoinder to Holzapfel's article; but it is not impossible to answer this argument. Supposing that Caesar intercalated in 710 in order to prevent the Kalends of January, 711, from falling on a market-day, why should we disregard the testimony of Dion? Caesar's regulation was that the intercalation should take place every four years. If, no matter for what reason, the first intercalation took place in 710, the second would fall due in 714. By transferring it to 713, Caesar's regulation would be contravened. Nor is the theory that Caesar intended to intercalate in 714, 718, 722, &c., necessarily inconsistent with the view that he intercalated in 710 in order to prevent the Kalends of January, 711, from falling on a market-day; for, as I have remarked in the text, he may perhaps have failed to look ahead.

as I know, attempted to remove it: but it is perhaps conceivable that a man so busy as Caesar should have failed to look far ahead.

Holzapfel[1] argues, further, that Matzat's theory, according to which the calendar, as reformed by Augustus, was inaugurated on the 1st of January, 757 (A.D. 4), rests upon the assumption that the first day of the intercalary cycle coincided with the first day of the civil year, whereas it was really the day after the *Terminalia*, that is to say, the sixth day before the Kalends of March. This, he maintains, is proved (*a*) by the place which Caesar gave to his intercalary day; (*b*) by the fact that the two months intercalated between November and December of 708 were called respectively *mensis intercalaris prior* and *mensis intercalaris posterior*, and also by the fact that, according to Dion Cassius, the number of days intercalated in that year was only 67, whereas the number of days intercalated in *the civil year* 708 was 90; (*c*) by the fact that, according to Macrobius and Solinus, Caesar ordained that the intercalary day [which followed the 23rd of February] should be inserted at the end of the fourth and before the fifth year of the Julian calendar.[2]

Matzat[3] summarily replies to these arguments. Referring to Macrobius,[4] he observes (*a*) that the place which Caesar gave to the

[1] *Röm. Chron.*, pp. 328–9; *Philologus*, xlix, 1890, pp. 77–8.

[2] The writers of the article CALENDARIUM in Smith's *Dictionary of Greek and Roman Antiquities* (i, 344), who assume that Caesar's calendar came into operation on the 1st of January, 45 B.C., argue that his motive for making the year begin on that day 'was probably the desire to gratify the superstition of the Romans by causing the first year (*sic*) of the reformed calendar to fall on the day of the new moon . . . the mean new moon occurred at Rome on the 1st of January, 45 B.C., at 6^h 16′ p.m. In this way alone can be explained the phrase used by Macrobius (*Sat.*, i, 14, 13): *annum civilem Caesar habitis ad lunam dimensionibus constitutum edicto palam posito publicavit.*' Holzapfel, on the other hand, shows (*Philologus*, xlix, 1890, p. 87) that 'Macrobius's words, if one considers the context, only imply that Caesar made no alteration in the place of Kalends, Nones, and Ides, which originally had reference to the lunar phases'. See also Th. Mommsen (*Die röm. Chron. bis auf Caesar*, 1859, p. 277, n. 2) and Matzat (*Hermes*, xxiii, 1888, pp. 61–3). Matzat's arguments were directed against A. Mommsen, who assumed (*Philologus*, xlv, 1886, pp. 411–38) that the new moon had occurred on the 2nd of January 45 B.C., and accordingly argued that Caesar's calendar began on that day. Mr. J. K. Fotheringham (*Journal of Philology*, No. 57, 1903, pp. 98–9) affirms that 'there was a new moon on the 2nd of January, 45 B.C., which Caesar may have calculated for the 1st, and there was another new moon on the 1st of March'. I have myself calculated the date of the new moon in question, first by reckoning back the number of lunations from the new moon of January 6, 1856, which occurred at 11.17 p.m., taking the length of a lunation to be 29 days, 12 hours, 44 minutes, 2·84 seconds, and allowing 2 hours for the secular acceleration of the moon's mean motion; and, secondly, by the method explained in Augustus De Morgan's *Book of Almanacs*, 1851, pp. xiv–xv. Both methods have led me to the same result, namely, that there was a new moon on January 2, 45 B.C.

[3] *Hermes*, xxiii, 1888, pp. 57–8.

[4] *Sat.*, i, 13, § 19.—dies ille quo abundare annum diximus eorum est permissus arbitrio qui fastis praeerant, uti, cum vellent, intercalaretur, dum modo eam in medio Terminaliorum vel mensis intercalaris ita locarent ut a suspecto die celebritatem averteret nundinarum. Atque hoc est quod quidam veterum retulerunt non solum mensem apud Romanos verum etiam diem intercalarem fuisse.

intercalary day was identical with the place which the intercalary day, whenever it occurred, had occupied before his reform; and (*b*) that the two intercalary months known as *mensis intercalaris prior* and *mensis intercalaris posterior* were added to the year 708 in order that the calendar year 709 might begin on the Kalends of January, and thus coincide with the consular year. If it be asked why they were called *prior* and *posterior* although another intercalary month had preceded them, the answer is easy: the other month ought in any case to have been intercalated in that year, whereas the *prior* and *posterior* were extraordinarily intercalated. The passages in Macrobius and Solinus on which Holzapfel relies have been already explained; and it has been shown that they do not necessarily bear the meaning which he ascribes to them. One fact alone appears to me to dispose of his contention, that the Julian calendar did not come into operation until the sixth day before the Kalends of March, 709: if it did not, why did January in that year contain 31 days, whereas in every previous year it had contained only 29?

Holzapfel also invokes the support of Böckh,[1] who remarked that it would have been unnatural for a reformer to correct the error caused by the difference of a quarter of a day between the civil and the solar year until the error required correction. The conclusion appears to Holzapfel inevitable that Caesar intended to make his first intercalation as soon as, and not before, the error should have amounted to one day, that is to say, in 713. Matzat,[2] on the contrary, maintains that logically the proper place for the intercalary day would have been immediately after the second year of the quadriennial cycle. But he does not believe that Caesar cared for such academic considerations. He undoubtedly fixed the place of the intercalary day in the year not on astronomical grounds, but according to usage. Why, then, asks Matzat, should it be considered improbable that he fixed the place of the intercalary year in the quadriennial cycle on the same principle?

3. Holzapfel[3] holds, as we have just seen, that the Caesarian cycle began on the day after the *Terminalia* of 709, that is to say, on the sixth day before the Kalends of March; that Caesar intended that the first intercalation should take place in 713; that the pontiffs, misunderstanding his directions, made the first intercalation at the beginning of the fourth year, that is to say, in 712; that, as Dion says, a day was extraordinarily intercalated in 713; that, to compensate for this extraordinary intercalation, a day was omitted in 714, which accordingly comprised 364 days only; that the pontiffs thenceforth intercalated every three years, namely in 715, 718, 721 . . . 745; that, to compensate for the three superfluous days which had been intercalated in consequence of the misunderstanding of Caesar's regulations, the intercalations which ought to

[1] *Ueber die vierjährigen Sonnenkreise der Alten*, 1863, p. 1.
[2] *Hermes*, xxiii, 1888, p. 56.
[3] *Röm. Chron.*, p. 328; *Philologus*, xlix, 1890, pp. 66–7, 72, 77.

have occurred in 749, 753, and 757, were omitted; and that the first intercalation after the reform of Augustus took place in 761.

This theory, as the reader will have already seen, cannot stand unless the evidence of Dion Cassius is to be rejected. Indeed it cannot stand even then. Holzapfel is not justified in assuming that in order to compensate for the extraordinary intercalation of 713, a day was omitted in 714; for, on his own theory, 714 was an ordinary year. Matzat[1] points out that, in the passage in which Dion[2] describes the omission of a day to compensate for the extraordinary intercalation—*καὶ δῆλον ὅτι ἀνθυφῃρέθη αὖθις, ὅπως ὁ χρόνος κατὰ τὰ τῷ Καίσαρι τῷ προτέρῳ δόξαντα συμβῇ*—the words *ἡμέρα ἐμβόλιμος* (an intercalary day) must necessarily be supplied, as the subject of the verb *ἀνθυφῃρέθη*, from the preceding sentence. Holzapfel retorts that his view does not involve the assumption of a change of subject. 'One can very well translate,' he says, '"an intercalary day was inserted, and self-evidently in turn omitted"' (Man kann sehr wohl übersetzen: 'es wurde ein Schalttag eingelegt und selbstverständlich wiederum in Abzug gebracht'[3]). This is not a satisfactory answer; for, on Holzapfel's own showing, the omitted day was *not* an intercalary day. There is no evidence that a day was ever withdrawn from an ordinary year in the Roman calendar; and, as Matzat[4] points out, the best proof that such a proceeding would have been regarded as out of the question is supplied by the procedure of Augustus. Instead of correcting the error of the pontiffs by withdrawing three days from one ordinary year, he omitted three intercalary days in three intercalary years, thus taking twelve years to accomplish a reform which, according to modern notions, might have been accomplished in one. If Dion's words are interpreted in their natural sense, they evidently mean that the next intercalary day which would have occurred, according to Caesar's regulations, was omitted. Thenceforth, accordingly, if the first intercalation occurred in 710, the intercalary years were 716, 719, 722 . . . 743. Or if, as Holzapfel insists, Dion's words, *παρὰ τὰ καθεστηκότα*, mean 'contrary to the regulations as erroneously interpreted by the pontiffs', and if, as he also insists, the year in which they first intercalated was 712, then the next year in which they would naturally have intercalated was 715: the extraordinary intercalation of 713 must have been compensated for by the omission of an intercalary day in 715; and the following series of intercalary years must have been 718, 721, 724 . . . 745. Again, Holzapfel's theory compels him to disregard silently the testimony of Solinus, on whose authority he lays such stress. Solinus[5] says that twelve days were intercalated in the first thirty-six years of the Julian calendar: according to Holzapfel, thirteen were intercalated.

Lastly, if we accept Holzapfel's view, that the first day of the

[1] *Hermes*, xxiii, 1888, p. 57.
[2] *Hist. Rom.*, xlviii, 33, § 4.
[3] *Philologus*, xlix, 1890, p. 76.
[4] *Hermes*, xxiii, 1888, p. 57.
[5] See p. 714, *supra*.

Julian calendar was the sixth day before the Kalends of March, 709, we find that a day *was* intercalated immediately after the end of the fourth year of the cycle, namely, after the *Terminalia* of 713. But Holzapfel assures us that, according to Macrobius and Solinus, this was not the case.

But I am not arguing against Holzapfel's theory as regards the first intercalary year of the Julian calendar; and what appears to tell most strongly in favour of it, besides the probability that the *Lepidianus tumultus* occurred in 711,[1] is the statement of Macrobius,[2] that Augustus enacted that the intercalation should take place 'every fifth year' (*quinto quoque anno*), that is to say, according to our reckoning, every four years. These words seem to imply that the pontiffs had actually misunderstood Caesar's regulation. On Matzat's theory, however, the pontiffs who intercalated *παρὰ τὰ καθεστηκότα* in 713 deliberately set that regulation aside in order to avoid the dreaded coincidence of the Kalends of January with a market-day. But, says Matzat, every three years this troublesome necessity recurred; and thus ultimately, as he suggests, the erroneous view might prevail that Caesar had himself intended to intercalate every three years ([anno] *quarto non peracto sed incipiente*[3]). But what right have we to assume that after 713 the pontiffs took any account of the nundinal superstition? At all events, if Holzapfel is right in maintaining that the first intercalary year of the reformed calendar was 712, there can be no doubt that the subsequent intercalary years were 713, 718 . . . 745; and it is therefore impossible for him to reconcile his view, that the first intercalation under the reform of Augustus occurred in 761, with the statement that Augustus allowed twelve years to pass without any intercalation. Augustus's first intercalation undoubtedly took place in 757; for in that year, if Caesar's regulation had been observed, the twelfth intercalation would have occurred.

Opinions may differ as to whether Matzat's theory or the modification of Holzapfel's which I have just suggested is the more probable. As, according to the latter, the series of intercalary years must have been 712, 713, 718, 721, 724, 727, 730, 733, 736, 739, 742, 745, it implies that after 713 the pontiffs thought it safe to disregard the nundinal superstition. So far my suggestion may be objectionable. On the other hand, it fits in with all the statements of the ancient writers, except that one remark of Dion Cassius which, as Holzapfel suggests, he may have made on erroneous information;[4] and particularly it fits in, as no other series which has been suggested does, with the statements that twelve days were intercalated in the first thirty-six years of the Julian calendar,[5] and that Augustus

[1] See pp. 719-21, *supra*.

[2] *Sat.*, i, 14, § 15.—post hoc unum diem secundum ordinationem Caesaris quinto quoque anno incipiente intercalari iussit, &c.

[3] *Hermes*, xxiii, 1888, pp. 52-3.

[4] See p. 719, *supra*.

[5] A. Mommsen, a brother of the great historian, has devised a singular theory of the working of the Julian calendar (*Philologus*, xlv, 1886, pp. 411-38), which Holzapfel (*ib.*, xlix, 1890, pp. 85-7) as well as Matzat (*Hermes*, xxiii, 1888, pp. 61 ff.) has conclusively refuted.

allowed the next twelve years to pass without any intercalation. However, the difference between Matzat's theory and mine (which is purely tentative) is unimportant; for they agree in the main point,—that the Kalends of January, 709, corresponded with the 1st of January, 45 B.C.

V. We have now gained the knowledge which will enable us to investigate the chronology of Caesar's second invasion of Britain. We have ascertained that the Kalends of January, 709, fell on the 1st of January, 45 B.C.; that 90 days were intercalated in 708, which accordingly consisted of 445 days; that a month of 23 days was intercalated in 702, which accordingly comprised 378 days; and that 701, 703, 704, 705, 706, and 707 were ordinary years, each comprising 355 days. It follows that the last day of 700, the year in which Caesar made his second expedition to Britain, corresponded with the 30th of November, 54 B.C., and that the sixth day before the Kalends of October, the day on which he wrote to tell Cicero that he was on the point of bringing back his army from Britain to Gaul, corresponded with the 29th of August. From these data it will be easy to ascertain the correspondence of any date in the year 700 which we find in our authorities with the Julian calendar.

After quitting Cisalpine Gaul, Caesar returned to his army, which had wintered in the country of the Belgae. He made a tour of inspection, visiting the various camps, which were of course in the immediate neighbourhood of the yards where the legionaries had been building the ships for his intended expedition, that is to say, at the Portus Itius (Boulogne), and probably on the estuaries of the Canche, the Authie, the Somme, and the Seine.[1] After ordering all the ships to assemble at the Portus Itius, he started with four legions in light marching order and 800 cavalry for the country of the Treveri, which, roughly speaking, comprised the greater part of the province of Luxembourg, the Grand Duchy of Luxembourg, and the southern part of Rhenish Prussia.[2] Two chiefs of this people, named Cingetorix and Indutiomarus, were struggling for supremacy. Cingetorix at once presented himself before Caesar, and promised fidelity. Indutiomarus collected levies, and prepared to fight. Many of the leading men, however, came into Caesar's camp and made terms for themselves. Indutiomarus found that he had miscalculated his strength, and hastened to excuse himself. Caesar, who had no time to spare, contented himself with taking hostages for his good behaviour and returned to the Portus Itius. About 25 days after his arrival, having meantime been detained by contrary winds, he sailed for Britain.[3]

The exact date of Caesar's departure from Cisalpine Gaul is uncertain. On the 2nd of June, that is to say, the 9th of May of the Julian calendar, Cicero received at Rome a letter from his brother, written at Placentia;[4] and on the following day he re-

[1] See p. 327, *supra*.
[2] Rice Holmes, *Caesar's Conquest of Gaul*, 1899, pp. 490–1.
[3] *B. G.*, v, 1–8.
[4] *Q. fr.*, ii, 13, § 1.

ceived another letter from his brother, written at Blandeno, a town near Placentia, the exact site of which is not known,[1] and also a letter from Caesar, written apparently at the same place.[2] Napoleon, erroneously maintaining that the second and the third of these letters were received by Cicero on the 5th, instead of the 3rd, of June, and assuming (' pour trouver le temps voulu ', as he naïvely remarks) that, in consequence of accidental delays, they took 13 days to reach Rome, concludes that Caesar quitted Blandeno on the 23rd of May, which he identifies with the 22nd [3] of the same month, but which really corresponded with the 29th of April of the Julian calendar. T. Bergk, on the contrary, maintains that if there had been any delay in the transmission of the letters, Cicero would have mentioned it in his reply ; and he supposes that Caesar and Quintus Cicero started on their journey for Transalpine Gaul on the 2nd of June, that is to say, on the 9th of May of the Julian calendar.[4] All that can be safely said is that, if the letter-carrier travelled at the usual rate, namely between 40 and 50 Roman miles a day,[5] the two letters were written about the 30th of April or the 1st of May of the Julian calendar ; and therefore we have no right to assume that Caesar quitted Blandeno before the former day.

It is possible, as we shall presently see, to fix the date of Caesar's arrival at the Portus Itius and of his voyage to Britain within a day or two ; but it is plainly impossible to make any satisfactory calculation of the dates of his movements from the time when he left Blandeno to the time when he left the country of the Treveri and marched for the Portus Itius ; and the minute computations of Napoleon and others are simply elaborate trifling.[6] For, although Caesar's *average* rate of travelling may be estimated approximately, we do not know how far he penetrated into the extensive country of the Treveri ; and it is waste of time to guess how long he stayed there. This much only can be said with certainty :—he did not let the grass grow under his feet.[7] For he left Blandeno about the 30th of April : he arrived, as we shall presently see,[8] finally at the Portus Itius about the 11th of June ; and in those 43 days he

[1] Napoleon (*Hist. de Jules César*, ii, 195, n. 4) arbitrarily identifies Blandeno with Lodi.

[2] *Q. fr.*, ii, 13, § 1.

[3] *Hist. de Jules César*, ii, 199.

[4] *Jahrbücher für classische Philologie*, 13 Supplementband, 1884, pp. 615, 620.

[5] See O. E. Schmidt, *Der Briefwechsel des M. Tullius Cicero*, pp. 201–5, 378–9. The distance from Placentia to Rome *via* Luca was 378 miles, *via* Ariminum 403. See *Itin. Ant.*, ed. Wesseling, pp. 124–7, 284, 287–8.

[6] *Jahrbücher für classische Philologie*, 13 Supplementband, 1884, pp. 615–20 ; Napoleon III, *Hist. de Jules César*, ii, 199.

[7] See O. E. Schmidt, *Der Briefwechsel des M. Tullius Cicero*, pp. 378–80. Caesar did occasionally, as Schmidt admits, travel at the rate of 100 Roman miles a day (Plutarch, *Caesar*, 17 ; Suetonius, *Divus Iulius*, 57. Cf. Caesar, *B. C.*, i, 3, § 6). In 1852, Lord Dalhousie rode and drove from Benares to Barrackpore, a distance of 400 miles, in 80 hours, including stoppages ; and in the same year General Godwin travelled from Meerut to Calcutta—over 950 miles—in 11 days (Sir W. Lee-Warner's *Life of the Marquis of Dalhousie*, i, 1904, pp. 403, 422).

[8] See p. 730, *infra*.

travelled from Blandeno across the Alps and across Gaul to the English Channel; moved along the coast to various points between Boulogne and the mouth of the Seine; and marched from Boulogne to the neighbourhood of Sedan, or further—at least 180 miles—and back again.

On the 27th of July, that is to say, on the 2nd of July of the Julian calendar, Cicero wrote to Atticus, 'Judging from my brother Quintus's letters, I imagine that by this time he is in Britain' (*ex Q. fratris litteris suspicor iam eum esse in Britannia* [1]). It would be very rash, however, to infer from this that Caesar landed in Britain before, or even as early as, the 2nd of July; for, as we have seen, his embarkation was delayed by the long continuance of adverse winds. The first letter which announced the arrival of the expeditionary force in Britain was referred to by Cicero in a letter to Quintus, in which he says, 'How I rejoiced at your letter from Britain! I was nervous about the sea and the coast of that island' (*O iucundas mihi tuas de Britannia literas! Timebam Oceanum, timebam litus insulae* [2]). This letter is undated; but it must have been written some time after the one which Cicero wrote to Atticus on the 27th of July; for in the letter to Atticus we find the words, 'I have undertaken to defend Messius . . . After that I have to prepare myself for Drusus, and then for Scaurus' (*Messius defendebatur a nobis . . . Deinde me expedio ad Drusum, inde ad Scaurum* [3]); while in the letter to Quintus Cicero wrote, 'The day I write this Drusus has been acquitted . . . The *comitia* have been put off to September. Scaurus's trial will take place immediately' (*Quo die haec scripsi, Drusus erat . . . absolutus . . . Comitia in mensem Septembrem reiecta sunt. Scauri iudicium statim exercebitur* [4]). Asconius [5] tells us that the last day of Scaurus's trial was the 2nd of September; and Cicero's remark that 'the *comitia* have been put off to September' makes it evident that he wrote in August; while from his saying that 'Scaurus's trial will take place immediately' we should naturally infer that when he wrote the 2nd of September was not far off. Letters from Britain generally reached Rome in about 27 days; [6]

[1] *Att.*, iv, 15, § 10.
[2] *Q. fr.*, ii, 15, § 4.
[3] *Att.*, iv, 15, § 9.
[4] *Q. fr.*, ii, 15, § 3.
[5] Asconius, *in Scaurianum*, p. 18 (*M. Tullii Ciceronis opera*, ed. Orelli and Baiter, vol. v, pars ii, 1833).—Summus iudicii dies fuit a. d. IIII Non. Septembr.
[6] 'Caesar,' writes Cicero, 'wrote me a letter from Britain on the 1st of September, which reached me on the 27th' (*Ex Britannia Caesar ad me K. Septembr. dedit litteras, quas ego accepi a. d. IIII K. Octobr.* [*Q. fr.*, iii, 1, § 25]). 'Your fourth letter,' he tells Quintus, 'reached me on the 13th of September, dated on the 10th of August from Britain' (*Quarta epistola mihi reddita est Idibus Sept., quam a. d. IIII Idus Sext. ex Britannia dederas* [*ib.*, 1, § 13]). And, as we have already seen, letters from Caesar and Quintus, written on the British coast on the 25th of September, reached Cicero on the 24th of October. The extraordinarily long time—33 days—which Quintus's 'fourth letter' took to reach his brother may easily be accounted for: Cicero was not at Rome when he received it, but at Laterium, near Arpinum, about 70 Roman miles E. by S. of Rome (*ib.*, iii, 1, § 4).

Napoleon insists (*Hist. de Jules César*, ii, 196, n, 3) that, in favourable circum-

and accordingly we may conclude that the letter in which Quintus Cicero announced his arrival in Britain was written about the end of July; that is to say, about the 6th of July of the Julian calendar.[1] Now Caesar says that the tide [in the Straits of Dover] turned westward soon after daybreak on the morning of his arrival in Britain;[2] and this statement proves that he landed either *about* the time of full moon or *about* the time of new moon. There was a full moon on the 21st of July, 54 B.C.; and the previous new moon occurred on the 7th of July. Napoleon[3] insists that the landing must have taken place on the day of full moon, arguing that without moonlight Caesar could not have undertaken the march which he made on the night following his arrival. But, as we have already seen, Napoleon argues on the erroneous assumption that Caesar intercalated only 67 days in the year 708, and accordingly he fixes all the dates of the unreformed calendar which occur in Cicero's letters of 700 twenty-three days too late. His argument that

stances, letters only required 20 days for transmission from Britain to Rome. This view is based upon a passage in one of Cicero's letters (*Q. fr.* iii, 1, § 17) which, in the MSS., runs as follows:—*tabellarii a vobis venerunt a. d. XI K. Septembr. vicesimo die* ('letter-carriers arrived from you and Caesar on the 22nd of August after a journey of 20 days'). It is obvious, and is universally admitted, that (unless Cicero made a slip) *Septembr.* is wrong, and that Cicero meant 'the eleventh day before the Kalends of October', that is to say, September 20. It is equally obvious that he did not write *vicesimo*, or that, if he did, he made a mistake. For, at the end of the letter, he says (as we have already seen), 'Caesar wrote me a letter from Britain on the 1st of September'; and, as the letter from Quintus reached him on the 20th of September, it must have been dispatched, if it really arrived *vicesimo die*, on the 1st of September, that is to say, on the same day as the letter from Caesar. But this, as Dr. Vogel remarks (*Neue Jahrbücher für Philologie*, &c., cliii, 1896, pp. 273–4), is disproved by the fact that Caesar, in this very letter, begged Cicero not to be alarmed at not having received a letter from Quintus by the same messenger, as Quintus was not with him when he reached the coast (*Ex Britannia Caesar ad me K. Septembr. dedit litteras . . . quibus, ne admirer, quod a te nullas acceperim, scribit se sine te fuisse, cum ad mare accesserit*). As it is clear, therefore, that *vicesimo* is wrong, various attempts have been made to amend the MS. reading. Bergk (*Jahrbücher für classische Philologie*, 13 Supplementband, 1884, p. 622) arbitrarily changes *vicesimo* to *tricesimo*. The most satisfactory conjecture, in my opinion, is that of C. Bardt (*Quaest. Tullianae*, 1866, p. 32). He believes that what Cicero wrote was *a. d. XI Kal., septimo vicesimo die*; that a copyist abbreviated this into *a. d. XI Kal., sept. vicesimo die*; and that this was corrupted into *a. d. XI Kal. Sept., vicesimo die*.

If Professor Tyrrell, who reads *a. d. XI Kal.* [*Sept.*] *vicensimo die* (*Correspondence*, ii, 1886, p. 150), reads this note, I am confident that he will allow his text to be emended in the next edition of his and Dr. Purser's great work.

[1] There can, I think, be little doubt that Quintus wrote and dispatched this letter on the very day of his arrival, or, at the latest, before the storm which totally wrecked 40 of Caesar's ships on the next night but one after his arrival. If the storm had occurred when he wrote, he would surely have mentioned it; and there is not a word in Marcus Cicero's reply which would lead us to suppose that he had done so. Moreover, Quintus knew that Marcus was waiting impatiently for news; and Caesar would naturally have desired to communicate at once with Labienus whom he had left in command in Gaul.

[2] *B. G.*, v, 8, § 2.—longius delatus aestu orta luce sub sinistra Britanniam relictam conspexit. Tum rursus aestus commutationem secutus, &c.

[3] *Hist. de Jules César*, ii, 198.

Caesar could not have made a night march except by the light of the moon is worthless. It must be remembered that, in the first half of July, there is no real night over any part of the British Isles; and no one familiar with the records of night marches would deny that Caesar could have marched on a clear night at that time of the year. Long[1] says that 'of course he had also the moon on the night on which he sailed from the Gallic coast'. There is no 'of course' in the matter. Caesar sailed from Gaul to Britain in 55 B.C. after midnight, on the 26th or 27th of August, that is to say either five or four days before the full moon;[2] and the moon set soon after midnight both of August 25–26 and of August 26–27. Moreover, according to Napoleon himself, Caesar sailed from Britain to Gaul in 55 B.C. on the 12th of September,[3] that is to say, on a moonless night: as new moon occurred on the 14th, and he did not sail until after midnight, he could not have had the benefit of moonlight unless he had deferred his voyage until the date of the equinox; and I am assured by Captain Iron, the harbour-master of Dover, that on a fine night, especially in July, there would not have been the least difficulty in sailing without a moon. As a matter of fact, William the Conqueror sailed to England on a dark night. 'The moon,' says Mr Freeman,[4] 'was hidden and the heavens were clouded over. The Duke therefore ordered every ship to bear a light. . . . The ships were to keep as near together as might be, and to follow closely after the beacon-light of his own ship.' If, then, we decide that Caesar landed in Britain on the day of new moon, the 7th of July, 54 B.C., we shall not be more than one day wrong; but to fix the date with absolute precision is impossible.[5]

As Caesar landed about the 7th of July, it follows that he had reached the Portus Itius, where he was delayed about 25 days, about the 11th of June.

On the day after he landed Caesar encountered a British force 12 miles from his camp on the coast. On the following day, while his troops were pursuing the fugitives, he was recalled to the coast by the news that a large number of his ships had been damaged by a storm. He then proceeded to construct a naval camp, and, as soon as it was finished, returned to the point from which he had started.[6] As the construction of the camp occupied 'about

[1] *Decline of the Roman Republic*, iv, 205.
[2] See pp. 600-3, *supra*.
[3] See pp. 706-7, *supra*.
[4] *Norman Conquest*, iii, 399.
[5] Dr. F. Vogel, who rightly concludes that Caesar could not have sailed on the 20th of July, has recourse to an unsatisfactory argument to prove his case. We know, he says (*Neue Jahrbücher für Philologie*, &c., cliii, 1896, p. 275), what misfortunes Caesar had met with in the preceding year owing to the high tide which was raised by the full moon: how then can we believe that he would have chosen the day of full moon for his second expedition? But Dr. Vogel himself argues that Caesar sailed about the 8th of July, the day after new moon. Did not the doctor forget that the tidal phenomena at full and new moon are nearly identical, and that the 8th of July was the very day on which a spring-tide occurred? If Caesar was himself unaware of these facts, his Gallic seamen could have enlightened him. Moreover, he must have known that at least one full moon would occur while he remained in Britain.
[6] *B. G.*, v, 9–10; 11, §§ 1–7.

ten days' (*circiter dies X*), we shall not be far wrong if we say that it was finished 12 days after the landing, that is to say, about the 19th of July of the Julian calendar.

We now come to a date the significance of which has hardly been appreciated. At the close of one of his letters to Quintus, Cicero writes, 'Caesar wrote me a letter from Britain on the 1st of September . . . in which, to prevent my wondering at not getting one from you, he tells me that you were not with him when he reached the coast' (*Ex Britannia Caesar ad me K. Septembr. dedit litteras . . . quibus, ne admirer, quod a te nullas acceperim, scribit se sine te fuisse, cum ad mare accesserit*[1]). This passage proves that Caesar had returned from the interior of Britain to the coast on or before the 1st of September, that is to say, the 5th of August of the Julian calendar. But we have already seen that he did not quit the coast after the construction of his naval camp until about the 19th of July, and that on the 29th of August he was still in Britain, 'on the point of bringing back the army,' and did not sail for Gaul till several days later.[2] We have to decide, then, between two alternatives. Either Caesar had finished the campaign against Cassivellaunus by the 5th of August of the Julian calendar, and thereafter remained on the coast until the 29th of August, when he was able to announce that he was 'on the point of bringing back the army'; or he made a hurried temporary visit to the coast, the object of which remains unexplained. The latter view is not supported by the *Commentaries*. Caesar's narrative certainly leaves the impression that, immediately after the completion of his naval camp, he resumed the military operations which had been interrupted by the shipwreck, and did not again return to the coast until the time came for him to prepare for his voyage to Gaul. He tells us that Cassivellaunus, after the failure of the attack which had been made by his orders upon the naval camp, and in consequence of the reverses which he had suffered, sued for peace; that he ordered Cassivellaunus to furnish hostages;[3] and that, 'on receiving the hostages, he led back the army to the sea, where he found the ships repaired' (*Obsidibus acceptis exercitum reducit ad mare, naves invenit refectas*[4]). 'When they were launched,' he continues, 'he determined to take the army back in two trips' (*his deductis . . . duobus commeatibus exercitum reportare instituit*[5]). Certainly there is not a word in this to support the view, which is advocated by Vogel, that the visit to the coast which Caesar made on the 1st of September (the 5th of August of the Julian calendar) was purely temporary, and took place *before* he began to march towards the country of Cassivellaunus; and the motive which Vogel[6] suggests for the visit—that Caesar wished to inspect the camp once more and to give the necessary instructions before marching against Cassivellaunus—is hardly adequate, unless we grant Vogel's assumption,

[1] *Q. fr.*, iii, i, § 25.
[2] See pp. 712 3, 726, *supra*.
[3] *B. G.*, v, 22, §§ 3–4.
[4] *Ib.*, 23, § 1.
[5] *Ib.*, 23, § 2.
[6] *Neue Jahrbücher für Philologie*, &c., cliii, 1896, p. 280.

that when Caesar returned to the sea the campaign had advanced no further than the stage which he describes in the 17th chapter of his Fifth Book, where three legions under Trebonius inflicted a decisive defeat on the enemy; in other words, that Caesar returned from a point within a day's march of the sea. But this affair, according to the *Commentaries*, took place on the very day after Caesar quitted his naval camp in order to resume the campaign; whereas he returned to the sea, on Vogel's own showing, about 17 days after he quitted the camp. If, on the other hand, we adopt Napoleon's view[1]—that Caesar, after he returned to the sea on the 1st of September, remained there until he sailed for Gaul—how are we to account for the 24 days which elapsed before he wrote to tell Cicero that he was on the point of bringing back the army? What was he doing all that time? Napoleon,[2] indeed, maintains that, on the day on which he wrote this last letter, he actually sailed for Gaul, and Bergk that he had already arrived in Gaul: but it has already been shown that both these assumptions are untenable. Moreover, in order to account for Caesar's having arrived at the sea so early, Napoleon and Bergk are forced to strain the words of the *Commentaries* (*exercitum reducit ad mare*),—to assume that Caesar hurried on in advance of his army, and that it did not reach the coast until several days later.[3]

[1] *Hist. de Jules César*, ii, 194, 199. [2] *Ib.*, pp. 198–9.

[3] Trebonius defeated Cassivellaunus about the 20th of July of the Julian calendar; and we may assume that Caesar did not begin to march towards the territories of Cassivellaunus until the following day. By the 5th of August he had returned to his naval camp. In those 17 days he marched to the Thames; crossed it at or near Brentford; marched on through the territory of Cassivellaunus into that of the Trinovantes (Essex); marched thence to the stronghold of Cassivellaunus, which was not far off; captured it in a single day; and marched back to the coast. Altogether the distance that he marched cannot have been less than about 200 miles. Evidently, therefore, he would not have had time enough to negotiate with Cassivellaunus and to receive the hostages whom he demanded before he returned to the coast.

Bergk insists (*Jahrbücher für classische Philologie*, 13 Supplementband, 1884, pp. 616–8) that the campaign must have been finished at the beginning of August of the Julian calendar, because Caesar (*B. G.*, v, 22, § 4) tells us that when it was finished the summer was nearly at an end, and, according to Caesar himself, autumn began on the 11th of August. But when Bergk says that, according to Caesar, autumn began on the 11th of August he seems to forget that this date was fixed in the Julian calendar, eight years after the invasion of Britain. He also forgets that the word *aestas*, in the *Commentaries*, denotes, not a season which ended on a fixed date, but the period during which campaigning was practicable; and two passages prove that it extended at least as far as the middle of September. In the last chapter of his First Book Caesar remarks that in a single 'summer' he had finished two important campaigns (*una aestate duobus maximis bellis confectis*); and it has been proved that the decisive battle of the second campaign was fought about the 14th of September (Rice Holmes, *Caesar's Conquest of Gaul*, p. 642). In the 20th chapter of the Fourth Book he says that he determined to invade Britain (in 55 B.C.), although only a small part of the 'summer' remained, and in this part of the world 'winter' set in early (*Exigua parte aestatis reliqua Caesar, etsi in his locis . . . maturae sunt hiemes, tamen in Britanniam proficisci contendit*); and we know that he did not land in Britain until the 26th of August (see pp. 600–3, *supra*). The second passage, moreover, is one of many which prove that

Although the problem cannot be definitively solved, I have no doubt but that Vogel's solution is wrong. Not only does it give the lie to Caesar's narrative, but it requires us to believe that Caesar had failed for about 17 days to make any headway against the Britons, and had been held in check by them within a dozen miles of the sea. Yet Caesar states that, in consequence of the defeat which Cassivellaunus suffered at the hands of Trebonius on the day after he left the newly-constructed naval camp, the British infantry levies dispersed; and in the same breath he goes on to say that he forthwith marched for the country of Cassivellaunus.[1] If, as Vogel implies, this statement had been false, surely Quintus Cicero would have informed his brother, in one of the five letters which he wrote before the 1st of September, of the real state of affairs! On the other hand, I find it difficult to believe that Caesar used the words *exercitum reducit ad mare* loosely, and that he remained on the coast from the 1st to the 25th of September, without once writing to Cicero between those two dates,[2] and then remained several days longer before embarking. This view, indeed, would compel us to assume that he left Trebonius or one of his other generals to carry on the negotiations with Cassivellaunus which are described in the 22nd chapter of his Fifth Book.

Napoleon, it is true, believes that Caesar did not leave his army and hurry on in advance of it to his naval camp until the negotiations were completed and he had received his hostages from Cassivellaunus.[3] But on this assumption what motive could he have had, first, for hurrying on in advance of his army, and, secondly, for delaying its re-embarkation until after the lapse of several weeks? I am inclined, therefore, to believe that he made a hurried temporary visit to his naval camp, escorted probably by a small column, and then, having accomplished his purpose, returned to the main army in order to conduct or to complete the negotiations with Cassivellaunus. The motive of his visit may have been connected with the attack which the Kentish chieftains made upon the naval camp.[4]

The date of Caesar's return to Gaul can only be given approxi-

Caesar generally took no account of spring and autumn, but (like Thucydides) divided the year into two seasons,—*aestas*, the season in which campaigning was practicable, and *hiems*. He only once uses the word *ver* (spring), namely, in *B. G.*, vi, 3, § 4; and only three times—once only in the *Gallic War* (vii, 35, § 1), twice in the *Civil War* (iii, 2, § 3; 87, § 3)—uses the word *autumnus*; and in none of these four passages is there any reference to campaigning. The Latin word for 'winter', properly so called, is not *hiems* but *bruma*.

[1] *B. G.*, v, 17, § 5—18, § 1.—Ex hac fuga protinus quae undique convenerant auxilia discesserunt, neque post id tempus umquam summis nobiscum copiis hostes contenderunt. Caesar cognito consilio eorum ad flumen Tamesim in fines Cassivellauni exercitum duxit.

[2] About the 20th of October Cicero wrote to his brother (*Q. fr.*, iii, 3, § 1), 'for more than fifty days I have heard nothing from you or from Caesar' (*dierum iam amplius quinquaginta intervallo nihil a te, nihil a Caesare . . . adfluxit*). The last letter which he had received was the one written by Caesar on the 1st of September.

[3] *Hist. de Jules César*, ii, 194.

[4] *B. G.*, v, 22, §§ 1-3.

mately. We have seen that on the 25th of September (the 29th of August of the Julian calendar) he wrote to Cicero, saying that he was on the point of bringing back the army.[1] Vogel[2], remarking that, after he reached the coast, his ships had to be launched and loaded, and that he did not sail with the second detachment of troops until he had waited a long time in vain for the return of the ships which had carried the first, concludes that he did not return to Gaul until about 20 days after he wrote to Cicero, that is to say, not until the 15th of October. This date corresponds with the 17th of September of the Julian calendar; and Vogel maintains that it agrees with Caesar's statement of the reason which led him to hurry on his return, namely, that the equinox was at hand. But it is impossible to estimate, from Caesar's statement, how many days he waited for the return of his ships. Let us examine the attempts which have been made to gain a clue from Cicero's correspondence.

Vogel[3] points out that Cicero,[4] in a letter written just after the 23rd of November—the 26th of October of the Julian calendar—referred to two letters which he had received from Quintus, and also to a third, which Quintus had handed to Labienus for transmission the day before he dispatched the earlier of the other two, but which had not yet arrived. Now Labienus had remained in Gaul during the invasion of Britain.[5] It is clear, therefore, that these three letters were not written until after Quintus had returned to Gaul. On the other hand, they would seem to have been the first letters which Quintus wrote to his brother after his return. For Cicero, in the letter in which he referred to them, said, 'where your Nervii dwell and how far off, I have no idea' (*Ubi enim isti sint Nervii et quam longe absint, nescio*[6]). The Nervii were the tribe in whose country Quintus, with his legion, was to pass the winter.[7] Evidently Quintus, when he wrote the two letters which Cicero received, had not yet reached the country of the Nervii; for otherwise he could not, on the previous day, have been with Labienus, who was to winter in the country of another tribe. Probably, as Vogel concludes, he wrote from Samarobriva, or Amiens, where Caesar had his head quarters and where the arrangements for the distribution of the legions were made.[8] Again, in the letter which has been already quoted, Cicero wrote to Quintus, 'Pray be careful to let me know to whom I am to give the letter which I shall then send you,—to Caesar's letter-carriers, for him to forward it direct to you, or to those of Labienus?' (*Tu velim cures ut sciam quibus nos dare oporteat eas quas ad te deinde litteras mittemus, Caesarisne*

[1] See p. 726, *supra*.
[2] *Neue Jahrbücher für Philologie*, &c., cliii, 1896, p. 284.
[3] *Ib.*, pp. 284–5.
[4] *Q. fr.*, iii, 8, § 1.—Superiori epistolae quod respondeam, nihil est; quae plena stomachi et querelarum est, quo in genere alteram quoque te scribis pridie Labieno dedisse, qui adhuc non venerat. Delevit enim mihi omnem molestiam recentior epistola.
[5] *B. G.*, v, 8, § 1; 23, § 4.
[6] *Q. fr.*, iii, 8, § 2.
[7] *B. G.*, v, 24, § 2.
[8] *Ib.*, 24, § 1; 46; 47, § 1.

tabellariis, ut is ad te protinus mittat, an Labieni[1]). Vogel remarks that 'this question is only intelligible on the hypothesis that Quintus was only just beginning to take up his quarters in Gaul' at the time when he wrote the two letters which Cicero had just received. Now a letter would have required about 25 days for transmission from Samarobriva to Rome;[2] and accordingly the letters to which Cicero referred, assuming that he replied to them promptly, would have been written about the end of October. Vogel, who thinks that they must have been written within a fortnight after Quintus and Caesar returned to Gaul, infers that they cannot have returned earlier than the 15th of October, the 17th of September of the Julian calendar. His reasoning is ingenious; but unfortunately we do not know exactly how soon after the receipt of his brother's second letter Cicero wrote, or how many days intervened between the arrival of the first and of the second.

On the whole, it appears to me that all we can say for certain regarding the date of Caesar's return is this. It cannot be fixed earlier than several days after the 29th of August of the Julian calendar,—the date of the letter in which he informed Cicero that he was on the point of bringing back the army. Bearing in mind that it occurred when 'the equinox was at hand', we may place it about the middle of September.

TOPOGRAPHICAL NOTES

On April 25, 1902, I observed more carefully than I had ever done before the coast between Sandgate and West Hythe. To speak of the hills between Sandgate and Hythe as *angusti montes* is sheer nonsense. Caesar would never have attempted to force a passage inland at any point between Lympne and Sandgate;[3] nor would the Britons have abandoned these *loca superiora*, which lay ready to hand. There are, indeed, depressions in the line of hills—(1) just west of Sandgate railway station, (2) nearly opposite the Seabrook (now Imperial) Hotel, and (3) west of Hythe, just west of the point where the road diverges from the military canal; but if Caesar had attempted to force these gaps, he would have found himself entangled in the hills behind. 27.4.02.

. . . . . .

Quite recently I explored the easternmost and the westernmost of the three valleys which partially break the continuity of the hills behind Hythe. Neither would have been [reasonably] practicable for an invading army [in the conditions of ancient warfare]. The road leading through the former, which branches off from the road

[1] *Q. fr.*, iii, 3, § 4.

[2] See p. 728, n. 6, *supra*.

[3] Of course I do not mean that he would not have attempted to do so in any conceivable circumstances; but that Volusenus would never have advised him to undertake such an operation when there was the alternative of landing between Walmer and Sandwich. 5.10.06.

[running from Sandgate] to Hythe, is level for the first 120 yards, and then ascends rapidly for a short distance. Then it is tolerably level until about 100 yards before one gets to the cross-road which turns off to the left, when it ascends rapidly for a long way. A column moving along it would have been exposed to attack from the hills on either side, and particularly on the west. 3.9.03.

. . . . . .

On the return voyage from Boulogne [September 5, 1903] I most carefully scrutinized the whole coast-line between Sandgate and the Foreland, as I had often done before on land. Caesar's description of the *angusti montes* is applicable only to Shakespeare's Cliff and the cliffs which extend from the Castle Hill to the neighbourhood of Kingsdown. It is not applicable even to the imposing heights which bound East Wear Bay, because, although they might fairly be called *angusti montes*, the missiles of which Caesar speaks could only have been thrown on to the beach from the precipitous but low chalk cliffs which form the lowest part of these heights; whereas he plainly means that the missiles would [or rather, could] have been thrown by the enemy who were standing *in omnibus collibus*; and if he anchored off East Wear Bay, the *colles* were a quarter of a mile or more from the sea. The low chalk cliffs of East Wear Bay would never have been called *montes*, although they are the lowest part of a range of *montes*. The notion that the cliffs between East Wear Bay and Folkestone Harbour, or the cliffs on which the 'Leas' stand, as seen from a vessel half a mile from the shore, would have been called *angusti montes* or *montes* at all is simply ludicrous. No! It is absolutely certain that Caesar's *angusti montes* were the cliffs of Dover,—the cliffs between [and including] the Castle Hill and the Foreland. And as for Airy's theory, how could cliffs 'ten to thirty feet high' have been called *angusti montes* by an observer standing on the deck of a ship five nautical miles away?[1]

[Cicero's description (*Att.*, iv, 16, § 7) of the cliffs which 'walled in the approaches to the island'—*mirificis molibus*—is applicable only to the heights behind East Wear Bay and the cliffs of Dover.]

. . . . . .

The following notes were jotted down on September 15, 1902, on the deck of a steamer running from Dover to Deal:—

Six *colles* at present visible off Dover Harbour, i.e. on east of old [pre-Roman and Roman] harbour: one on left, not counting Shakespeare's Cliff.

Eight from off the Foreland. Nothing could be more appropriate than the expressions *in omnibus collibus expositas* [hostium copias] and *montes angusti*. In the various 'dips' the *hostium copiae* would have been very conspicuous.

. . . . . .

Behind the low rampart between Kingsdown and Walmer Castle the ground rises. Caesar might have encamped [in his first expedition]

[1] See pp. 613–4, *supra*.

on this rising ground or on the rising ground which extends behind the castle towards the church.

Just after passing Deal Castle one turns to the left down Gilford Road, and, after walking about 300 yards, passes under a railway bridge, and then, very gradually ascending for a few paces, walks along a path, which crosses fields. It seemed to me just possible that C. might have encamped on these fields, but very—to the last degree—unlikely.

The only suitable camping-grounds that I can see anywhere are those mentioned above. 16.9.02.

. . . . . .

Walked over the cliffs from St. Margaret's to Walmer. . . . After walking for a short distance along the edge of this [natural] rampart [south of Walmer Castle], I struck inland along a fence, and came to a path which traverses a rolling chalk plateau, and runs exactly in a straight line with the tower of St. Mary's Church, Walmer. There is splendid camping-ground on this plateau. The camp would have commanded the approaches from every point of the compass; and the descent from the plateau on the west or landward side is steep. Beyond the valley which bounds the plateau on this side the ground rises again fairly rapidly. The plateau extends northward to a point just south of Walmer Castle, which it commands. If Caesar encamped on the plateau, the camp could be discovered by excavation: but if he encamped on the gently rising ground north-west of the plateau, the camp could not be discovered; for this ground is covered by buildings. (I assume of course that he did not encamp on the high ground west of the plateau or on the ground on which the windmill stands; for both sites are too far from the sea.) 17.9.02.

ADDENDA

PAGE 122. 'There was certainly a Copper Age . . . Ireland.' Professor Gowland (*Journ. Anthr. Inst.*, xxxvi, 1906, pp. 26–7) deprecates the use of the expression 'Copper Age', remarking that 'the so-called Copper Age possesses no characteristics which are not common to the Neolithic Age, except the imitations and limited use of stone forms in metal', &c. The question seems to be purely verbal.

PAGE 140. 'It is worthy of remark . . . not pure.' The proportion of lead in Scottish bronze implements appears to have been remarkable; but Professor Gowland (*Journ. Anthr. Inst.*, xxxvi, 1906, p. 30) observes that lead is 'found in small quantities in nearly all bronze implements'. See, however, J. Evans, *Anc. Bronze Implements*, p. 417.

PAGE 148. 'All the open ones . . . sand.' Professor Gowland (*Journ. Anthr. Inst.*, xxxvi, 1906, p. 36) affirms that 'moulds of sand or loam were undoubtedly of later times [in the Bronze Age], as there are considerable mechanical difficulties in preparing them'.

PAGE 194, note 3. To the list of counties in which drinking-cups have been found must now be added Kincardineshire (*Proc. Soc. Ant. Scot.*, xl, 1906, pp. 304–6).

PAGE 205, note 4. To the list of papers on cup- and ring-markings may be added *Proc. Soc. Ant. Scot.*, xl, 1906, pp. 318–27.

PAGE 208. 'Stone circles . . . Kincardineshire.' A circle in Stirlingshire is described in *Proc. Soc. Ant. Scot.*, xl, 1906, pp. 301–4.

PAGE 211, note 2. To the list of papers on Scottish stone circles may be added *Proc. Soc. Ant. Scot.*, xl, 1906, pp. 164–206, 245–54.

PAGE 221. 'They told him . . . ankle-bones.' This statement rests upon the reading *ἀστραγάλων* in Diodorus Siculus, v, 22, § 2. Professor Ridgeway (*Folk-Lore*, i, 1890, p. 83, n. 1) suggests that the true reading may be *ἀστραβῶν* (saddles).

PAGE 237. 'Coral . . . later period.' M. S. Reinach (*Rev. arch.*, 4e sér., vi, 1906, pp. 309–10) argues that the development of enamelling in Gaul was due to the growing dearness of coral, the price of which rose because large quantities were bought for exportation to India after the conquests of Alexander the Great. This view leaves unexplained the continued use of coral in Britain long after it had fallen into disuse in Gaul.

PAGE 259, note 3. In a recent article on vitrified forts (*Proc. Soc. Ant. Scot.*, xl, 1906, pp. 136–50) Lieut.-Col. A. B. McHardy suggests that some of them may have been built in the time of the Vikings, and argues that the vitrifaction was probably never intentional, because (1) 'in various forts . . . the loose stones below the vitrification (*sic*) are supported by ordinary masonry, which apparently might have been carried up the whole way had the builders so desired,' and (2) the greatest amount of vitrifaction is generally found 'where a strong parapet is least needed'. He also thinks that the forts were used for signalling, and that the vitrifaction was produced by smouldering beacon fires.

PAGE 288, note 1. Canon Greenwell, in a paper to which I have already referred (p. 676, n. 6), states that in the 'Danes' Graves' 'by far the larger number [of bodies] were laid on the left side, and . . . about one-

third had the head pointing to north-east. It does not seem, however,' he adds, 'that the dead were laid with the face turned towards the sun.' He also observes that 'the presence of charcoal in close proximity to an unburnt body, so universal a feature in the graves of the Bronze Age [on the Yorkshire Wolds] was not noticed except in a few cases, at the Danes' Graves, and in these it may only have been there accidentally'.

PAGE 379, note 3. I omitted to mention Professor Karl Pearson's method of estimating stature. Dr. Beddoe, who has criticized it in *Journ. Roy. Inst. Cornwall*, XV, 1902, pp. 163–71, thinks that it 'probably underestimates the stature of tall men', and observes that in one case, where the actual height of living men had been ascertained, Professor Pearson's calculations erred considerably more than his own or M. Manouvrier's.

PAGE 408, note 4. See also *Biometrika*, iii, 1904, pp. 243–4.

PAGE 490, note 5. Further evidence of tin mining in the Scilly Islands will be found in *Memoirs Geol. Survey,—The Geology of the Isles of Scilly*, 1906, pp. 10, 11.

PAGES 504–5. 'Is he not aware . . . distinguished.' C. Müller, in his edition of Ptolemy's *Geography* (p. 106), argues that Pliny's Vectis cannot be the Vectis which was the Isle of Wight, because he mentions it among the islands between England and Ireland. But if this argument is sound and Pliny did not blunder, it remains true that there is no evidence in Pliny or any other writer for identifying Ictis with Vectis.

PAGE 527, note 9. Mr. Spurrell has proved that a slight subsidence has taken place in the London district since the Roman occupation. See also p. 566, *supra*.

PAGES 581–3. 'There is one passage . . . anchorage.' The argument in this paragraph depends upon the assumption that the ships which were 'carried back' (*referrentur*) in 55 B.C. to the port from which they had started were laid to, and that they could not lie within less than four points and a half of the wind, or, if, with Falconer and James Smith, we allow six points for leeway, within less than two and a half. It may not perhaps be impertinent to explain that when a ship is laid to her head is brought as close to the wind as possible without losing way, under little sail. In these circumstances the helm is often lashed, and the ship then has a tendency to yaw, that is, to fall off the wind and come up again alternately. An experienced seaman with whom I have recently discussed the matter believes, however, that it might have been practicable for the captains of the ships in question to adopt the plan of 'head reaching', that is, working to windward under comparatively low sail but still enough to enable the steersmen to keep the ships under control, to luff up to dangerous seas, and to make considerable headway. As he remarked, the difference between lying to, when the helm is not lashed, and 'head reaching' is simply one of degree: in 'head reaching' the vessel carries a little more sail; and, judging from illustrations of ancient ships, he thinks that Caesar's would have been able, when 'head reaching', to lie within two points of the wind. Allowing five points for leeway, he holds that they could have made good a course within seven points: and he insists that an ancient ship with its sails properly trimmed could have gone as near the wind as a modern lugger!

This, I need hardly say, is rank heresy, and I do not believe that any one who is conversant with the literature of ancient navigation will accept it. Besides, we do not know whether Gallic ships were rigged exactly like those of the Mediterranean. James Smith (*The Voyage and Shipwreck of St. Paul*, 1880, pp. 75, 127, 215) may or may not have been right in maintaining that an ancient ship could not make good a course in moderate weather within less than about seven points of the wind; but numerous passages prove that ancient ships, if they could work to windward, could

not do so without difficulty. If my heretical friend is right, why were Caesar's cavalry transports windbound in the *ulterior portus*, less than eight miles from the port which their captains wished to make, and why were they obliged by contrary winds to put back, even in moderate weather, after they first started from the *ulterior portus*? M. Jules Vars, indeed (*L'art nautique dans l'antiquité*, 1887, pp. 181 ff.), apparently doubts whether ancient ships could work to windward at all; but a passage which James Smith quotes from one of Cicero's letters (*Fam.*, xiv, 5, § 1)—*cum sane adversis ventis usi essemus tardeque et incommode navigassemus*—and another in Livy, which I am obliged to quote at second hand—*hinc atque hinc ad ventos obliqua transferre vela*—suggest, if they do not prove, that they could. I hope to discuss the question soon in another place. Meanwhile it is enough to say that if Caesar's cavalry transports could not work to windward, they could neither 'head-reach' nor lie to; and those which were carried back to their port of departure must have fetched it by sailing with the wind nearly abeam. But those which were 'driven down in great peril to the lower and more westerly part of the island' (*ad inferiorem partem insulae quae est propius solis occasum magno suo cum periculo deicerentur*) were evidently scudding; and while the word *referrentur* ('were carried back'), which is strongly contrasted with *deicerentur*, suggests that the others were laid to, Caesar's words show that they were comparatively in little danger, though, if they had not been brought close to the wind, they would evidently have run as great a risk as the others. A distinguished admiral suggests to me that they did sail with the wind nearly abeam, and disposes of the question of danger by assuming that Caesar exaggerated its force. But, with due deference, I would ask whether, since it was strong enough to drive ashore and wreck ships which were riding at anchor, and since the ships that ran before it were 'in great peril', ships sailing with it nearly abeam would not have been in peril equally great. It is universally admitted that ancient ships could sail with the wind abeam: it seems hardly credible that the discovery should not have followed that Gallic ships, which were not shallow, *could* also make headway within less than eight points of the wind; and I therefore believe that, however rare, from one cause or another, tacking may have been, ships were occasionally laid to in bad weather.

Anyhow, my argument remains unshaken. If the ships which were carried back to the *ulterior portus* neither lay to nor 'head-reached', it is self-evident that they could not have fetched Sangatte, and therefore that the *ulterior portus* was Ambleteuse: if they sailed with the wind abeam, they could not have fetched any port east of Wissant: if they were laid to, they could not have fetched Sangatte; nor could they have done so even if they 'head-reached' unless they possessed a capacity for working to windward with which few modern ships and no ancient ship can be credited. Indeed they could not have done so even then; for it would evidently have been impossible for them to beat across the Channel in a single tide: the flood (see p. 583) would hardly have helped them; and the ebb, prolonged and strengthened by the north-easterly gale, would have carried them far out of their course.

The foregoing remarks apply of course to other passages in which I have mentioned the cavalry transports. By no means could they have returned either from near Hythe or Lympne to Ambleteuse or from near Pevensey to the mouth of the Authie.

PAGE 680. 'Therefore, unless . . Bekesbourne.' Mr. George Barrow of the Geological Survey, whom I have consulted, thinks that there is no reason to suppose that the relative level of water and banks in the Little Stour above Littlebourne was appreciably different in 54 B.C. from what it is now.

PAGE 698. 'The claim . . . better grounds.' Mr. F. H. Baring (*Eng. Hist. Rev.*, October, 1907, pp. 726–8) argues that Caesar crossed the Thames by 'an undoubted ford just above tidewater at Hampton', which was mentioned by Lord Lumley in 1685. He argues that, owing to the tides, Brentford would only have been available for 'five or six hours out of twelve'; that 'this important fact would surely have been mentioned'; and that the stakes which have been found at Brentford are fifteen inches round and therefore too large. I am not sure that he is referring to the stakes described by Mr. Montagu Sharpe, whom he does not mention; but surely the stakes would not have answered their purpose worse for being strong. I do not believe that Caesar would have mentioned the presence of tides at Brentford if they had not affected his passage; but, as we have seen (p. 696), Mr. Spurrell has argued that the tides did not reach Brentford. If there was a ford at Hampton before the days of locks and weirs, there was another, if the evidence of the name *Hallifort* is trustworthy, at or near Coway Stakes. Are we entitled to assume that either existed in the time of Caesar? The reader will have understood that I do not pin my faith to Mr. Sharpe's theory: I only think that it is less feebly supported than any other.

PAGE 712, note 2. W. Sternkopf (*Hermes*, xl, 1905, p. 37) rejects Boot's emendation, *proximis*, and conjectures that Cicero wrote *proxime*, meaning that the letter in question was the most recent of several which had arrived together.

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